BRITISH ASSOCIATION
FOR THE ADVANCEMENT OF SCIENCE

REPORT

OF THE

EIGHTY-NINTH MEETING

EDINBURGH—1921
SEPTEMBER 7–14

LONDON
JOHN MURRAY, ALBEMARLE STREET

OFFICE OF THE ASSOCIATION
BURLINGTON HOUSE, LONDON, W. 1

1922

CONTENTS.

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TABLE OF

Date of Meeting	Where held	Presidents	Old Life Members	New Life Members
1831, Sept. 27	York	Viscount Milton, D.C.L., F.R.S.	—	—
1832, June 19	Oxford	The Rev. W. Buckland, F.R.S.	—	—
1833, June 25	Cambridge	The Rev. A. Sedgwick, F.R.S.	—	—
1834, Sept. 8	Edinburgh	Sir T. M. Brisbane, D.C.L., F.R.S.	—	—
1835, Aug. 10	Dublin	The Rev. Provost Lloyd, LL.D., F.R.S.	—	—
1836, Aug. 22	Bristol	The Marquis of Lansdowne, F.R.S.	—	—
1837, Sept. 11	Liverpool	The Earl of Burlington, F.R.S.	—	—
1838, Aug. 10	Newcastle-on-Tyne	The Duke of Northumberland, F.R.S.	—	—
1839, Aug. 26	Birmingham	The Rev. W. Vernon Harcourt, F.R.S.	—	—
1840, Sept. 17	Glasgow	The Marquis of Breadalbane, F.R.S.	—	—
1841, July 20	Plymouth	The Rev. W. Whewell, F.R.S.	169	65
1842, June 23	Manchester	The Lord Francis Egerton, F.G.S.	303	169
1843, Aug. 17	Cork	The Earl of Rosse, F.R.S.	109	28
1844, Sept. 26	York	The Rev. G. Peacock, D.D., F.R.S.	226	150
1845, June 19	Cambridge	Sir John F. W. Herschel, Bart., F.R.S.	313	36
1846, Sept. 10	Southampton	Sir Roderick I. Murchison, Bart., F.R.S.	241	10
1847, June 23	Oxford	Sir Robert H. Inglis, Bart., F.R.S.	314	18
1848, Aug. 9	Swansea	The Marquis of Northampton, Pres. R.S.	149	3
1849, Sept. 12	Birmingham	The Rev. T. R. Robinson, D.D., F.R.S.	227	12
1850, July 21	Edinburgh	Sir David Brewster, K.H., F.R.S.	235	9
1851, July 2	Ipswich	G. B. Airy, Astronomer Royal, F.R.S.	172	8
1852, Sept. 1	Belfast	Lieut.-General Sabine, F.R.S.	164	10
1853, Sept. 3	Hull	William Hopkins, F.R.S.	141	13
1854, Sept. 20	Liverpool	The Earl of Harrowby, F.R.S.	238	23
1855, Sept. 12	Glasgow	The Duke of Argyll, F.R.S.	194	33
1856, Aug. 6	Cheltenham	Prof. C. G. B. Daubeny, M.D., F.R.S.	182	14
1857, Aug. 26	Dublin	The Rev. H. Lloyd, D.D., F.R.S.	236	15
1858, Sept. 22	Leeds	Richard Owen, M.D., D.C.L., F.R.S.	222	42
1859, Sept. 14	Aberdeen	H.R.H. The Prince Consort	184	27
1860, June 27	Oxford	The Lord Wrottesley, M.A., F.R.S.	286	21
1861, Sept. 4	Manchester	William Fairbairn, LL.D., F.R.S.	321	113
1862, Oct. 1	Cambridge	The Rev. Professor Willis, M.A., F.R.S.	239	15
1863, Aug. 26	Newcastle-on-Tyne	Sir William G. Armstrong, C.B., F.R.S.	203	36
1864, Sept. 13	Bath	Sir Charles Lyell, Bart., M.A., F.R.S.	287	40
1865, Sept. 6	Birmingham	Prof. J. Phillips, M.A., LL.D., F.R.S.	292	44
1866, Aug. 22	Nottingham	William R. Grove, Q.C., F.R.S.	207	31
1867, Sept. 4	Dundee	The Duke of Buccleuch, K.C.B., F.R.S.	167	25
1868, Aug. 19	Norwich	Dr. Joseph D. Hooker, F.R.S.	196	18
1869, Aug. 18	Exeter	Prof. G. G. Stokes, D.C.L., F.R.S.	204	21
1870, Sept. 14	Liverpool	Prof. T. H. Huxley, LL.D., F.R.S.	314	39
1871, Aug. 2	Edinburgh	Prof. Sir W. Thomson, LL.D., F.R.S.	246	28
1872, Aug. 14	Brighton	Dr. W. B. Carpenter, F.R.S.	245	36
1873, Sept. 17	Bradford	Prof. A. W. Williamson, F.R.S.	212	27
1874, Aug. 19	Belfast	Prof. J. Tyndall, LL.D., F.R.S.	162	13
1875, Aug. 25	Bristol	Sir John Hawkshaw, F.R.S.	239	36
1876, Sept. 6	Glasgow	Prof. T. Andrews, M.D., F.R.S.	221	35
1877, Aug. 15	Plymouth	Prof. A. Thomson, M.D., F.R.S.	173	19
1878, Aug. 14	Dublin	W. Spottiswoode, M.A., F.R.S.	201	18
1879, Aug. 20	Sheffield	Prof. G. J. Allman, M.D., F.R.S.	184	16
1880, Aug. 25	Swansea	A. C. Ramsay, LL.D., F.R.S.	144	11
1881, Aug. 31	York	Sir John Lubbock, Bart., F.R.S.	272	28
1882, Aug. 23	Southampton	Dr. C. W. Siemens, F.R.S.	178	17
1883, Sept. 19	Southport	Prof. A. Cayley, D.C.L., F.R.S.	203	60
1884, Aug. 27	Montreal	Prof. Lord Rayleigh, F.R.S.	235	20
1885, Sept. 9	Aberdeen	Sir Lyon Playfair, K.C.B., F.R.S.	225	18
1886, Sept. 1	Birmingham	Sir J. W. Dawson, C.M.G., F.R.S.	314	25
1887, Aug. 31	Manchester	Sir H. E. Roscoe, D.C.L., F.R.S.	428	86
1888, Sept. 5	Bath	Sir F. J. Bramwell, F.R.S.	266	36
1889, Sept. 11	Newcastle-on-Tyne	Prof. W. H. Flower, C.B., F.R.S.	277	20
1890, Sept. 3	Leeds	Sir F. A. Abel, C.B., F.R.S.	259	21
1891, Aug. 19	Cardiff	Dr. W. Huggins, F.R.S.	189	24
1892, Aug. 3	Edinburgh	Sir A. Geikie, LL.D., F.R.S.	280	14
1893, Sept. 13	Nottingham	Prof. J. S. Burdon Sanderson, F.R.S.	201	17
1894, Aug. 8	Oxford	The Marquis of Salisbury, K.G., F.R.S.	327	21
1895, Sept. 11	Ipswich	Sir Douglas Galton, K.C.B., F.R.S.	214	13
1896, Sept. 16	Liverpool	Sir Joseph Lister, Bart., Pres. R.S.	330	31
1897, Aug. 18	Toronto	Sir John Evans, K.C.B., F.R.S.	120	8
1898, Sept. 7	Bristol	Sir W. Crookes, F.R.S.	281	19
1899, Sept. 13	Dover	Sir Michael Foster, K.C.B., Sec.R.S.	296	20

* Ladies were not admitted by purchased tickets until 1843. † Tickets of Admission to Sections only.

[*Continued on p.* xii.

ANNUAL MEETINGS.

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
—	—	—	—	—	353	—	—	1831
—	—	—	—	—	—	—	—	1832
—	—	—	—	—	900	—	—	1833
—	—	—	—	—	1298	—	£20 0 0	1834
—	—	—	—	—	—	—	167 0 0	1835
—	—	—	—	—	1350	—	435 0 0	1836
—	—	—	—	—	1840	—	922 12 6	1837
—	—	—	1100*	—	2400	—	932 2 2	1838
—	—	—	—	34	1438	—	1595 11 0	1839
—	—	—	—	40	1353	—	1546 16 4	1840
46	317	—	60*	—	891	—	1235 10 11	1841
75	376	33†	331*	28	1315	—	1449 17 8	1842
71	185	—	160	—	—	—	1565 10 2	1843
45	190	9†	260	—	—	—	981 12 8	1844
94	22	407	172	35	1079	—	831 9 9	1845
65	39	270	196	36	857	—	685 16 0	1846
197	40	495	203	53	1320	—	208 5 4	1847
54	25	376	197	15	819	£707 0 0	275 1 8	1848
93	33	447	237	22	1071	963 0 0	159 19 6	1849
128	42	510	273	44	1241	1085 0 0	345 18 0	1850
61	47	244	141	37	710	620 0 0	391 9 7	1851
63	60	510	292	9	1108	1085 0 0	304 6 7	1852
56	57	367	236	6	876	903 0 0	205 0 0	1853
121	121	765	524	10	1802	1882 0 0	380 19 7	1854
142	101	1094	543	26	2133	2311 0 0	480 16 4	1855
104	48	412	346	9	1115	1098 0 0	734 13 9	1856
156	120	900	569	26	2022	2015 0 0	507 15 4	1857
111	91	710	509	13	1698	1931 0 0	618 18 2	1858
125	179	1206	821	22	2564	2782 0 0	684 11 1	1859
177	59	636	463	47	1689	1604 0 0	766 19 6	1860
184	125	1589	791	15	3138	3944 0 0	1111 5 10	1861
150	57	433	242	25	1161	1089 0 0	1293 16 6	1862
154	209	1704	1004	25	3335	3640 0 0	1608 3 10	1863
182	103	1119	1058	13	2802	2965 0 0	1289 15 8	1864
215	149	766	508	23	1997	2227 0 0	1591 7 10	1865
218	105	960	771	11	2303	2469 0 0	1750 13 4	1866
193	118	1163	771	7	2444	2613 0 0	1739 4 0	1867
226	117	720	682	45‡	2004	2042 0 0	1940 0 0	1868
229	107	678	600	17	1856	1931 0 0	1622 0 0	1869
303	195	1103	910	14	2878	3096 0 0	1572 0 0	1870
311	127	976	754	21	2463	2575 0 0	1472 2 6	1871
280	80	937	912	43	2533	2649 0 0	1285 0 0	1872
237	99	796	601	11	1983	2120 0 0	1685 0 0	1873
232	85	817	630	12	1951	1979 0 0	1151 16 0	1874
307	93	884	672	17	2248	2397 0 0	960 0 0	1875
331	185	1265	712	25	2774	3023 0 0	1092 4 2	1876
238	59	446	283	11	1229	1268 0 0	1128 9 7	1877
290	93	1285	674	17	2578	2615 0 0	725 16 6	1878
239	74	529	349	13	1404	1425 0 0	1080 11 11	1879
171	41	389	147	12	915	899 0 0	731 7 7	1880
313	176	1230	514	24	2557	2689 0 0	476 8 1	1881
253	79	516	189	21	1253	1286 0 0	1126 1 11	1882
330	323	952	841	5	2714	3369 0 0	1083 3 3	1883
317	219	826	74	26 & 60 H.§	1777	1855 0 0	1173 4 0	1884
332	122	1053	447	6	2203	2256 0 0	1385 0 0	1885
428	179	1067	429	11	2453	2532 0 0	995 0 6	1886
510	244	1985	493	92	3838	4336 0 0	1186 18 0	1887
399	100	639	509	12	1984	2107 0 0	1511 0 5	1888
412	113	1024	579	21	2437	2441 0 0	1417 0 11	1889
368	92	680	334	12	1775	1776 0 0	789 16 8	1890
341	152	672	107	35	1497	1664 0 0	1029 10 0	1891
413	141	733	439	50	2070	2007 0 0	864 10 0	1892
328	57	773	268	17	1661	1653 0 0	907 15 6	1893
435	69	941	451	77	2321	2175 0 0	583 15 6	1894
290	31	493	261	22	1324	1236 0 0	977 15 5	1895
383	139	1384	873	41	3181	3228 0 0	1104 6 1	1896
286	125	682	100	41	1362	1398 0 0	1059 10 8	1897
327	96	1051	639	33	2446	2399 0 0	1212 0 0	1898
324	68	548	120	27	1403	1328 0 0	1430 14 2	1899

‡ Including Ladies. § Fellows of the American Association were admitted as Hon. Members for this Meeting.

[*Continued on p.* xiii.

Table of

Date of Meeting	Where held	Presidents	Old Life Members	New Life Members
1900, Sept. 5	Bradford	Sir William Turner, D.C.L., F.R.S. ...	267	13
1901, Sept. 11	Glasgow..................	Prof. A. W. Rücker, D.Sc., Sec.R.S. ...	310	37
1902, Sept. 10	Belfast	Prof. J. Dewar, LL.D., F.R.S.	243	21
1903, Sept. 9	Southport	Sir Norman Lockyer, K.C.B., F.R.S.	250	21
1904, Aug. 17......	Cambridge...............	Rt. Hon. A. J. Balfour, M.P., F.R.S.	419	32
1905, Aug. 15......	South Africa	Prof. G. H. Darwin, LL.D., F.R.S. ...	115	40
1906, Aug. 1	York	Prof. E. Ray Lankester, LL.D., F.R.S.	322	10
1907, July 31	Leicester	Sir David Gill, K.C.B., F.R.S.	276	19
1908, Sept. 2	Dublin	Dr. Francis Darwin, F.R.S.	294	24
1909, Aug. 25......	Winnipeg	Prof. Sir J. J. Thomson, F.R.S.	117	13
1910, Aug. 31	Sheffield..................	Rev. Prof. T. G. Bonney, F.R.S.	293	26
1911, Aug. 30	Portsmouth	Prof. Sir W. Ramsay, K.C.B., F.R.S.	284	21
1912, Sept. 4	Dundee	Prof. E. A. Schäfer, F.R.S...............	288	14
1913, Sept. 10	Birmingham	Sir Oliver J. Lodge, F.R.S...............	376	40
1914, July-Sept....	Australia	Prof. W. Bateson, F.R.S.	172	13
1915, Sept. 7	Manchester	Prof. A. Schuster, F.R.S.	242	19
1916, Sept. 5	Newcastle-on-Tyne...	Sir Arthur Evans, F.R.S.	164	12
1917	(No Meeting)		—	—
1918	(No Meeting)		—	—
1919, Sept. 9	Bournemouth	Hon. Sir C. Parsons, K.C.B., F.R.S....	235	47
1920, Aug. 24......	Cardiff	Prof. W. A. Herdman, C.B.E., F.R.S.	288	11
1921, Sept. 7	Edinburgh	Sir T. E. Thorpe, C.B., F.R.S.	336	9

¶ Including 848 Members of the South African Association.

‡‡ Grants from the Caird Fund are not included in this and subsequent sums.

Annual Meetings—(continued).

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
297	45	801	482	9	1915	£1801 0	£1072 10 0	1900
374	131	794	246	20	1912	2046 0	920 9 11	1901
314	86	647	305	6	1620	1644 0	947 0 0	1902
319	90	688	365	21	1754	1762 0	845 13 2	1903
449	113	1338	317	121	2789	2650 0	887 18 11	1904
937¶	411	430	181	16	2130	2422 0	928 2 2	1905
356	93	817	352	22	1972	1811 0	882 0 9	1906
339	61	659	251	42	1647	1561 0	757 12 10	1907
465	112	1166	222	14	2297	2317 0	1157 18 8	1908
290**	162	789	90	7	1468	1623 0	1014 9 9	1909
379	57	563	123	8	1449	1439 0	963 17 0	1910
349	61	414	81	31	1241	1176 0	922 0 0	1911
368	95	1292	359	88	2504	2349 0	845 7 6	1912
480	149	1287	291	20	2643	2756 0	978 17 1‡‡	1913
139	4160‖	539‖	—	21	5044‖	4873 0	1086 16 4	1914
287	116	628*	141	8	1441	1406 0	1159 2 8	1915
250	76	251*	73	—	826	821 0	715 18 10	1916
—	—	—	—	—	—	—	427 17 2	1917
—	—	—	—	—	—	—	220 13 3	1918
254	102	688*	153	3	1482	1736 0	160 0 0	1919
Old Annual Regular Members	Annual Members: Meeting and Report	Annual Members: Meeting only	Transferable Tickets	Students' Tickets				
136	192	571	42	120	20	1380	1272 10	959 13 9	1920
133	410	1394	121	343	22	2768	2599 15	418 1 10	1921

** Including 137 Members of the American Association.

‖ Special arrangements were made for Members and Associates joining locally in Australia, see Report, 1914, p. 686. The numbers include 80 Members who joined in order to attend the Meeting of L'Association Française at Le Havre.

* Including Students' Tickets, 10*s*.

REPORT OF THE COUNCIL, 1920-21.

I. Professor Charles Scott Sherrington, Pres. R.S., has been unanimously nominated by the Council to fill the office of President of the Association for the year 1922-23 (Hull Meeting).

II. A resolution from Section D, supported by other Sections, urging the need for a national expedition for the further exploration of the sea, was referred by the General Committee at the Cardiff Meeting to the Council for consideration, and, if desirable, for action. A memorandum, printed as an appendix to this report, was drawn up by a Committee which (as stated therein) was appointed by the Council for the purpose.

The Council, however, at its meeting on March 4, 1921, adopted a report from the General Officers recommending that no further action should be taken for the present, in view of the need for economy in national expenditure, in regard to the presentation of the above scheme to H.M. Government. The scheme, however, is retained under consideration, and the Council hopes that the expedition is only postponed for a season, and that the interval may be usefully employed in perfecting plans and making other essential preparations.

Meanwhile, the memorandum has been communicated to the Cabinet Secretariat of H.M. Government, the Admiralty, and the Department of Scientific and Industrial Research.

III. Other resolutions referred by the General Committee, at the Cardiff Meeting, to the Council for consideration and if desirable for action were dealt with as follows:—

(*a*) An application to H.M. Stationery Office to print tables on Congruence Solutions, prepared by Lieut.-Col. A. Cunningham and Mr. T. G. Creak, was forwarded to the Department of Scientific and Industrial Research, which replied that the work could not be undertaken at present owing to the high cost of printing, but suggested that the application should be renewed later. (Resolution of Section A.)

(*b*) The need for a central British institute for training and research in surveying, hydrography, and geodesy has been brought to the notice of the Royal Commissions on the Universities of Oxford and Cambridge. (Resolutions of Sections A and E.)

(*c*) A resolution on the desirability of continuing experiments on industrial alcohol in Government establishments was forwarded to the Director of Fuel Research. (Resolution of Section B.)

(*d*) The Council took note that the forecasting of the length of Research Committee reports was regarded as impossible in many cases by the Committee of Section C. Forecasts have not been requested this year, and alternative measures will be adopted in future.

(*e*) Following on a resolution by Section D, supported by other sections, the Council communicated the following resolution to the First Lord of the Treasury:—

That the Council considers that no scheme of payment of professional scientific men in the service of the State is satisfactory which places them on a lower level than that of the higher grade of the Civil Service.

(*f*) Resolutions from Sections E and H in favour of the collection of rural lore through the agency of schools and colleges were approved and forwarded to the President of the Board of Education and to the Scottish Education Department.

(*g*) A resolution from Section E, on geographical education in advanced courses, was approved and forwarded to the President of the Board of Education.

(*h*) The Council forwarded to the Royal Society a resolution from Section E, asking that the representative of the Association on the National Committee on Geographical Research should be one who might hold office for a longer term than the President of Section E during his year of office (as proposed by the Society). Having ascertained the concurrence of the Society with this view, the Council appointed Professor J. L. Myres to represent the Association.

(*i*) The Council communicated with the Government of the Union of South Africa as to the desirability of instituting an ethnological bureau (Resolution of Section H), and were informed that a school of Bantu studies is being established in connection with the University of Cape Town.

(*j*) A resolution of Section H on the desirability of instituting an anthropological survey of aborigines in Western Australia was approved and forwarded to the Government of that State. A resolution on the protection of aborigines in central Australia was approved and forwarded to the High Commissioner for Australia, with an expression of satisfaction at the measures to that end which, as the Council learned, were already in hand.

(*k*) The Council resolved to give effect to a resolution (from Section H) that associations for the advancement of science in the Dominions and foreign countries should be asked to send official representatives to attend annual meetings of the British Association, and the Australasian, South African, American, French, Italian, and Spanish Associations for the Advancement of Science were accordingly invited to send delegates to the Edinburgh meeting.

(*l*) A resolution from Sections H and L, urging the extension of anthropometric observations as part of the medical inspection in schools, was approved and forwarded to the President of the Board of Education and the Minister for Health. The former, however, pointed out that it was not possible to impose further duties upon local educational bodies in regard to medical inspection.

(*m*) The Council approved the formation of Section J, Psychology.

(*n*) A resolution from Section K, urging Government support for afforestation experiments on pit-mounds by the Midlands Afforestation Committee, was approved and forwarded to the Minister for Agriculture and to the Forestry Commission.

(*o*) The Council were unable to approve a proposal (from Section L) that the Organising Committee of that Section should allow a book on Citizenship to be published with its approval, but informed the Committee of its power to bring such a book to the notice of the Council itself.

(*p*) The Council took no action upon proposals received from the Conference of Delegates: (i) That a meeting of delegates should be held in London; (ii) That the Council should urge the reappointment of a Royal Commission on Railways. As regards (ii) the Council felt that such a proposal lies outside the scope of the Association.

IV. The Council have had under careful consideration various suggestions which have been made, in correspondence in *Nature* and elsewhere, in regard to the organisation of Sections, the improvement of annual meetings, etc. The Council appointed a Committee 'to consider and report upon the redistribution of Sections and on other matters in connection with the proceedings of the Annual Meeting,' and this Committee has presented a valuable and suggestive report. The Council also caused all the Organising Sectional Committees to be summoned to meet on one day (February 25, 1921) at Burlington House, accommodation being provided for them through the kind collaboration of the Chemical Society, the Society of Antiquaries, and the Linnean Society. The Committees met jointly, as well as separately,[1] and the opportunity thus afforded for interchange of views was greatly appreciated, and resulted in many valuable suggestions, especially in the direction of formulating subjects for joint discussion at the Edinburgh Meeting.

In the outcome, the Council have made the following arrangements, which they hope will add to the success of the forthcoming and future Annual Meetings:—

(*a*) Out of the subjects proposed for discussion at joint sectional meetings the Council have selected six, for which they have empowered the General Officers to fix times in the programme of the meeting, with a view to their arrangement as special features.

(*b*) Sectional Presidents have been given the opportunity either of reading their addresses, as hitherto, or of speaking and introducing discussion upon their subjects (without formal reading). The Council have also empowered the General Officers to fix the times of presidential addresses in the programme, in order to avoid the clashing of subjects of kindred interest.

(*c*) The Council have endorsed the opinion of the meeting on February 25 that any grouping of the Sections should be voluntary and temporary, not binding and permanent. They propose, however, that the General Committee should meet in special session at the Edinburgh Meeting to consider the question of a reduction in the number of Sections.

(*d*) The Council have empowered Sectional Committees to submit each year a short list of names suitable for Presidents of their respective Sections at the next meeting.

[1] These meetings rendered unnecessary a meeting of Recorders of Sections similar to that which was so successfully held in 1920, and is referred to in *Report of Council*, 1919-20, § xv.

V. Conference of Delegates and Corresponding Societies Committee.—The following nominations are made by the Council: *Conference of Delegates*: Sir R. A. Gregory (*President*), Mr. W. Mark Webb (*Vice-President and Secretary*), Mr. T. C. Day (*Local Secretary for the Edinburgh Meeting*). *Corresponding Societies Committee*: Mr. W. Whitaker (*Chairman*), Mr. W. Mark Webb (*Secretary*), Mr. P. J. Ashton, Dr. F. A. Bather, Rev. J. O. Bevan, Sir Edward Brabrook, Sir H. G. Fordham, Sir R. A. Gregory, Mr. T. Sheppard, Rev. T. R. R. Stebbing, Mr. Mark L. Sykes, and the President and General Officers of the Association.

VI. Under the powers delegated to them by the General Committee, the Council appointed Dr. E. H. Griffiths General Treasurer of the Association for the year 1920-21. His accounts have been audited and are presented to the General Committee.

VII. The Council empowered the General Officers to grant the sum of 250*l*. annually for five years out of the gift of 1,000*l*. (with accumulated interest) made by the late Sir James Caird for the study of radio-activity, subject to reports to the Council by the General Officers and the recipients. The grant for the year 1921-22 has been made to Sir E. Rutherford.

VIII. The retiring Ordinary Members of the Council are:—

By seniority: Dr. F. A. Dixey, Sir F. W. Dyson, Miss E. R. Saunders.

By least attendance: Sir D. Morris.

Dr. E. H. Griffiths, on his appointment as General Treasurer, ceased to be an Ordinary Member of the Council.

The Council nominated the following new members:—

Dr. F. W. Aston, Prof. H. J. Fleure, Prof. A. C. Seward, leaving two vacancies to be filled by the General Committee without nomination by the Council.

The full list of nominations of Ordinary Members is as follows:—

Dr. E. F. Armstrong.
Dr. F. W. Aston.
Mr. J. Barcroft.
Professor W. A. Bone.
Professor H. J. Fleure.
Professor A. Fowler.
Professor J. Stanley Gardiner.
Sir R. A. Gregory.
Sir R. Hadfield.
Sir Daniel Hall.
Sir S. F. Harmer.
Mr. J. H. Jeans.
Professor A. Keith.
Sir J. Scott Keltie.
Professor A. W. Kirkaldy.
Dr. P. Chalmers Mitchell.
Sir W. J. Pope.
Dr. W. H. R. Rivers.
Professor W. R. Scott.
Professor A. C. Seward.
Sir Aubrey Strahan.
Mr. W. Whitaker.
Dr. A. Smith Woodward.

IX The General Officers have been nominated by the Council as follows:—

General Treasurer, Dr. E. H. Griffiths.
General Secretaries, Prof. H. H. Turner and Prof. J. L. Myres.

X. Dr. E. H. Griffiths and Prof. J. L. Myres have been appointed representatives of the Association on the Conjoint Board of Scientific Societies.

XI. The following have been admitted as members of the General Committee:—

Canon J. A. MacCulloch, Dr. J. R. Milne, Prof. T. P. Nunn, Dr. J. Reilly, Mr. W. Alfred Richardson, Sir R. Robertson, Prof. W. H. Watkinson.

XII. The Council have been informed that invitations for future Annual Meetings will be presented in due course to the General Committee as follows: Liverpool, 1923; Toronto, 1924.

XIII. The following changes in the Rules are proposed, namely:—

(*a*) Rule VII., 4, 'the balance standing . . . to the credit of the Association in the books of *the Bank of England*,' to read '. . . *the Association's bankers as authorised by the Council.*'

(*b*) To add in Rule X., after clause (iii.):

(iv.) University students, not resident or working in the locality where the Annual Meeting takes place, may, on the recommendation of any recognised University or College, obtain, on one occasion only, students' tickets for the meeting on payment of 10*s*. Holders of such tickets shall not be entitled to any privilege beyond attendance at the Annual Meeting, but shall not be debarred from admission under clause (iii.).

APPENDIX TO REPORT OF COUNCIL.

MEMORANDUM ON PROPOSED NATIONAL EXPEDITION FOR THE EXPLORATION OF THE SEA.

I.

Origin of Proposal.

At the Annual Meeting of the British Association for the Advancement of Science in August 1920 the President, Dr. W. A. Herdman, F.R.S., Professor of Oceanography in the University of Liverpool, delivered an address dealing with some of the problems of oceanography, and suggested that the time had come for a new British expedition to explore the great oceans of the globe. This suggestion was afterwards put forward more definitely and with further detail in the discussion 'On the Need for the Scientific Investigation of the Ocean' at a joint meeting of the Sections of Zoology and Geography. The proposal then made was, in brief, that there was now urgent need for another great exploring expedition like that of the *Challenger* (1872-76), national in character, world-wide in scope, to investigate further the science of the sea, in all departments, by modern methods under the best expert advice and control.

Action by Committees and Council of the Association.

This proposal was received with such favour that at the next meeting of the Committee of Section D (Zoology) a resolution was unanimously passed:—

> That Section D is profoundly impressed with the importance of urging the initiation of a further National Expedition for the Exploration of the Ocean, and requests the Council of the British Association to appoint a Committee to take the necessary steps to impress this need upon His Majesty's Government and the nation.

This resolution was supported by the Committees of all the other Sections of the Association interested in such an exploration. The Committee of Recommendations and the General Committee on the following day passed a resolution 'pointing out the importance of urging the initiation of a national expedition for the exploration of the ocean, and requesting that the Council of the British Association should take the necessary steps to impress this need upon His Majesty's Government and the nation.' The Council of the Association thereupon appointed a Committee, representative of all the departments of science concerned, to prepare and take steps for the presentation of the present statement; while, following upon a reference from the Association, the Council of the Royal Society also appointed a Committee to confer with that appointed by the Council of the Association.

Many men of science, both British and foreign, wrote expressing the hope that the cogent scientific reasons for the expedition may be pressed without delay upon the Government so as to induce the nation to undertake this great enterprise.

II.

'Challenger' Expedition.

The *Challenger* expedition, the great British circumnavigating and deep-sea exploring expedition under Sir George Nares and Sir Wyville Thomson in 1872-76, brought back collections and results unrivalled either before or since, which added enormously to our scientific and practical knowledge of the oceans. Our knowledge of the science of the sea, however, has undergone great changes during the last half-century. Physics, Chemistry, Geology, Zoology, Botany, Physiology, and Geography all have problems awaiting solution,[1] and there are many modern methods of investigation of the ocean depths which have been devised or improved since the days of the *Challenger*. All civilised nations of the world have contributed by means of expeditions during the last quarter-century to the advance of oceanography, and it is remarkable that our country, considering the relations of our Empire to the oceans, has done comparatively little. In view of our maritime position, of the pre-eminence of our Navy, of our great mercantile marine, and of our sea-fisheries, Great Britain should undoubtedly lead the world in oceanographical research.

III.

Scope and Period of Proposed Expedition.

Such an expedition as is contemplated ought, in order to make worthy contributions to science, to be at least as extensive in duration and as comprehensive in scope as the *Challenger* Expedition. It ought to explore all the great oceans during a period of three or four years. It ought to be prepared to establish landing parties on oceanic islands, coral reefs, and other places where special detailed explorations on shore or in shallow water are required. Special scientific apparatus may have to be devised, and young scientific men may have to be trained to fit them for the work of such an expedition. At least one year, therefore, would have to be devoted to the work of preparation. It will be apparent from the Appendix to this statement that a number of the investigations proposed are of the highest direct practical importance, and there are many reasons why it is important that the scheme should be initiated and preparations organised with as little delay as possible.

Ship.

Preliminary inquiries lead tentatively to the belief that a vessel of the mercantile marine, of about 3,000 tons, chartered by H.M. Government for the occasion, would best suit the general purposes of the expedition; with the possible exception, as already indicated, of certain investigations which might be carried out independently of the main body.

Date of Departure.[2]

It has been suggested that the expedition should start in the summer of 1922, which date would, incidentally, permit of its conveying the astronomical observers of the eclipse of the sun visible on September 30 of that year in the Maldive Islands (Indian Ocean).

Scientific Personnel.

It is estimated that the scientific staff of such an expedition should consist of a director with ten or twelve assistants, exclusive of landing parties and any officers of the Royal Navy who might be detailed for special investigations for Admiralty purposes.

Cost.

While it is difficult under present conditions, and in the present preliminary stage of inquiry into the possibility and scope of the expedition, to form any near estimate of its cost, it is believed that (apart from the provision of the

[1] See Schedule appended for a Summary of the proposed investigations.

[2] This clause is now subject to the Council's decision stated in Report § II.

ship, which it is hoped would be undertaken by the Admiralty) this should lie between 200,000*l*. and 300,000*l*., with a bias toward the higher figure. It is to be observed that the expenditure would be spread over a number of years.

Publication of Results.

In this connection suitable arrangements for the adequate publication of the results of the expedition must be borne in mind. The working out and publication of the results of the *Challenger* Expedition are stated to have cost about as much as the expedition itself, and a similar expenditure may be anticipated in the present case.

Preservation of Specimens.

The natural repository of type specimens collected during the expedition would be the British Museum (Natural History Department), while duplicate specimens should be offered to museums, universities, etc., in various parts of the Empire.

The Committee trust at this stage to obtain such assurance from H.M. Government of their support as will justify them in reporting to their Council that the organisation of the expedition is to be proceeded with. Having regard to the world-wide and therefore Imperial character of the investigations proposed, to which they have already called attention, and to the valuable assistance which could be rendered locally to the expedition by the Governments of the Dominions whose territories border upon the great oceans, they venture to suggest to H.M. Government that the subject of the expedition is one which might properly be brought to the notice of the Imperial Conference.

SCHEDULE.

Subjects for Investigation.

To give some idea of the amount and variety of scientific work that might be undertaken by such an expedition, the following may be mentioned as some of the chief recommendations which have been received from representatives of the various Sections of the Association concerned :—

(1) In the departments of marine biology and physiology extensive investigations are required of fish and fisheries in the interest of food supplies. These include a very wide range of inquiry, which may be summarised thus : the effects of temperature and other conditions on the distribution and life of organisms ; the distribution of the plankton (which includes organisms of first-rate importance as food for fishes which supply food for man) ; ocean currents in relation to fisheries (just enough is known as to the influence of variations in the great oceanic currents upon the movements and abundance of migratory fishes to make evident the need for further and more complete investigation of the subject) ; the physiology of deep-sea and other oceanic animals ; the investigation of marine algæ, both coastal and planktonic ; marine bacteria ; bio-chemical investigation of the metabolism of the sea (this is perhaps the department of oceanography which deals with the most fundamental problems and which is most in need of immediate investigation) : the question of the abundance of tropical plankton as compared with that of temperate and polar seas, the distribution and action of denitrifying bacteria, the variations of the plankton in relation to environmental conditions, the factors which determine uniformity of conditions over a large sea area from the point of view of plankton distribution, the supply of the necessary minimal substances such as nitrogen, silica, and phosphorus to the living organisms, and the determination of the rate of production and rate of destruction of all organic substances in the sea—these are some of the fundamental problems of the metabolism of the ocean ;

all of them require investigation, and bear, directly or indirectly, upon the harvest of the sea for man's use, just as agricultural researches bear upon the harvest of the land.

(2) In the appropriate departments of chemistry observations are required on the temperature, salinity and chemistry of sea-water, the hydrogen-ion concentration, and the source and distribution of nitrogen in the sea.

(3) In the department of physics there is need for investigation of meteorological problems, the distribution of oceanic temperature, atmospheric electricity, long-distance transmission of electro-magnetic waves, and other problems of wireless telegraphy at sea. The study of the variation in the force of gravity over the great ocean basins is also suggested, and bears upon the problem of the figure of the earth, and the density of materials of which it is composed. It may be stated here that such an investigation might need to be carried out on a larger and steadier ship than that which would most probably be detailed for the expedition. On the other hand, there is no reason why the whole of the investigations associated with the expedition should be confined to a single vessel, for the opportunity might be made for collateral investigations on other vessels in the ordinary course of navigation. Similarly, the investigation of the phenomena of tides, one of the most urgent on the physical side, could most profitably be begun in shallow seas, and not on the vessel carrying the main expedition over the deep oceans.

(4) In the departments of geology and geography there are indicated as subjects for study both shallow and deep water deposits, and the various methods of deposition; sediments on the sea-bottom in relation to the movement (rising or sinking) of adjacent land areas (a matter which in turn bears upon the encroachments of the sea upon the land, or the reverse); borings on the floor of the sea for the extension of knowledge of the rocks composing the crust of the earth; the physical conditions of oceanic islands; the growth and other problems of coral reefs and islands.

(5) In the department of anthropology it is pointed out that the opportunity for landing parties on oceanic islands (especially in the Pacific) would give occasion for observations on the ethnography, habits, and life of native populations; any medical officer attached to such parties would find matter for study in the physical characters and diseases of natives.

It is not suggested that the foregoing summary by any means covers a complete list of the problems of the ocean requiring investigation, nor on the other hand that these need all be undertaken by one expedition; but they are sufficient to show that there is still much to be found out in all branches of oceanography, and that a further scientific exploration of the oceans will add to knowledge in many branches of science, and should also aid in the advancement of various industries based upon marine products of economic importance.

It may be desirable to refer to the relations between the work of such an expedition as is here proposed—work which, while temporary in character would be world-wide in scope—and that carried on under the International Council for the Study of the Sea in the North Atlantic and adjoining European seas. This latter work, while restricted in scope, is permanent, and the proposed oceanographic expedition covers a wider range in science, and would offer an unsurpassable opportunity of qualifying investigators to take part in future oceanographical and fisheries research under a permanent organisation.

GENERAL TREASURER'S ACCOUNT.

July 1, 1920, to June 30, 1921.

On the recommendation of the Hon. Auditors, the former simple statement of receipts and payments for the year is replaced on this occasion, and for the future, by a balance sheet showing liabilities and assets, an expenditure and income account, and a separate statement for the Caird Fund.

In the accounts of expenditure and income certain comparative figures for the year 1919–20 are furnished, and notes upon some of these are appended to the accounts. Owing to the alteration in the method of presenting the accounts, a complete comparison is, on this occasion, not possible.

The financial position of the Association cannot be regarded as satisfactory. It is natural that under present circumstances there should be a downward tendency in the receipts from membership subscriptions, which represent over two-thirds of the receipts from normal sources. On the other hand, all ordinary expenditure has increased since 1914 : salaries approximately by 28 per cent., printing by 120 per cent., and expenditure on postage, stationery, and travelling very largely. Economies have been effected wherever possible ; for example, there are now in the London office only two permanent paid officers, whereas in 1914 there were three and in 1910 and earlier four ; while printing, which is the heaviest single item of expenditure, has been reduced, it is believed, to the limit of efficiency, the cost last year being £1,475 10*s.* 11*d.*, as against £2,400, which would have been the approximate cost at present rates if printing had been maintained at pre-war standard. Notwithstanding these economies, the balance sheet reveals an excess of expenditure over income amounting to £80, and this deficit would have been much greater had we not received certain non-recurrent items of income.

It is impossible to avoid the conclusion that the deficit in the coming year will be largely increased, and the General Treasurer asks members to recognise the necessity of economy in such matters as the reduction in printing, grants for research, etc.

E. H. GRIFFITHS,
(*General Treasurer*).

Balance Sheet,

LIABILITIES.

		£ s. d.	£ s. d.
To Sundry Creditors			282 3 7
,, Capital Accounts—			
General Fund per contra . . .		10,575 15 2	
Caird Fund do.		9,582 16 3	
Sir F. Bramwell's Gift for Enquiry into Prime Movers, 1931— £50 Consols accumulated to June 30, 1921, per contra		49 15 0	
			20,208 6 5
,, Caird Fund Income and Expenditure Account Balance at July 1, 1920		582 3 0	
Add Income Tax Recovered year 1919		110 9 8	
		692 12 8	
Add Excess of Income over Expenditure for year to June 30, 1921 . .		275 19 0	
			968 11 8
,, Caird Gift—			
Radio-Activity Investigation, Balance at July 1, 1920.		1,208 8 11	
Add Interest on Deposit	35 0 5		
Dividends on Treasury Bonds . . .	14 19 4		
		49 19 9	
		1,258 8 8	
Less Grant to Sir E. Rutherford .		250 0 0	
			1,008 8 8
,, Income and Expenditure Account, Balance at July 1, 1920		3,367 14 11	
Add Income Tax Recovered year 1919		95 14 1	
		3,463 9 0	
Less Outstanding Account for year 1919 due to Printers—paid since . .		1,328 7 1	
		2,135 1 11	
Less Excess of Expenditure over Income for year to June 30, 1921 .		80 2 9	
			2,054 19 2
			£24,522 9 6

I have examined the foregoing Balance Sheet and accompanying Accounts with balances at the Bankers and the investments.

Approved,

ARTHUR L. BOWLEY,
Auditor.

July 29, 1921.

June 30, 1921.

ASSETS.

	£ s. d.	£ s. d.
By Sundry Debtors		259 2 5
,, Investments on Capital Accounts—		
£4,651 10*s.* 5*d.* Consolidated 2½ per cent. Stock at cost	3,942 3 3	
£3,600 India 3 per cent. Stock at cost .	3,522 2 6	
£879 14*s.* 9*d.* £43 Great Indian Peninsula "B" Annuity at cost	827 15 0	
£863 2*s.* 10*d.* War Stock, 1929–47, at cost .	889 17 6	
£1,400 War Bonds 5 per cent., 1929–47, at cost	1,393 16 11	
		10,575 15 2
Value at date, £6,794 8*s.* 4*d.*		
,, Caird Fund—		
£2,627 0*s.* 10*d.* India 3½ per cent. Stock at cost	2,400 13 3	
£2,100 London and North Western Rly. Consolidated 4 per cent. Preference at cost	2,190 4 3	
£2,500 Canada 3½ per cent., 1930–50, Registered Stock at cost	2,397 1 6	
£2,500 London & South Western Rly. Consolidated 4 per cent. Preference at cost .	2,594 17 3	
		9,582 16 3
Value at date, £5,889 18*s.* 2*d.*		
,, Sir F. Bramwell's Gift—		
2½ per cent, Self-Cumulating Consolidated Stock at Nominal Value	50 0 0	
Add accumulations to June 30, 1920 .	50 19 3	
Do. to June 30, 1921 . .	2 14 0	
	103 13 3	
Value at 48		49 15 0
,, Caird Gift—		
£1,000 Treasury Bonds		1,000 0 0
,, Investments out of Income—		
£900 Treasury Bonds		900 0 0
,, Cash—		
On Deposit	1,657 18 1	
At Bank	490 10 2	
In Hand	6 12 5	
		2,155 0 8
Viz.—		
Caird Fund 968 11 8		
Caird Gift 8 8 8		
General Purposes 1,178 0 4		
£2,155 0 8		
		£24,522 9 6

the books and vouchers and certify the same to be correct. I have also verified the

W. B. KEEN,
Chartered Accountant.

General Treasurer in Account

JULY 1, 1920, TO

EXPENDITURE.

	£ s. d.	£ s. d.	Corresponding Figures in 1919–20. £ s. d.
To Heating and Lighting	7 13 7		
,, Stationery	43 13 11		
,, Advertising	21 13 3		
,, Rent	8 2 6		
,, Electric Light Installation and Gas Fittings	96 9 6		
,, Postages	53 12 5		
,, Gift to Miss Stewardson	100 0 0		
,, Travelling Expenses	28 5 11		
,, General Expenses	171 5 2		
	530 16 3		*292 14 8*(1)
,, Salaries and Gratuity	972 10 0		*877 0 0*
,, Printing, Binding, etc.	1,475 10 11		*859 15 3*(2)
		2,978 17 2	
,, Grants to Research Committees :—			
Table of Constants	40 0 0		
Bronze Implements	100 0 0		
Colloid Chemistry	5 0 0		
Corresponding Societies	40 0 0		
Macedonia (excavations)	50 0 0		
Primary Survey of Wales	15 0 0		
Gravity at Sea	10 0 0		
Colour in Lepidoptera	24 0 0		
Inheritance in Silkworms	17 2 1		
Œnothera	17 15 0		
Training in Citizenship	15 0 0		
Colloid Chemistry	5 0 0		
Primary Survey of Wales	5 0 0		
International Languages	7 10 0		
Credit, Currency, and Finance	50 0 0		
Zoological Bibliography	0 4 9		
Educational Pictures	6 10 0		
Physiology of Heredity	10 0 0		
		418 1 10	*1,011 13 0*
,, Cardiff Meeting Expenses		121 3 4	*260 8 7*(3)
		£3,518 2 4	

(1) The higher figure for 1920–21 is to be accounted for mainly by non-recurrent charges (gift to Miss Stewardson, electric light installation, and certain advertising), amounting to £216. The remainder is largely accounted for by increased postal charges and travelling expenses.

(2) The sum of £859 15*s.* 3*d.* does not represent the whole cost of printing *incurred* in 1919–20, which was £2,188 2*s.* 4*d.* The sum of £1,475 10*s.* 11*d.*, on the other hand, represents the whole cost of printing incurred in 1920 21. The economies in printing put into force by the Council have, therefore, proved effective.

(3) The falling-off in life compositions is accounted for by the raising of the fee from £10 to £15 in 1919, and by the fact that a large number of members compounded immediately before the raising of the fee came into effect.

(4) The sums on account of Life Members' additional subscriptions represent the result of an appeal made by the late General Treasurer to old Life Members to add to their original compositions in order to help to meet the increased cost of printing, if they

Caird

EXPENDITURE.

		£ s. d.
1921 June 30.	To Grant to Seismological Committee	100 0 0
	,, Balance carried to Balance Sheet	275 19 0
		£375 19 0

with the British Association.

JUNE 30, 1921.

INCOME.

	£ s. d.	£ s. d.	Corresponding Figures in 1919–20. £ s. d.
By Life Compositions		150 0 0	*734 0 0*(3)
,, Annual Members' Subscriptions (Regular)		367 0 0	
,, ,, ,, (Temporary)		613 0 0	
,, ,, ,, with Report		371 10 0	*1,720 10 0*
,, Transferable Tickets		47 10 0	
,, Students' Tickets		60 0 0	
,, Life Members' Additional Subscriptions		282 4 0	*446 13 0*(4)
,, Refund of Travelling Expenses *re* Australian Meeting, 1914		75 0 0	
,, Donations		148 13 5	*2,489 6 6*(5)
,, Interest on Deposits		103 11 7	*53 6 8*
,, Sales of Publications		466 11 8	*224 11 10*(6)
,, Unexpended Balance of Grants returned		224 10 3	*51 19 3*(7)
,, Income Tax recovered		95 11 0	
,, Dividends :—			
Consols	81 8 0		
India 3 %	75 12 0		
Great Indian Peninsula Railway "B" Annuity	23 9 11		
War Stock	92 3 0		
Treasury Bonds	6 0 9		
		278 13 8	*272 10 2*
		3,283 15 7	
,, Legacies :—			
T. W. Backhouse	50 0 0		
Wm. Palmer	104 4 0		
		154 4 0	
,, Balance being Excess of Expenditure over Income for Year		80 2 9	
		£3,518 2 4	

wished to receive the annual report. This source of income must be regarded as non-recurrent.

(5) The greater part of the 'Bournemouth Fund,' initiated to meet the cost of researches maintained during and after the war, when the normal resources of the Association became severely limited, is represented by the figure of £2,489 6*s.* 6*d.* for 1919–20. Only small additions were received in 1920–21, and this source of income is to be regarded as non-recurrent, and has been devoted to the purpose for which it was subscribed.

(6) The increase in receipts from sale of publications in 1920–21 is largely accounted for by the publication of the Presidential Addresses as 'The Advancement of Science: 1920,' a measure first undertaken in that year.

(7) 'Unexpended balances' are from grants made in the *preceding* year.

(8) The reduction in the expenses of the Annual Meeting was effected mainly by ceasing to issue a daily journal independently of the Annual Report: the single journal now issued is subsequently incorporated in the Report.

Fund.

INCOME.

		£ s. d.	£ s. d.
1921 June 30.	By Dividends on Investments :—		
	India 3½ %	64 7 4	
	Canada 3½ % (including extra ½ %)	70 0 0	
	London and South-Western Railway Consolidated 4 % Pref. Stock	70 0 0	
	London and North-Western Railway Consolidated 4 % Pref. Stock	58 16 0	
			263 3 4
	,, Income Tax Recovered		112 15 8
			£375 19 0

Bank Agreement, June 30, 1921.

	£ s. d.	£ s. d.
Balance as per Pass Book		629 0 0
Less Cheques not presented :—		
Prof. Love	10 0 0	
,, Bateson	24 0 0	
,, Bateson	17 2 1	
Dr. Hoyle	1 5 0	
,, Thomas	5 0 0	
,, Foster Morley	7 10 0	
Prof. Scott	50 0 0	
,, Poulton	0 4 9	
,, Myres	16 18 0	
Sir R. Gregory	6 10 0	
		138 9 10
Balance as per Cash Book		£490 10 2

Dividends and Interest Received Year ended June 30, 1921.

£ s. d.			£ s. d.	£ s. d.
4,651 10 5	Consolidated 2½ per cent. Stock, ¼ Year	1920 July 5	20 7 0	
		Oct. 5	20 7 0	
		1921 Jan. 5	20 7 0	
		April 7	20 7 0	
				81 8 0
3,600 0 0	India 3 per cent. Stock	1920 July 5	18 18 0	
		Oct. 5	18 18 0	
		1921 Jan. 5	18 18 0	
		April 7	18 18 0	
				75 12 0
879 14 9	£43 Great Indian Peninsula 'B' Annuity	June 30	11 14 7	
		Dec. 31	11 15 4	
				23 9 11
810 10 3	War Stock 5 per cent. 1929–47	Dec. 1	20 5 3	
		1921 June 1	20 5 3	
				40 10 6
52 12 7	,, ,, Post Office Issue	1920 Dec. 1	1 6 3	
		1921 June 1	1 6 3	
				2 12 6
1,400 0 0	War Bonds, 5 per cent. 1929–47	1920 Dec. 1	24 10 0	
		1921 June 1	24 10 0	
				49 0 0
900 0 0	Treasury Bonds			6 0 9
				£278 13 8

Caird Fund.

£	s.	d.			£	s.	d.	£	s.	d.
2,500	0	0	Canada 3½ per cent. 1930–50 Registered	June 30	35	0	0			
				Dec. 31	35	0	0			
								70	0	0
2,627	0	10	India 3½ per cent. Stock	July 5	16	1	10			
				Oct. 5	16	1	10			
				1921						
				Jan. 6	16	1	10			
				April 5	16	1	10			
								64	7	4
2,500	0	0	London and South-Western Railway Consolidated 4 per cent. Preference	June 30	35	0	0			
				Dec. 31	35	0	0			
								70	0	0
2,100	0	0	London and North-Western Railway Consolidated 4 per cent. Preference	June 30	29	8	0			
				Dec. 31	29	8	0			
								58	16	0
								£263	3	4

Caird Gift.

1,000	0	0	Treasury Bonds	£14	19	4

General Account	278	13	8
Caird Fund	263	3	4
Caird Gift	14	19	4
	£556	16	4

Investment Values, June 30, 1921.

£	s.	d.		Market Price.	£	s.	d.	Corresponding Values at June 30, 1920. £	s.	d.
103 4,651	13 10	3 5	Consols, 2½ per cent.	48	2,284	16	0	*2,233*	*13*	*4*
3,600	0	0	India 3 per cent.	50	1,800	0	0	*1,728*	*0*	*0*
879	14	9	£43 Great Indian Peninsula Railway 'B' Annuity	14½	623	10	0	*580*	*10*	*0*
2,627	0	10	India 3½ per cent.	57	1,497	8	2	*1,471*	*2*	*4*
2,100	0	0	London and North-Western Consolidated 4 per cent. Preference 1881	60½	1,280	0	0	*1,291*	*10*	*0*
2,500	0	0	Canada 3½ per cent. 1930–50 (Deposited with Treasury)	66	1,650	0	0	*1,537*	*10*	*0*
2,500	0	0	London and South-Western Railway Consolidated 4 per cent. Preference	58½	1,462	10	0	*1,512*	*10*	*0*
810 52	10 12	3 7	War Stock 5 per cent. 1929–47 ,, ,, Post Office Issue	88½	763	17	4	*731*	*12*	*5*
1,400	0	0	War Bonds, 5 per cent. 1929–47	98	1,372	0	0	*1,330*	*0*	*0*
					12,734	1	6			
1,900	0	0	Treasury Bonds		1,900	0	0			
					£14,634	1	6	*£12,416*	*8*	*1*

GENERAL MEETINGS AT EDINBURGH.

Inaugural General Meeting.

On Wednesday, September 7, at 8.30 p.m., in the Usher Hall, Professor W. A. Herdman, C.B.E., F.R.S., resigned the office of President to Sir T. Edward Thorpe, C.B., F.R.S. In the greatly regretted absence of Sir Edward Thorpe owing to indisposition, his Address (for which see p. 1) was read by Principal Sir J. Alfred Ewing, K.C.B., F.R.S., a Vice-President of the Association.

The meeting was preceded by a recital of organ music by British composers, given by Mr. Robert M'Leod, Mus.Bac., F.R.C.O., Edinburgh.

Evening Discourses.

On Friday, September 9, at 8.30 p.m., in the Usher Hall, Professor C. E. Inglis, O.B.E., delivered a discourse on 'A Comparison of the Forth and Quebec Bridges, showing the Evolution of Cantilever Bridge Construction during the past thirty years.'

On Tuesday, September 13, at 8.30 p.m., in the Usher Hall, Professor W. A. Herdman, C.B.E., F.R.S., delivered a discourse on 'Edinburgh and Oceanography.'

Concluding General Meeting.

The Concluding General Meeting was held in the M'Ewan Hall on Wednesday, September 14, at 12 noon, the President, Sir Edward Thorpe, in the Chair. The following Resolutions were unanimously adopted, conveying the thanks of the Association:

(1) To the Lord Provost and the Citizens of Edinburgh, and to the University of Edinburgh, for their invitation to hold a meeting in their city, and for the cordial reception which they have given to the members of the British Association.

(2) To the University of Edinburgh, for the use of the McEwan Hall and other University buildings for the Sectional meetings; to H.M. Office of Works, for the use of the Parliament Hall as a reception room; to the Advocates' Library, the Royal Scottish Museum, and the Zoological Society of Scotland, for throwing open their buildings and garden for the instruction and delight of members; to the Very Reverend Dr. Wallace Williamson and the Kirk Session of St. Giles' Cathedral, for an occasion of public worship in that historic building; to the Royal Society of Edinburgh, the Colleges of Physicians and of Surgeons, the Philosophical Institution, the Society of Antiquaries of Scotland, the Royal Scottish Geographical Society, and other learned Societies and Institutions, for admittance to their libraries and collections; to the Air Ministry, for establishing a Meteorological Office at the place of meeting; and to the Carnegie Trust, for their hospitable reception at Dunfermline.

(3) To the Hon. Local Secretaries.

(4) To the Hon. Local Treasurer and the Members of the Finance Committee.

(5) To the Members of the Hospitality Committee, and especially to its Secretary, Miss S. H. Turcan; the Wardens of the University Hostels; the Committees of the University Union; the Women's University Union; and the

Clubs and Golf Clubs which have admitted Members of the Association during their stay.

(6) To the Members of the Reception and Rooms Committee, and especially to Dr. F. A. E. Crew; to the Publications Sub-Committee, the Excursions Committee, and the Entertainments Committee.

(7) To the Staffs of the University, the Advocates' Library, the Parliament Hall, the Town Clerk's Office; to Mr. Whitson's Office Staff; to the Station-masters of the Waverley and Caledonian Stations; to the Post Office officials at the Reception Room, and to the Reception Room Staff generally, and to the Boy Scouts, for their assistance at every point to ensure the success of a memorable meeting.

PUBLIC LECTURES AT EDINBURGH.

Public or Citizens' Lectures were delivered in the Usher Hall at 8 P.M. as follows:

Tuesday, September 6: Sir Oliver J. Lodge, F.R.S., on 'Speech through the Ether, or the Scientific Principles underlying Wireless Telephony.'

Thursday, September 8: Professor A. Dendy, F.R.S., on 'The Stream of Life.'

Monday, September 12: Professor H. J. Fleure, D.Sc., on 'Countries as Personalities.'

RESEARCH COMMITTEES, Etc.

APPOINTED BY THE GENERAL COMMITTEE, MEETING IN EDINBURGH: SEPTEMBER, 1921.

Grants of money, if any, from the Association for expenses connected with researches are indicated in heavy type.

For Committees concerned with the Nucleus Catalogue for the Carnegie United Kingdom Trust, see end of this list.

SECTION A.—MATHEMATICS AND PHYSICS.

Seismological Investigations.—Prof. H. H. Turner (*Chairman*), Mr. J. J. Shaw (*Secretary*), Mr. C. Vernon Boys, Dr. J. E. Crombie, Sir H. Darwin, Dr. C. Davison, Sir F. W. Dyson, Sir R. T. Glazebrook, Prof. C. G. Knott, Prof. H. Lamb, Sir J. Larmor, Prof. A. E. H. Love, Prof. H. M. Macdonald, Prof. H. C. Plummer, Mr. W. E. Plummer, Prof. R. A. Sampson, Sir A. Schuster, Sir Napier Shaw, Dr. G. T. Walker. **£100** (Caird Fund grant).

To assist work on the Tides.—Prof. H. Lamb (*Chairman*), Dr. A. T. Doodson (*Secretary*), Colonel Sir C. F. Close, Dr. P. H. Cowell, Sir H. Darwin, Dr. G. H. Fowler, Admiral F. C. Learmonth, Sir J. E. Petavel, Prof. J. Proudman, Major G. I. Taylor, Prof. D'Arcy W. Thompson, Sir J. J. Thomson, Prof. H. H. Turner. **£10**.

Annual Tables of Constants and Numerical Data, chemical, physical, and technological. —Sir E. Rutherford (*Chairman*), Prof. A. W. Porter (*Secretary*), Mr. A. E. G. Egerton. **£40** from Caird Fund, to be applied for from Council.

Calculation of Mathematical Tables.—Prof. J. W. Nicholson (*Chairman*), Dr. J. R. Airey (*Secretary*), Mr. T. W. Chaundy, Prof. L. N. G. Filon, Colonel Hippisley, Prof. E. W. Hobson, Mr. G. Kennedy, and Profs. Alfred Lodge, A. E. H. Love, H. M. Macdonald, G. B. Mathews, G. N. Watson, and A. G. Webster. **£20**.

Determination of Gravity at Sea.—Prof. A. E. H. Love (*Chairman*), Dr. W. G. Duffield (*Secretary*), Mr. T. W. Chaundy, Sir H. Darwin, Prof. A. S. Eddington, Major E. O. Henrici, Sir A. Schuster, and Prof. H. H. Turner.

Radiotelegraphic Investigations.—Sir Oliver Lodge (*Chairman*), Prof. W. H. Eccles (*Secretary*), Mr. S. G. Brown, Dr. C. Chree, Sir F. W. Dyson, Prof. A. S. Eddington, Dr. Erskine-Murray, Profs. J. A. Fleming, G. W. O. Howe, H. M. Macdonald, and J. W. Nicholson, Sir H. Norman, Sir A. Schuster, Sir Napier Shaw, and Prof. H. H. Turner.

Investigation of the Upper Atmosphere.—Sir Napier Shaw (*Chairman*), Mr. C. J. P. Cave (*Secretary*), Prof. S. Chapman, Mr. J. S. Dines, Mr. W. H. Dines, Sir R. T. Glazebrook, Col. E. Gold, Dr. H. Jeffreys, Sir J. Larmor, Mr. R. G. K. Lempfert, Prof. F. A. Lindemann, Dr. W. Makower, Sir J. E. Petavel, Sir A. Schuster, Dr. G. C. Simpson, Mr. F. J. W. Whipple, Prof. H. H. Turner.

To aid the work of Establishing a Solar Observatory in Australia.—Prof. H. H. Turner, (*Chairman*), Dr. W. G. Duffield (*Secretary*), Rev. A. L. Cortie, Dr. W. J. S. Lockyer, Mr. F. McClean, and Sir A. Schuster.

SECTION B.—CHEMISTRY.

Colloid Chemistry and its Industrial Applications.—Prof. F. G. Donnan (*Chairman*), Dr. W. Clayton (*Secretary*), Mr. E. Ardern, Dr. E. F. Armstrong, Prof. W. M. Bayliss, Prof. C. H. Desch, Dr. A. E. Dunstan, Mr. H. W. Greenwood, Mr. W. Harrison, Mr. E. Hatschek, Mr. G. King, Prof. W. C. McC. Lewis, Prof. J. W. McBain, Dr. R. S. Morell, Profs. H. R. Proctor and W. Ramsden, Dr. E. J. Russell, Mr. A. B. Searle, Dr. S. A. Shorter, Dr. R. E. Slade, Mr. Sproxton, Dr. H. P. Stevens, Mr. H. B. Stocks, Mr. R. Whymper. **£10**.

Fuel Economy ; Utilisation of Coal ; Smoke Prevention.—Prof. W. A. Bone (*Chairman*), Mr. H. James Yates (*Vice-Chairman*), Mr. Robert Mond (*Secretary*), Mr. A. H. Barker, Prof. P. P. Bedson, Dr. W. S. Boulton, Mr. E. Bury, Prof. W. E. Dalby, Mr. E. V. Evans, Dr. W. Galloway, Sir Robert Hadfield, Bart., Dr. H. S. Hele-Shaw, Mr. D. H. Helps, Dr. G. Hickling, Mr. A. Hutchinson, Mr. S. R. Illingworth, Principal G. Knox, Prof. Henry Louis, Mr. H. M. Morgans, Dr. J. S. Owens, Mr. W. H. Patchell, Mr. A. T. Smith, Dr. J. E. Stead, Mr. C. E. Stromeyer, Prof. W. W. Watts, Mr. C. H. Wordingham, and Mr. H. James Yates. **£5**.

Absorption Spectra and Chemical Constitution of Organic Compounds.—Prof. I. M. Heilbron (*Chairman*), Prof. E. E. C. Baly (*Secretary*), Prof. A. W. Stewart. **£10**.

SECTION C.—GEOLOGY.

The Old Red Sandstone Rocks of Kiltorcan, Ireland.—Prof. Grenville Cole (*Chairman*), Prof. T. Johnson (*Secretary*), Dr. J. W. Evans, Dr. R. Kidston, and Dr. A. Smith Woodward. **£15**.

To excavate Critical Sections in the Palæozoic Rocks of England and Wales.—Prof. W. W. Watts (*Chairman*), Prof. W. G. Fearnsides (*Secretary*), Prof. W. S. Boulton, Mr. E. S. Cobbold, Prof. E. J. Garwood, Mr. V. C. Illing, Dr. J. E. Marr, and Dr. W. K. Spencer. **£15**.

The Collection, Preservation, and Systematic Registration of Photographs of Geological Interest.—Prof. E. J. Garwood (*Chairman*), Prof. S. H. Reynolds (*Secretary*), Mr. G. Bingley, Dr. T. G. Bonney, Messrs. C. V. Crook, R. Kidston, and A. S. Reid, Sir J. J. H. Teall, Prof. W. W. Watts, and Messrs. R. Welch and W. Whitaker.

To consider the preparation of a List of Characteristic Fossils.—Prof. P. F. Kendall (*Chairman*), Prof. W. T. Gordon (*Secretary*), Prof. W. S. Boulton, Dr. A. R. Dwerryhouse, Profs. J. W. Gregory, Sir T. H. Holland, and S. H. Reynolds, Dr. Marie C. Stopes, Dr. J. E. Marr, Prof. W. W. Watts, Mr. H. Woods, and Dr. A. Smith Woodward.

To investigate the Flora of Lower Carboniferous times as exemplified at a newly-discovered locality at Gullane, Haddingtonshire.—Dr. R. Kidston (*Chairman*), Prof. W. T. Gordon (*Secretary*), Dr. J. S. Flett, Prof. E. J. Garwood, Dr. J. Horne, and Dr. B. N. Peach.

To investigate the Stratigraphical Sequence and Palæontology of the Old Red Sandstone of the Bristol district.—Mr. H. Bolton (*Chairman*), Mr. F. S. Wallis (*Secretary*), Miss Edith Bolton, Mr. D. E. I. Innes, Prof. C. Lloyd Morgan, Prof. S. H. Reynolds.

SECTION D.—ZOOLOGY.

To aid competent Investigators selected by the Committee to carry on definite pieces of work at the Zoological Station at Naples.— Mr. E. S. Goodrich (*Chairman*), Prof. J. H. Ashworth (*Secretary*), Dr. G. P. Bidder, Prof. F. O. Bower, Dr. W. B. Hardy, Sir S. F. Harmer, Prof. S. J. Hickson, Sir E. Ray Lankester, Prof. W. C. McIntosh, Dr. A. D. Waller. **£100** from Caird Fund, subject to approval of Council.

To summon meetings in London or elsewhere for the consideration of matters affecting the interests of Zoology, and to obtain by correspondence the opinion of Zoologists on matters of a similar kind, with power to raise by subscription from each Zoologist a sum of money for defraying current expenses of the organisation.—Prof. S. J. Hickson (*Chairman*), Dr. W. M. Tattersall (*Secretary*), Profs. G. C. Bourne, A. Dendy, J. Stanley Gardiner, W. Garstang, Marcus Hartog, W. A. Herdman, J. Graham Kerr, R. D. Laurie, E. W. MacBride, A. Meek, Dr. P. Chalmers Mitchell, and Prof. E. B. Poulton. **£20** for reprint and distribution of Report (not exceeding 2,500).

Zoological Bibliography and Publication.—Prof. E. B. Poulton (*Chairman*), Dr. F. A. Bather (*Secretary*), Mr. E. Heron-Allen, Dr. W. E. Hoyle, and Dr. P. Chalmers Mitchell. **£1**.

Gilbert White Memorial (Brent Valley Bird Sanctuary).—Sir S. F. Harmer (*Chairman*), Mr. W. Mark Webb (*Secretary*), Dr. W. T. Calman, Mr. E. Heron-Allen. **£5**.

Parthenogenesis.—Prof. A. Meek (*Chairman*), Mr. A. D. Peacock (*Secretary*), Mr. R. S. Bagnall, Dr. J. W. Heslop-Harrison. **£5**.

To nominate competent Naturalists to perform definite pieces of work at the Marine Laboratory, Plymouth.—Prof. A. Dendy (*Chairman and Secretary*), Prof. E. S. Goodrich, Prof. J. P. Hill, Prof. S. J. Hickson, Sir E. Ray Lankester.

Experiments in Inheritance in Silkworms.—Prof. W. Bateson (*Chairman*), Mrs. Merritt Hawkes (*Secretary*), Dr. F. A. Dixey, Prof. E. B. Poulton, Prof. R. C. Punnett.

Experiments in Inheritance of Colour in Lepidoptera.—Prof. W. Bateson (*Chairman*), The Hon. H. Onslow (*Secretary*), Dr. F. A. Dixey, Prof. E. B. Poulton.

SECTIONS E, L.—GEOGRAPHY, EDUCATION.

To formulate suggestions for a syllabus for the teaching of Geography both to Matriculation Standard and in Advanced Courses; to report upon the present position of the geographical training of teachers, and to make recommendations thereon; and to report, as occasion arises, to Council through the Organising Committee of Section E, upon the practical working of Regulations issued by the Board of Education affecting the position of Geography in Training Colleges and Secondary Schools.—Prof. T. P. Nunn (*Chairman*), Mr. W. H. Barker (*Secretary*), Mr. C. E. Browne, Sir H. J. Makinder.

SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

The Effects of the War on Credit, Currency, Finance, and Foreign Exchanges.—Prof. W. R. Scott (*Chairman*), Mr. J. E. Allen (*Secretary*), Prof. C. F. Bastable, Sir E. Brabrook, Dr. J. H. Clapham, Dr. Hugh Dalton, Mr. B. Ellinger, Sir D. Drummond Fraser, Mr. A. H. Gibson, Mr. C. W. Guillebaud, Mr. F. W. Hirst, Prof. A. W Kirkaldy, Mr. F. Lavington, Mr. D. H. Robertson, Mr. E. Sykes, Sir J. C. Stamp. **£25.**

SECTION G.—ENGINEERING.

To report on certain of the more complex Stress Distributions in Engineering Materials. —Prof. E. G. Coker (*Chairman*), Prof. L. N. G. Filon and Prof. A. Robertson (*Secretaries*), Prof. A. Barr, Dr. Gilbert Cook, Prof. W. E. Dalby, Sir J. A. Ewing, Messrs. A. R. Fulton and J. J. Guest, Dr. B. P. Haigh, Profs. Sir J. B. Henderson, C. E. Inglis, F. C. Lea, A. E. H. Love, and W. Mason, Sir J. E. Petavel, Dr. F. Rogers, Dr. W. A. Scoble, Mr. R. V. Southwell, Dr. T. E. Stanton, Mr. C. E. Stromeyer, and Mr. J. S. Wilson. **£10.**

SECTION H.—ANTHROPOLOGY.

To report on the Distribution of Bronze Age Implements.—Prof. J. L. Myres (*Chairman*), Mr. H. Peake (*Secretary*), Dr. E. C. R. Armstrong, Dr. G. A. Auden, Mr. H. Balfour, Mr. L. H. D. Buxton, Mr. O. G. S. Crawford, Sir W. Boyd Dawkins, Prof. H. J. Fleure, Mr. G. A. Garfitt, Dr. R. R. Marett, Mr. R. Mond, Sir C. H. Read, Sir W. Ridgeway. **£50.**

To conduct Archæological Investigations in Malta.—Prof. J. L. Myres (*Chairman*), Sir A. Keith (*Secretary*), Dr. T. Ashby, Mr. H Balfour, Dr. A. C. Haddon, Dr. R. R. Marett, Miss M. Murray, and Mr. H. Peake. **£25.**

To conduct Explorations with the object of ascertaining the Age of Stone Circles.—Sir C. H. Read (*Chairman*), Mr. H. Balfour (*Secretary*), Dr. G. A. Auden, Prof. Sir W. Ridgeway, Dr. J. G. Garson, Sir Arthur Evans, Sir W. Boyd Dawkins, Prof. J. L. Myres, and Mr. H. Peake. **£30**, provided work completed by December 31, 1921.

To excavate Early Sites in Macedonia.—Prof. Sir W. Ridgeway (*Chairman*), Mr. A. J. B. Wace (*Secretary*), Prof. R. C. Bosanquet, Mr. S. Casson, Dr. W. L. H. Duckworth, Prof. J. L. Myres.

To excavate a Palæolithic Site in Jersey.—Dr. R. R. Marett (*Chairman*), Mr. G. de Gruchy (*Secretary*), Dr. C. W. Andrews, Mr. H. Balfour, Prof. A. Keith, and Colonel Warton.

To report on the Classification and Distribution of Rude Stone Monuments.—Dr R. R. Marett (*Chairman*), Prof. H. J. Fleure (*Secretary*), Miss R. M. Fleming, Prof. J. L. Myres, Mr. H. Peake.

The Collection, Preservation, and Systematic Registration of Photographs of Anthropological Interest.—Sir C. H. Read (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Dr. G. A. Auden, Dr. H. O. Forbes, Mr. E. Heawood, Prof. J. L. Myres.

To conduct Archæological and Ethnological Researches in Crete.—Dr. D. G. Hogarth (*Chairman*), Prof. J. L. Myres (*Secretary*), Prof. R. C. Bosanquet, Dr. W. L. H. Duckworth, Sir A. Evans, Sir W. Ridgeway, Dr. F. C. Shrubsall.

To co-operate with Local Committees in excavation on Roman Sites in Britain.—Sir W. Ridgeway (*Chairman*), Mr. H. J. E. Peake (*Secretary*), Dr. T. Ashby, Mr. Willoughby Gardner, Prof. J. L. Myres.

To report on the present state of knowledge of the Ethnography and Anthropology of the Near and Middle East.—Dr. A. C. Haddon (*Chairman*), Mr. S. Casson (*Secretary*), Prof. H. J. Fleure, Mr. H. J. E. Peake.

To report on the present state of knowledge of the relation of early Palæolithic Instruments to Glacial Deposits.—Mr. H. J. E. Peake (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Mr. H. Balfour, Mr. M. Burkitt.

To investigate the Lake Villages in the neighbourhood of Glastonbury in connection with a Committee of the Somerset Archæological and Natural History Society.—Sir W. Boyd Dawkins (*Chairman*), Mr. A. Bulleid (*Secretary*), Mr. H. Balfour, Mr. Willoughby Gardner, Mr. F. S. Palmer, Mr. H. Peake.

To co-operate with a Committee of the Royal Anthropological Institute in the exploration of Caves in the Derbyshire district.—Sir W. Boyd Dawkins (*Chairman*), Mr. G. A. Garfitt (*Secretary*), Mr. Leslie Armstrong, Mr. E. N. Fallaize, Dr. R. R. Marett, Mr. H. Peake, Dr. W. M. Tattersall.

SECTION J.—PSYCHOLOGY.

The Place of Psychology in the Medical Curriculum.—Prof. G. Robertson (*Chairman*), Dr. W. Brown (*Secretary*), Dr. J. Drever, Dr. R. G. Gordon, Dr. C. S. Myers, Prof. J. H. Pear, Dr. W. H. R. Rivers.

Vocational Tests.—Dr. C. S. Myers (*Chairman*), Dr. G. H. Miles (*Secretary*), Prof. J. H. Pear, Mr. F. Watts.

SECTION K.—BOTANY.

Experimental Studies in the Physiology of Heredity.—Dr. F. F. Blackman (*Chairman*), Miss E. R. Saunders (*Secretary*), Profs. Bateson and Keeble. **£25**, subject to result of application elsewhere.

To continue Breeding Experiments on Oenothera and other Genera.—Dr. A. B. Rendle (*Chairman*), Dr. R. R. Gates (*Secretary*), Prof. W. Bateson, Mr. W. Brierley, Prof. O. V. Darbishire, Dr. M. C. Rayner. **£7. 5s.**

Primary Botanical Survey in Wales.—Dr. E. N. Miles Thomas (*Chairman*), Prof. O. V. Darbishire (*Secretary*), Miss A. J. Davey, Prof. F. W. Oliver, Prof. Stapledon, Mr. A. G. Tansley, Miss E. Vachell, Miss Wortham. **£10.**

The Renting of Cinchona Botanic Station in Jamaica.—Prof. F. O. Bower, (*Chairman*), Prof. A. J. Ewart (*Secretary*), Prof. F. F. Blackman.

SECTION L.—EDUCATIONAL SCIENCE.

Training in Citizenship.—Rt. Rev. J. E. C. Welldon (*Chairman*), Lady Shaw (*Secretary*), Sir R. Baden-Powell, Mr. C. H. Blakiston, Mr. G. D. Dunkerley, Mr. W. D. Eggar, Mr. C. R. Fay, Mr. J. C. Maxwell Garnett, Sir R. A. Gregory, and Sir T. Morison. **£10.**

To inquire into the Practicability of an International Auxiliary Language.—Dr. H. Foster Morley (*Chairman*), Dr. E. H. Tripp (*Secretary*), Mr. E. Bullough, Prof. J. J. Findlay, Sir Richard Gregory, Mr. W. B. Hardy, Dr. C. W. Kimmins, Sir E. Cooper Perry, Mr. Nowell Smith, Mr. A. E. Twentyman. **£5**.

The Influence of School Books upon Eyesight.—Dr. G. A. Auden (*Chairman*), Mr. G. F. Daniell (*Secretary*), Mr. C. H. Bothamley, Mr. W. D. Eggar, Sir R. A. Gregory, Dr. N. Bishop Harman, Mr. J. L. Holland, Dr. W. E. Sumpner, and Mr. Trevor Walsh.

CORRESPONDING SOCIETIES.

Corresponding Societies Committee for the preparation of their Report.—Mr. W. Whitaker (*Chairman*), Mr. W. Mark Webb (*Secretary*), Mr. P. J. Ashton, Dr. F. A. Bather, Rev. J. O. Bevan, Sir Edward Brabrook, Sir H. G. Fordham, Mr. T. Sheppard, Rev. T. R. R. Stebbing, Mr. Mark L. Sykes, and the President and General Officers of the Association. **£40**.

To take steps to obtain Kent's Cavern for the Nation.—Mr. W. Whitaker (*Chairman*), Mr. W. M. Webb (*Secretary*), Prof. Sir W. Boyd Dawkins, Mr. Mark L. Sykes.

LIST OF COMMITTEES.

Appointed by the General Committee to co-operate with the Carnegie United Kingdom Trust in preparing the Scientific Sections of a Nucleus Catalogue of Books for the use of Rural Libraries.

Section B (Chemistry).—Prof. C. H. Desch (*Chairman*), Dr. A. Holt (*Secretary*), Dr. C. H. Keane.

Section C (Geology).—Dr. J. S. Flett (*Chairman*), Mr. W. Whitaker (*Secretary*), Dr. J. W. Evans.

Section D (Zoology).—Sir S. F. Harmer (*Chairman*), Dr. W. T. Calman (*Secretary*), Prof. J. H. Ashworth, Dr. P. Chalmers Mitchell.

Section E (Geography).—Dr. H. R. Mill (*Chairman*), Dr. R. N. Rudmose Brown (*Secretary*), Mr. G. G. Chisholm, Mr. O. J. R. Howarth, Dr. Marion Newbigin.

Section F (Economics).—Prof. E. Cannan (*Convener*), Prof. H. M. Hallsworth, Miss Jebb.

Section H (Anthropology).—Dr. E. S. Hartland (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Mr. W. Crooke, Prof. H. J. Fleure.

Section I (Physiology).—Prof. H. E. Roaf (*Chairman*), Dr. C. Lovatt Evans (*Secretary*).

Section J (Psychology).—Dr. J. Drever (*Chairman*), Miss Bickersteth (*Secretary*), Dr. H. J. Watt.

Section K (Botany).—Dr. H. W. T. Wager (*Chairman*), Mr. F. T. Brooks (*Secretary*), Prof. W. Neilson Jones, Prof. F. E. Weiss.

Section L (Education).—Dr. A. Darroch (*Chairman*), Prof. J. A. Green (*Secretary*), Prof. T. P. Nunn.

THE CAIRD FUND.

An unconditional gift of £10,000 was made to the Association at the Dundee Meeting, 1912, by Mr. (afterwards Sir) J. K. Caird, LL.D., of Dundee.

The Council, in its report to the General Committee at the Birmingham Meeting, made certain recommendations as to the administration of this Fund. These recommendations were adopted, with the Report, by the General Committee at its meeting on September 10, 1913.

The following allocations have been made from the Fund by the Council to September 1921:—

Naples Zoological Station Committee (p. xxxiii).—£50 (1912–13); £100 (1913–14); £100 annually in future, subject to the adoption of the Committee's report (reduced to £50 during war; suspended, 1920–21, pending approval by Council of Committee's report on future control of the Station, etc.).

Seismology Committee (p. xxxi).—£100 (1913–14); £100 annually in future, subject to the adoption of the Committee's report.

Radiotelegraphic Committee (p. xxxii).—£500 (1913–14).

Magnetic Re-survey of the British Isles (in collaboration with the Royal Society).—£250.

Committee on Determination of Gravity at Sea (p. xxxii).—£100 (1914–15).

Mr. F. Sargent, Bristol University, in connection with his Astronomical Work.—£10 (1914).

Organising Committee of Section F (Economics), towards expenses of an Inquiry into Outlets for Labour after the War.—£100 (1915).

Rev. T. E. R. Phillips, for aid in transplanting his private observatory.—£20 (1915).

Committee on Fuel Economy (p. xxxii).—£25 (1915–16), £10 (1919–20).

Committee on Training in Citizenship (p. xxxv).—£10 (1919–20).

Geophysical Committee of Royal Astronomical Society.—£10 (1920).

Conjoint Board of Scientific Societies.—£10 (1920); £10 (1921).

Marine Biological Association, Plymouth.—£200 (1921).

On September 7, 1921, the Council authorised the expenditure of £300 from accumulated income of the fund upon grants to Research Committees approved by the General Committee at the Edinburgh Meeting, in addition to grants ordinarily made by, or applied for from, the Council.

Sir J. K. Caird, on September 10, 1913, made a further gift of £1,000 to the Association, to be devoted to the study of Radio-activity. In 1920 the Council decided to devote the principal and interest of this gift at the rate of £250 per annum for five years to purposes of the research intended. The grant for the year ending March 24, 1922, was made to Sir E. Rutherford, F.R.S.

RESOLUTIONS & RECOMMENDATIONS.

The following Resolutions and Recommendations were referred to the Council (unless otherwise stated) by the General Committee at Edinburgh for consideration and, if desirable, for action :—

By the General Committee.

That the General Committee commends the action of the Council in encouraging arrangements for joint discussions on subjects of interest to two or more Sections.

From Section B.

That in the opinion of the Sectional Committee it is desirable that all Reports of Research Committees should be sent, in the first instance, to the Organising Committee of the appropriate Section (through the Recorder) before the Annual Meeting.

From Section B.

The Sectional Committee recommends the re-appointment of the Fuel Economy Committee. The Sectional Committee requests the General Committee to inform the Council that, whilst in the opinion of some of the members of the Sectional Committee the present position of the Fuel Economy Committee is not satisfactory, it has been thought undesirable, in the enforced absence of Professor Bone, to enter into a detailed consideration of the subject. The Sectional Committee recommends the publication of the Fourth Report of the Fuel Economy Committee.

From Sections E and L.

To draw the attention of the Council to the Revised Regulations for Secondary Schools, England, 1921, so far as they concern instruction in Geography, and to ask the Council to take the necessary steps to ascertain :—

1. Whether the effect of the new regulations is to ensure the continuity of instruction in Geography up to the age of 16 (as appears to be provided in page 7, pars. 6 and 7).
2. Whether it is the intention of the Board that the phrase 'language, literature, and history' in 48 (*a*) B, C, D, in the absence of specific reference to the Geography of selected countries, should be construed as necessarily including adequate geographical study of the selected region.
3. Whether the Board accepts Geography as a 'main subject of study' under Sub-section A, Science and Mathematics.

(In view of the urgency of the above resolution, the General Committee instructed the General Secretaries to take action upon it and report to the Council.)

From Section E.

That the Council recommend that the Census authorities of the United Kingdom should, if practicable, indicate in their final report the population not merely of municipal and other administrative areas, but also of urban aggregates.

(In view of the urgency of the above resolution, the General Committee instructed the General Secretaries to take action upon it and report to the Council.)

From Section E.

The Committee of Section E suggests that the Council of the Association should ask the Air Ministry to be kind enough to supply the Council with a detailed statement, giving the reasons for the adoption of Mercator's projection for the international series of aeronautical maps.

From Section H.

That it is in the interests of the Empire that a knowledge of Anthropology should be more widely disseminated;

That for this purpose Universities and other institutions be encouraged to provide instruction in this subject;

And, further, that there should be a central institution in London, not necessarily new, for the collection, co-ordination, and publication of the results of anthropological research, and the provision of information derived therefrom, for the use of the Imperial Services, teachers, missionaries, and others;

That the Council of the Association, in conjunction with other bodies interested, be requested to consider the advisability of taking such steps as may be necessary to secure this end.

From Section L.

The Committee of Section L recommends the Council to call the attention of the Board of Education to the desirability of strengthening the position of Music, and its appreciation, in the curriculum of Secondary Schools.

From the Conference of Delegates (a) *and Section L* (b).

(*a*) That the Council be asked to represent to the Postmaster-General the very heavy burden which the postage of their publications and notices entails upon scientific societies, and to request him to alleviate it at the earliest possible moment.

(*b*) That Section L, whilst believing it to be useless to appeal to the Postmaster-General to reduce the cost of postage on scientific publications, recommend the Council to appeal to the Presidents of the Royal Society and other scientific bodies to approach the Postmaster-General, urging him that scientific publications may be registered for postage reduction.

COMMUNICATIONS RECOMMENDED FOR PRINTING IN EXTENDED FORM.

Sections A and B.—Report of Joint Discussion on the Structure of Molecules. (See p. 468.)

Section A.—Report of Discussion on the Quantum Theory. (See p. 473.)

Section A.—Extended Abstract of Professor J. C. Kapteyn's Paper on 'The Structure and Motions of the Stellar Systems.' (See p. 409.)

Section L.—Dr. E. H. Griffiths's Paper on 'Science and Ethics.' (See p. 479.)

THE

PRESIDENTIAL ADDRESS,

BY

Sir T. EDWARD THORPE, C.B., D.Sc., Sc.D., LL.D., F.R.S., Hon. F.R.S., Edin.,

PRESIDENT OF THE ASSOCIATION.

The British Association for the Advancement of Science owes its origin, and, in great measure, its specific aims and functions, to the public spirit and zeal for the interests of science of Scotsmen. Its virtual founder was Sir David Brewster; its scope and character were defined by Principal Forbes. In constitution it differed from the migratory scientific associations existing on the Continent, which mainly served to promote the social intercourse of their members by annual gatherings, in that it was to be a permanent organisation, with a settled establishment and headquarters, which should have not merely its yearly reunions, but which, 'by methods and by influence peculiarly its own, should continue to operate during the intervals of these public assemblies, and should aspire to give an impulse to every part of the scientific system; to mature scientific enterprise; and to direct the labours requisite for discovery.'

Although, for reasons of policy, it was decided that its first meeting of September 27, 1831, should be held at York, as the most central city for the three kingdoms, and its second and third meetings at the ancient Universities of Oxford and Cambridge respectively, it was inevitable that the Association should seize the earliest opportunity to visit the Metropolis of Scotland where, as an historical fact, it may be said to have had its origin.

The meeting in this city of September 8, 1834, was noteworthy for many reasons. It afforded the first direct proof that the Association was fulfilling its purpose. This was shown by the popular appreciation which attended its activities, by the range and character of its reports on the state and progress of science, by the interest and value of its sectional proceedings, and by the mode in which its funds were employed. In felicitous terms the President of the preceding year, the Rev. Professor Sedgwick, congratulated the gathering 'on the increased strength in which they had assembled, in a place endeared to the feelings of every lover of science by so many delightful and elevating

recollections, especially by the recollection of the great men whom it had fostered, or to whom it had given birth.' In a few brief sentences Professor Sedgwick indicated the great power which this Association is able to apply towards the advancement of science by combination and united action, and he supported his argument by pointing to the results which it had already achieved during the three short years of its existence. Professor Sedgwick's words are no less true to-day. His contention that one of the most important functions of this philosophical union is to further what he termed the 'commerce of ideas' by joint discussions on subjects of kindred interest, has been endorsed by the recent action of the Council in bringing the various sections into still closer touch with each other with a view to the discussion of common problems of general interest. This slight reorganisation of the work of the Sections, which is in entire accord with the spirit and aims of the Association, as defined by its progenitors and formulated in its constitution, will take effect during the present meeting. Strictly speaking, such joint sectional discussions are not unknown in our history, and their utility and influence have been freely recognised. But hitherto the occasions have been more or less informal. They are now, it is hoped, to be part of the regular official procedure of the meetings, to which it is anticipated they will afford additional interest and value.

Another noteworthy change in our procedure is the introduction of discussions on the addresses of the Presidents of Sections. Hitherto these addresses have been formally read and never discussed. To the extent that they have been brief chronicles of the progress of the special departments of science with which the section is concerned they have given but little opportunity for discussion. With the greatly increased facilities which now exist for every worker to keep himself informed of the development of the branch of knowledge in which he is more particularly interested, such *résumés* have in great measure lost their true purpose, and there has, consequently, been a growing tendency of late years for such presidential addresses to deal with contemporary topics of general interest and of fundamental importance, affording ample opportunity for a free exchange of opinion. The experiment will certainly conduce to the interest of the proceedings of the sections, and will contribute to the permanent value of their work. We see in these several changes the development of ideas connected with the working of the Association which may be said to have had their birth at its first meeting in Edinburgh, eighty-seven years ago.

Sixteen years later, that is on July 21, 1850, Edinburgh again extended her hospitality to the British Association, which then honoured itself by electing the learned Principal of the United Colleges of St. Salvator and St. Leonard, St. Andrews, to the presidential chair—at once a tribute to Sir David Brewster's eminence as a natural philo-

sopher, and a grateful recognition of his services to this body in suggesting and promoting its formation.

On the occasion of his inaugural address, after a brief account of recent progress in science, made with the lucidity of expression which characterised all the literary efforts of the learned biographer of Newton and versatile editor of the *Edinburgh Encyclopædia*, the *Edinburgh Magazine*, and the *Edinburgh Journal of Science*, the President dwelt upon the beneficent influence of the Association in securing a more general attention to the objects of science, and in effecting a removal of disadvantages of a public kind that impeded its progress. It was largely to the action of the Association, assisted by the writings and personal exertions of its members, that the Government was induced to extend a direct national encouragement to science and to aid in its organisation.

Brewster had a lofty ideal of the place of science in the intellectual life of a community, and of the just position of the man of science in the social scale. In well-weighed words, the outcome of matured experience and of an intimate knowledge of the working of European institutions created for the advancement of science and the diffusion of knowledge, he pleaded for the establishment of a national institution in Britain, possessing a class of resident members who should devote themselves wholly to science—with a place and station in society the most respectable and independent—'free alike,' as Playfair put it, 'from the embarrassments of poverty or the temptations of wealth.' Such men, 'ordained by the State to the undivided functions of science,' would, he contended, do more and better work than those who snatch an hour or two from their daily toil or nightly rest.

This ideal of 'combining what is insulated, and uniting in one great institution the living talent which is in active but undirected and unbefriended exercise around us,' was not attained during Brewster's time; nor, notwithstanding the reiteration of incontrovertible argument during the past seventy years, has it been reached in our own.

I have been led to dwell on Sir David Brewster's association with this question of the relations of the State towards research for several reasons. Although he was not the first to raise it—for Davy more than a century ago made it the theme of presidential addresses, and brought his social influence to bear in the attempt to enlist the practical sympathy of the Government—no one more consistently urged its national importance, or supported his case with a more powerful advocacy, than the Principal of the University of Edinburgh. It is only seemly, therefore, that on this particular occasion, and in this city of his adoption, where he spent so much of his intellectual energy, I should specially allude to it. Moreover, we can never forget what this Association owes to his large and fruitful mind. Every man is a

debtor to his profession, from which he gains countenance and profit. That Brewster was an ornament to his is acknowledged by every lover of learning. That he endeavoured to be a help to it was gratefully recognised during his lifetime. After his death it was said of him that the improved position of men of science in our time is chiefly due to his exertions and his example.

I am naturally led to connect the meeting of 1850 with a still more memorable gathering of this Association in this city. In August 1871—just over half a century ago—the British Association again assembled in Edinburgh under the presidency of Lord Kelvin—then Sir William Thomson. It was an historic occasion by reason of the address which inaugurated its proceedings. Lord Kelvin, with characteristic force and insistence, still further elaborated the theme which had been so signal a feature of Sir David Brewster's address twenty years previously: 'Whether we look to the honour of England,' he said, 'as a nation which ought always to be the foremost in promoting physical science, or to those vast economical advantages which must accrue from such establishments, we cannot but feel that experimental research ought to be made with us an object of national concern, and not left, as hitherto, exclusively to the private enterprise of self-sacrificing amateurs, and the necessarily inconsecutive action of our present Governmental Departments and of casual committees.'

Lord Kelvin, as might have been anticipated, pleaded more especially for the institution of physical observatories and laboratories for experimental research, to be conducted by qualified persons, whose duties should be not teaching, but experimenting. Such institutions as then existed, he pointed out, only afforded a very partial and inadequate solution of a national need. They were, for the most part, 'absolutely destitute of means, material, or *personnel* for advancing science, except at the expense of volunteers, or of securing that volunteers should be found to continue such little work as could then be carried on.'

There were, however, even then, signs that the bread cast upon the waters was slowly returning after many days. The establishment of the Cavendish Laboratory at Cambridge, by the munificence of its then Chancellor, was a notable achievement. Whilst in its constitution as part of a university discipline it did not wholly realise the ideal of the two Presidents, under its successive directors, Prof. Clerk-Maxwell, the late Lord Rayleigh, and Sir J. J. Thomson, it has exerted a profound influence upon the development of experimental physics, and has inspired the foundation of many similar educational institutions in this country. Experimental physics has thus received an enormous impetus during the last fifty years, and although in matters of science there is but little folding of the hands to sleep, 'the divine discontent' of its followers

has little cause for disquietude as regards the position of physics in this country.

In the establishment of the National Physical Laboratory we have an approach to the ideal which my predecessors had so earnestly advocated. Other Presidents, among whom I would specially name the late Sir Douglas Galton, have contributed to this consummation. The result is a remarkable testimony to the value of organised and continuous effort on the part of the British Association in forming public opinion and in influencing Departmental action. It would, however, be ungrateful not to recall the action of the late Lord Salisbury—himself a follower of science and in full sympathy with its objects—in taking the first practical steps towards the creation of this magnificent national institution. I may be allowed, perhaps, to refer to this matter, as I have personal knowledge of the circumstances, being one of the few survivors of the Committee which Lord Salisbury caused to be formed, under the chairmanship of the late Lord Rayleigh, to inquire and report upon the expediency of establishing an institution in Great Britain upon the model of certain State-aided institutions already existing on the Continent, for the determination of physical constants of importance in the arts, for investigations in physical problems bearing upon industry, for the standardisation and verification of physical instruments, and for the general purposes of metrology. I do not profess to give the exact terms of the reference to the Committee, but, in substance, these were recognised to be the general aims of the contemplated institute. The evidence we received from many men of science, from Departmental officers, and from representatives of engineering and other industrial establishments, was absolutely unanimous as to the great public utility of the projected laboratory. It need hardly be said that the opportunity called forth all the energy and power of advocacy of Lord Kelvin, and I well remember with what strength of conviction he impressed his views upon the Committee. That the National Physical Laboratory has, under the ability, organising power, and business capacity of its first director, Sir Richard Glazebrook, abundantly justified its creation is recognised on all hands. Its services during the four years of war alone are sufficient proof of its national value. It has grown to be a large and rapidly increasing establishment, occupying itself with an extraordinary range of subjects, with a numerous and well-qualified staff, engaged in determinative and research work on practically every branch of pure and applied physics. The range of its activities has been further increased by the establishment since the war of co-ordinating research boards for physics, chemistry, engineering and radio-research. Government Departments have learned to appreciate its services. The photometry division, for example, has been busy on experiments on navigation lamps for the

Board of Trade, on miners' lamps for the Home Office and on motor-car head-lamps for the Ministry of Transport, and on the lighting of the National Gallery and the Houses of Parliament. Important work has been done on the forms of ships, on the steering and manœuvring of ships, on the effect of waves on ship resistance, on the interaction between passing ships, on seaplane floats, and on the hulls of flying-boats.

It is also actively engaged in the study of problems connected with aviation, and has a well-ordered department for aerodynamical research.

It can already point to a long and valuable series of published researches, which are acknowledged to be among the most important contributions to pure and applied physics which this country has made during recent years.

I may be pardoned, I hope, for another personal reference, if I recall that it was at the Edinburgh meeting, under Lord Kelvin's presidency, fifty years ago, that I first became a member of this Association, and had the honour of serving it as one of the secretaries of its chemical section. Fifty years is a considerable span in the life of an individual, but it is a relatively short period in the history of science. Nevertheless, those fifty years are richer in scientific achievement and in the importance and magnitude of the utilitarian applications of practically every branch of science than any preceding similar interval. The most cursory comparison of the state of science, as revealed in his comprehensive address, with the present condition of those departments on which he chiefly dwelt, will suffice to show that the development has been such that even Lord Kelvin's penetrative genius, vivid imagination, and sanguine temperament could hardly have anticipated. No previous half-century in the history of science has witnessed such momentous and far-reaching achievements. In pure chemistry it has seen the discovery of argon by Rayleigh, of radium by Madame Curie, of helium as a terrestrial element by Ramsay, of neon, xenon, and krypton by Ramsay and Travers, the production of helium from radium by Ramsay and Soddy, and the isolation of fluorine by Moissan. These are undoubtedly great discoveries, but their value is enormously enhanced by the theoretical and practical consequences which flow from them.

In applied chemistry it has witnessed the general application of the Gilchrist-Thomas process of iron-purification, the production of calcium cyanamide by the process of Frank and Caro, Sabatier's process of hydrogenation, a widespread application of liquefied gases, and Haber's work on ammonia synthesis—all manufacturing processes which have practically revolutionised the industries with which they are concerned.

In pure physics it has seen the rise of the electron theory, by

Lorentz; Hertz's discovery of electro-magnetic waves; the investigation of cathode rays by Lenard, and the elucidation of crystal structure by Bragg.

It has seen, moreover, the invention of the telephone, the establishment of incandescent lighting, the electric transmission of force, the invention of the cinematograph, of wireless telegraphy, the application of the Röntgen rays, and the photographic reproduction of colour.

In physical chemistry it has witnessed the creation of stereochemistry by Van t'Hoff and Le Bel, Gibbs' work on the phase rule, Van t'Hoff's theory of solutions, Arrhenius's theory of ionic dissociation, and Nernst's theory of the galvanic cell.

Such a list is far from complete, and might be greatly extended. But it will at least serve to indicate the measure of progress which the world owes to the development and application during the last fifty years of the two sciences—physics and chemistry—to which Lord Kelvin specially referred.

The more rapid dissemination of information concerning the results of recent or contemporary investigation, which Lord Kelvin so strongly urged as 'an object to which the powerful action of the British Association would be thoroughly appropriate,' has been happily accomplished. The timely aid of the Association in contributing to the initial expense of preparing and publishing monthly abstracts of foreign chemical literature by the Chemical Society is gratefully remembered by British chemists. The example has been followed by the greater number of our scientific and technical societies, and the results of contemporary inquiry in every important branch of pure and applied science are now quickly brought to the knowledge of all interested workers. In fact, as regards the particular branch of science with which I am more directly concerned, the arrangements for the preparation and dissemination of abstracts of contemporary foreign chemical literature are proving to be a veritable embarrassment of riches, and there is much need for co-operation among the various distributing societies. This need is especially urgent at the present time owing to the greatly increased cost of paper, printing, binding, and indeed of every item connected with publication, which expense, of course, ultimately falls upon the various societies and their members. The problem, which has already received some attention from those entrusted with the management of the societies referred to, is not without its difficulties, but these are not insoluble. There is little doubt that a resolute and unanimous effort to find a solution would meet with success.

The present high cost of book production, which in the case of specialised books is about three times what it was in 1914, is exercising a most prejudicial effect upon the spread of scientific knowledge. Books on science are not generally among the 'best sellers.' They appeal to a

comparatively limited and not particularly wealthy public, largely composed of the professional classes who have suffered in no small measure from the economic effects of the War. The present high price of this class of literature is to the public detriment. Eventually it is no less to the detriment of the printing and publishing trades. Publishers are well aware of this fact, and attempts are being made by discussions between employers and the executives of the Typographical Association and other societies of compositors to reach an equitable solution, and it is greatly to be hoped that it will be speedily found.

All thinking men are agreed that science is at the basis of national progress. Science can only develop by research. Research is the mother of discovery, and discovery of invention. The industrial position of a nation, its manufactures and commerce, and ultimately its wealth, depend upon invention. Its welfare and stability largely rest upon the equitable distribution of its wealth. All this seems so obvious, and has been so frequently and so convincingly stated, that it is superfluous to dwell upon it in a scientific gathering to-day.

A late distinguished Admiral, you may remember, insisted on the value of reiteration. On this particular question it was never more needed than now. The crisis through which we have recently passed requires it in the interests of national welfare. Of all post-war problems to engage our serious attention, none is more important in regard to our position and continued existence than the nation's attitude towards science and scientific research, and there is no more opportune time than the present in which to seek to enforce the teaching of one of the most pregnant lessons of our late experience.

It is, unfortunately, only too true that the industrial world has in the past underrated the value of research. One indication that the nation is at length aroused to its importance is to be seen in the establishment of the Department of Scientific and Industrial Research, with its many subordinate associations. The outbreak of the Great War, and much in its subsequent history, revealed, as we all know, many national shortcomings, due to our indifference to and actual neglect of many things which are at the root of our prosperity and security. During the War, and at its close, various attempts, more or less unconnected, were made to find a remedy. Of the several committees and boards which were set up, those which still exist have now been co-ordinated, and brought under the control of a central organisation—the Department of Scientific and Industrial Research. Research has now become a national and State-aided object. For the first time in our history its pursuit with us has been organised by Government action. As thus organised it seeks to fulfil the aspirations to which I have referred, whilst meeting many of the objections which have been urged against the endowment of research. It must be recognised that modern

ideas of democracy are adverse to the creation of places to which definite work is not assigned and from which definite results do not emanate. This objection, which strikes at the root of the establishment of such an institution as Sir David Brewster contemplated, is, to a large extent, obviated by the scheme of the Department of Scientific and Industrial Research. It does not prescribe or fetter research, but, whilst aiding by personal payments the individual worker, leaves him free to pursue his inquiry as he thinks best. Grants are made, on the recommendation of an Advisory Council of experts, to research workers in educational institutions and elsewhere, in order to promote research of high character on fundamental problems of pure science or in suitable cases on problems of applied science. Of the boards and committees and similar organisations established prior to or during the War, or subsequent to it, with one or two exceptions, all are now directly under the Department. They deal with a wide range of subjects, such as the Building Research Board, established early in 1920 to organise and supervise investigations on building materials and construction, to study structural failures, and to fix standards for structural materials. The Food Investigation Board deals with the preservation by cold of food, and with the engineering problems of cold storage, with the chemistry of putrefaction, and the agents which induce it, with the bionomics of moulds, and the chemistry of edible oils and fats. The Fuel Research Board is concerned with the immediate importance of fuel economy and with investigations of the questions of oil-fuel for the Navy and Mercantile Marine, the survey of the national coal resources, domestic heating, air pollution, pulverised fuel, utilisation of peat, the search for possible substitutes for natural fuel oil, and for practicable sources of power alcohol.

The Geological Survey Board has taken over the Geological Survey of Great Britain and the control of the Museum of Practical Geology. The maintenance of the National Physical Laboratory, originally controlled by a General Board and an Executive Committee appointed by the President and Council of the Royal Society, is now transferred to the Department of Scientific and Industrial Research. A Mines Research Committee and a Mine Rescue Apparatus Committee are attached to the Department. The former is concerned with such questions as the determination of the geothermic gradient, the influence of temperature of intake and return air on strata, the effect of seasonal changes on strata temperature of intakes, the cooling effect due to the evolution of fire-damp, heat production from the oxidation of timber, etc. The Department is also directing inquiries on the preservation and restoration of antique objects deposited in the British Museum. It is concerned with the gauging of rivers and tidal currents, with special reference to a hydrographical survey of Great Britain in relation to

the national resources of water-power. In accordance with the Government policy, four co-ordinating boards have been established to organise scientific work in connection with the fighting forces, so as to avoid unnecessary overlapping and to provide a single direction and financial control. The four boards deal, respectively, with chemical and physical problems, problems of radio-research, and engineering. These boards have attached to them various committees dealing with special inquiries, some of which will be carried out at the National Physical Laboratory. The Government have also authorised the establishment of a Forest Products Research Board.

The Department is further empowered to assist learned or scientific societies and institutions in carrying out investigations. Some of these were initiated prior to the War, and were likely to be abandoned owing to lack of funds. Whenever the investigation has a direct bearing upon a particular industry that had not hitherto been able to establish a Research Association, it has been a condition of a grant that the institution directing the research should obtain contributions towards the cost on a £ for £ basis, either directly through its corporate funds or by special subscriptions from interested firms. On the formation of the appropriate association the research is, under suitable safeguards, transferred to it for continuance. The formation of a number of Research Associations has thus been stimulated, dealing, for example, with scientific instruments, non-ferrous metals, glass, silk, refractories, electrical and allied industries, pottery, etc.

Grants are made to Research Associations formed voluntarily by manufacturers for the purposes of research, from a fund of a million sterling, placed at the disposal of the Research Department for this purpose. Such Associations, to be eligible for the grant, must submit Articles of Association for the approval of the Department and the Board of Trade. If these are approved, licences are issued by the Board of Trade recognising the Associations as limited liability companies working without profits. Subscriptions paid to an Association by contributing firms are recognised by the Board of Inland Revenue as business costs of the firms, and are not subject to income or excess profits taxes. The income of the Association is similarly free of income tax. Grants are ordinarily made to these Associations on the basis of £1 for every £1 raised by the Association between limits depending upon the particular industry concerned. In the case of two Research Associations grants are made at a higher rate than £ for £, as these industries are regarded as having a special claim to State assistance on account of their 'pivotal' character. The results of research are the sole property of the Association making them, subject to certain rights of veto possessed by the Department for the purposes of ensuring that they are not communicated to foreign countries, except with the consent

of the Department, and that they may be made available to other interested industries and to the Government itself on suitable terms.

These arrangements have been found to be generally satisfactory, and at the present time twenty-four of such Research Associations have been formed to whom licences have been issued by the Board of Trade. Others are in process of formation, and may be expected to be at work at an early date. These Research Associations are concerned with nearly all our leading industries. The official addresses of most of them are in London; others have their headquarters in Manchester, Leeds, Sheffield, Birmingham, Northampton, Coventry, Glasgow, and Belfast.

The Department has further established a Records Bureau, which is responsible for receiving, abstracting, filing and collating communications from research workers, boards, institutions, or associations related to or supervised by the Department. This information is regarded as confidential, and will not be communicated except in writing, and after consultation with the research worker or organisation from which it has been received. Also such non-confidential information as comes into the possession of the Department which is of evident or probable value to those working in touch with the Department is collected and filed in the Bureau and made generally available.

It is also a function of the Bureau to effect economy in preventing repetition and overlapping of investigations and in ensuring that the fullest possible use is made of the results of research. Thus, the programmes of Research Associations are compared in order to ensure that researches are not unwittingly duplicated by different Research Associations. Sometimes two or more Research Associations may be interested in one problem from different points of view, and when this occurs it may be possible for the Bureau to arrange a concerted attack upon the common problem, each Research Association undertaking that phase of the work in which it is specially interested and sharing in the general results.

As researches carried out under the Department frequently produce results for which it is possible to take out patents, careful consideration has been given to the problems of policy arising on this subject, and other Government Departments also interested have been freely consulted. As the result, an Inter-Departmental Committee has been established with the following terms of reference:—

(1) To consider the methods of dealing with inventions made by workers aided or maintained from public funds, whether such workers be engaged (*a*) as research workers, or (*b*) in some other technical capacity, so as to give a fair reward to the inventor and thus encourage further effort, to secure the

utilisation in industry of suitable inventions and to protect the national interest, and

(2) To outline a course of procedure in respect of inventions arising out of State-aided or supported work which shall further these aims and be suitable for adoption by all Government Departments concerned.

About forty patents have been taken out by the Department jointly with the inventors and other interested bodies, but of these, nine have subsequently been abandoned. At least five patents have been developed to such a stage as to be ready for immediate industrial application.

It will be obvious from this short summary of the activities of the Department, based upon information kindly supplied to me by Sir Francis Ogilvie, that this great scheme of State-aided research has been conceived and is administered on broad and liberal lines. A considerable number of valuable reports from its various boards and committees have already been published, and others are in the press, but it is, of course, much too soon to appreciate the full effects of their operations. But it can hardly be doubted that they are bound to exercise a profound influence upon industries which ultimately depend upon discovery and invention. The establishment of the Department marks an epoch in our history. No such comprehensive organisation for the application of science to national needs has ever been created by any other State. We may say we owe it directly to the Great War. Even from the evil of that great catastrophe there is some soul of goodness would we observingly distil it out.

I turn now to a question of scientific interest which is attracting general attention at the present time. It is directly connected with Lord Kelvin's address fifty years ago.

The molecular theory of matter—a theory which in its crudest form has descended to us from the earliest times and which has been elaborated by various speculative thinkers through the intervening ages—hardly rested upon an experimental basis until within the memory of men still living. When Lord Kelvin spoke in 1871, the best-established development of the molecular hypothesis was exhibited in the kinetic theory of gases as worked out by Joule, Clausius, and Clerk-Maxwell. As he then said, no such comprehensive molecular theory had ever been even imagined before the nineteenth century. But, with the eye of faith, he clearly perceived that, definite and complete in its area as it was, it was ' but a well-drawn part of a great chart, in which all physical science will be represented with every property of matter shown in dynamical relation to the whole. The prospect we now have of an early completion of this chart is based on the assumption of atoms. But there can be no permanent satisfaction to the mind in explaining

heat, light, elasticity, diffusion, electricity and magnetism, in gases, liquids and solids, and describing precisely the relations of these different states of matter to one another by statistics of great numbers of atoms when the properties of the atom itself are simply assumed. When the theory, of which we have the first instalment in Clausius and Maxwell's work, is complete, we are but brought face to face with a superlatively grand question: What is the inner mechanism of the atom?'

If the properties and affections of matter are dependent upon the inner mechanism of the atom, an atomic theory, to be valid, must comprehend and explain them all. There cannot be one kind of atom for the physicist and another for the chemist. The nature of chemical affinity and of valency, the modes of their action, the difference in characteristics of the chemical elements, even their number, internal constitution, periodic position, and possible isotopic rearrangements must be accounted for and explained by it. Fifty years ago chemists, for the most part, rested in the comfortable belief of the existence of atoms in the restricted sense in which Dalton, as a legacy from Newton, had imagined them. Lord Kelvin, unlike the chemists, had never been in the habit of 'evading questions as to the hardness or indivisibility of atoms by virtually assuming them to be infinitely small and infinitely numerous.' Nor, on the other hand, did he realise, with Boscovich, the atom 'as a mystic point endowed with inertia and the attribute of attracting or repelling other such centres.' Science advances not so much by fundamental alterations in its beliefs as by additions to them. Dalton would equally have regarded the atom 'as a piece of matter of measureable dimensions, with shape, motion, and laws of action, intelligible subjects of scientific investigation.'

In spite of the fact that the atomic theory, as formulated by Dalton, has been generally accepted for nearly a century, it is only within the last few years that physicists have arrived at a conception of the structure of the atom sufficiently precise to be of service to chemists in connection with the relation between the properties of elements of different kinds, and in throwing light on the mechanism of chemical combination.

This further investigation of the 'superlatively grand question—the inner mechanism of the atom'—has profoundly modified the basic conceptions of chemistry. It has led to a great extension of our views concerning the real nature of the chemical elements. The discovery of the electron, the production of helium in the radioactive disintegration of atoms, the recognition of the existence of isotopes, the possibility that all elementary atoms are composed either of helium atoms or of atoms of hydrogen and helium, and that these atoms, in their turn, are built up of two constituents, one of which is the electron, a particle of

negative electricity whose mass is only 1/1800 of that of an atom of hydrogen, and the other a particle of positive electricity whose mass is practically identical with that of the same atom—the outcome, in short, of the collective work of Soddy, Rutherford, J. J. Thomson, Collie, Moseley, and others—are pregnant facts which have completely altered the fundamental aspects of the science. Chemical philosophy has, in fact, now definitely entered on a new phase.

Looking back over the past, some indications of the coming change might have been perceived wholly unconnected, of course, with the recent experimental work which has served to ratify it. In a short paper entitled 'Speculative Ideas respecting the Constitution of Matter,' originally published in 1863, Graham conceived that the various kinds of matter, now recognised as different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. This idea, in its essence, may be said to be as old as the time of Leucippus. To Graham as to Leucippus 'the action of the atom as one substance taking various forms by combinations unlimited, was enough to account for all the phenomena of the world. By separation and union with constant motion all things could be done.' But Graham developed the conception by independent thought, and in the light of experimentally ascertained knowledge which the world owes to his labours. He might have been cognisant of the speculations of the Greeks, but there is no evidence that he was knowingly influenced by them. In his paper Graham uses the terms atom and molecule if not exactly in the same sense that modern teaching demands, yet very different from that hitherto required by the limitations of contemporary chemical doctrine. He conceives of a lower order of atoms than the chemical atom of Dalton, and founds on his conception an explanation of chemical combination based upon a fixed combining measure, which he terms the *metron*, its relative weight being one for hydrogen, sixteen for oxygen, and so on with the other so-called 'elements.' Graham, in fact, like Davy before him, never committed himself to a belief in the indivisibility of the Daltonian atom. The original atom may, he thought, be far down.

The idea of a primordial ylé, or of the essential unity of matter, has persisted throughout the ages, and, in spite of much experimental work, some of it of the highest order, which was thought to have demolished it, it has survived, revivified and supported by analogies and arguments drawn from every field of natural inquiry. This idea of course was at the basis of the hypothesis of Prout, but which, even as modified by Dumas, was held to be refuted by the monumental work of Stas. But, as pointed out by Marignac and Dumas, anyone who will impartially look at the facts can hardly escape the feeling that there must be some reason for the frequent recurrence of atomic weights differing by so little

from the numbers required by the law which the work of Stas was supposed to disprove. The more exact study within recent years of the methods of determining atomic weights, the great improvement in experimental appliances and technique, combined with a more rigorous standard of accuracy demanded by a general recognition of the far-reaching importance of an exact knowledge of these physical constants, has resulted in intensifying the belief that some natural law must be at the basis of the fact that so many of the most carefully determined atomic weights on the oxygen standard are whole numbers. Nevertheless there were well-authenticated exceptions which seemed to invalidate its universality. The proved fact that a so-called element may be a mixture of isotopes—substances of the same chemical attributes but of varying atomic weight—has thrown new light on the question. It is now recognised that the fractional values independently established in the case of any one element by the most accurate experimental work of various investigators are, in effect, 'statistical quantities' dependent upon a mixture of isotopes. This result, indeed, is a necessary corollary of modern conceptions of the inner mechanism of the atom. The theory that all elementary atoms are composed of helium atoms, or of helium and hydrogen atoms, may be regarded as an extension of Prout's hypothesis, with, however, this important distinction, that whereas Prout's hypothesis was at best a surmise, with little, and that little only weak, experimental evidence to support it, the new theory is directly deduced from well-established facts. The hydrogen isotope H_3, first detected by J. J. Thomson, of which the existence has been confirmed by Aston, would seem to be an integral part of atomic structure. Rutherford, by the disruption of oxygen and nitrogen, has also isolated a substance of mass 3 which enters into the structure of atomic nuclei, but which he regards as an isotope of helium, which itself is built up of four hydrogen nuclei together with two cementing electrons. The atomic nuclei of elements of even atomic number would appear to be composed of helium nuclei only, or of helium nuclei with cementing electrons; whereas those of elements of odd atomic number are made up of helium and hydrogen nuclei together with cementing electrons. In the case of the lighter elements of the latter class the number of hydrogen nuclei associated with the helium nuclei is invariably three, except in that of nitrogen where it is two. The frequent occurrence of this group of three hydrogen nuclei indicates that it is structurally an isotope of hydrogen with an atomic weight of three and a nuclear charge of one. It is surmised that it is identical with the hypothetical 'nebulium' from which our 'elements' are held by astro-physicists to be originally produced in the stars through hydrogen and helium.

These results are of extraordinary interest as bearing on the question

of the essential unity of matter and the mode of genesis of the elements. Members of the British Association may recall the suggestive address on this subject of the late Sir William Crookes, delivered to the Chemical Section at the Birmingham meeting of 1886, in which he questioned whether there is absolute uniformity in the mass of the atoms of a chemical element, as postulated by Dalton. He thought, with Marignac and Schutzenberger, who had previously raised the same doubt, that it was not improbable that what we term an atomic weight merely represents a mean value around which the actual weights of the atoms vary within narrow limits, or, in other words, that the mean mass is 'a statistical constant of great stability.' No valid experimental evidence in support of this surmise was or could be offered at the time it was uttered. Maxwell pointed out that the phenomena of gaseous diffusion, as then ascertained, would seem to negative the supposition. If hydrogen, for example, were composed of atoms of varying mass it should be possible to separate the lighter from the heavier atoms by diffusion through a porous septum. 'As no chemist,' said Maxwell, 'has yet obtained specimens of hydrogen differing in this way from other specimens, we conclude that all the molecules of hydrogen are of sensibly the same mass, and not merely that their mean mass is a statistical constant of great stability.'[1] But against this it may be doubted whether any chemist had ever made experiments sufficiently precise to solve this point.

The work of Sir Norman Lockyer on the spectroscopic evidence for the dissociation of 'elementary' matter at transcendental temperatures, and the possible synthetic intro-stellar production of elements, through the helium of which he originally detected the existence, will also find its due place in the history of this new philosophy.

Sir J. J. Thomson was the first to afford direct evidence that the atoms of an element, if not exactly of the same mass, were at least approximately so, by his method of analysis of positive rays. By an extension of this method Mr. F. W. Aston has succeeded in showing that a number of elements are in reality mixtures of isotopes. It has been proved, for example, that neon, which has a mean atomic weight of about 20.2, consists of two isotopes having the atomic weights respectively of 20 and 22, mixed in the proportion of 90 per cent. of the former with 10 per cent. of the latter. By fractional diffusion through a porous septum an apparent difference of density of 0.7 per cent. between the lightest and heaviest fractions was obtained. The kind of experiment which Maxwell imagined proved the invariability of the hydrogen atom has sufficed to show the converse in the case of neon.

[1] Clerk-Maxwell, Art. 'Atom,' *Ency. Brit.* 9th Ed.

The element chlorine has had its atomic weight repeatedly determined, and, for special reasons, with the highest attainable accuracy. On the oxygen standard it is 35.46, and this value is accurate to the second decimal place. All attempts to prove that it is a whole number—35 or 36—have failed. When, however, the gas is analysed by the same method as that used in the case of neon it is found to consist of at least two isotopes of relative mass 35 and 37. There is no evidence whatever of an individual substance having the atomic weight 35.46. Hence chlorine is to be regarded as a complex element consisting of two principal isotopes of atomic weights 35 and 37 present in such proportion as to afford the mean mass 35.46. The atomic weight of chlorine has been so frequently determined by various observers and by various methods with practically identical results that it seems difficult to believe that it consists of isotopes present in definite and invariable proportion. Mr. Aston meets this objection by pointing out that all the accurate determinations have been made with chlorine derived originally from the same source, the sea, which has been perfectly mixed for æons. If samples of the element could be obtained from some other original source it is possible that other values of atomic weight would be obtained, exactly as in the case of lead in which the existence of isotopes in the metal found in various radioactive minerals was first conclusively established.

Argon, which has an atomic weight of 39.88, was found to consist mainly of an isotope having an atomic weight of 40, associated to the extent of about 3 per cent., with an isotope of atomic weight 36. Krypton and xenon are far more complex. The former would appear to consist of six isotopes, 78, 80, 82, 83, 84, 86; the latter of five isotopes, 129, 131, 132, 134, 136.

Fluorine is a simple element of atomic weight 19. Bromine consists of equal quantities of two isotopes, 79 and 81. Iodine, on the contrary, would appear to be a simple element of atomic weight 127. The case of tellurium is of special interest in view of its periodic relation to iodine, but the results of its examination up to the present are indefinite.

Boron and silicon are complex elements, each consisting of two isotopes, 10 and 11, and 28 and 29, respectively.

Sulphur, phosphorus, and arsenic are apparently simple elements. Their accepted atomic weights are practically integers.

All this work is so recent that there has been little opportunity, as yet, of extending it to any considerable number of the metallic elements. These, as will be obvious from the nature of the methods employed, present special difficulties. It is, however, highly probable that mercury is a mixed element consisting of many isotopes. These have been partially separated by Brönsted and Hervesy by fractional

distillation at very low pressures, and have been shown to vary very slightly in density. Lithium is found to consist of two isotopes, 6 and 7. Sodium is simple, potassium and rubidium are complex, each of the two latter elements consisting, apparently, of two isotopes. The accepted atomic weight of cæsium, 132.81, would indicate complexity, but the mass spectrum shows only one line at 133. Should this be confirmed cæsium would afford an excellent test case. The accepted value for the atomic weight is sufficiently far removed from a whole number to render further investigation desirable.

This imperfect summary of Mr. Aston's work is mainly based upon the account he recently gave to the Chemical Society. At the close of his lecture he pointed out the significance of the results in relation to the Periodic Law. It is clear that the order of the chemical or 'mean' atomic weights in the Periodic table has no practical significance; anomalous cases such as argon and potassium are simply due to the relative proportions of their heavier and lighter isotopes. This does not necessarily invalidate or even weaken the Periodic Law which still remains the expression of a great natural truth. That the expression as Mendeléeff left it is imperfect has long been recognised. The new light we have now gained has gone far to clear up much that was anomalous, especially Moseley's discovery that the real sequence is the atomic number, not the atomic weight. This is one more illustration of the fact that science advances by additions to its beliefs rather than by fundamental or revolutionary changes in them.

The bearing of the electronic theory of matter, too, on Prout's discarded hypothesis that the atoms of all elements were themselves built up of a primordial atom—his *protyle* which he regarded as probably identical with hydrogen—is too obvious to need pointing out. In a sense Prout's hypothesis may be said to be now re-established, but with this essential modification—the primordial atoms he imagined are complex and are of two kinds—atoms of positive and negative electricity—respectively known as protons and electrons. These, in Mr. Aston's words, are the standard bricks that Nature employs in her operations of element building.

The true value of any theory consists in its comprehensiveness and sufficiency. As applied to chemistry, this theory of 'the inner mechanism of the atom' must explain all its phenomena. We owe to Sir J. J. Thomson its extension to the explanation of the Periodic Law, the atomic number of an element, and of that varying power of chemical combination in an element we term valency. This explanation I give substantially in his own words. The number of electrons in an atom of the different elements has now been determined, and has been found to be equal to the atomic number of the element, that is to the position which the element occupies in the series when the elements are

arranged in the order of their atomic weights. We know now the nature and quantity of the materials of which the atoms are made up. The properties of the atom will depend not only upon these factors but also upon the way in which the electrons are arranged in the atom. This arrangement will depend on the forces between the electrons themselves and also on those between the electrons and the positive charges or protons. One arrangement which naturally suggested itself is that the positive charges should be at the centre with the negative electrons around it on the surface of a sphere. Mathematical investigation shows that this is a possible arrangement if the electrons on the sphere are not too crowded. The mutual repulsion of the electrons resents overcrowding, and Sir J. J. Thomson has shown that when there are more than a certain number of electrons on the sphere, the attraction of a positive charge, limited as in the case of the atom in magnitude to the sum of the charges on the electrons, is not able to keep the electrons in stable equilibrium on the sphere, the layer of electrons explodes and a new arrangement is formed. The number of electrons which can be accommodated on the outer layer will depend upon the law of force between the positive charge and the electrons. Sir J. J. Thomson has shown that this number will be eight with a law of force of a simple type.

To show the bearing of this result as affording an explanation of the Periodic Law, let us, to begin with, take the case of the atom of lithium, which is supposed to have one electron in the outer layer. As each element has one more free electron in its atom than its predecessor, glucinum, the element next in succession to lithium, will have two electrons in the outer layer of its atom, boron will have three, carbon four, nitrogen five, oxygen six, fluorine seven and neon eight. As there cannot be more than eight electrons in the outer layer, the additional electron in the atom of the next element, sodium, cannot find room in the same layer as the other electrons, but will go outside, and thus the atom of sodium, like that of lithium, will have one electron in its outer layer. The additional electron, in the atom of the next element, magnesium, will join this, and the atom of magnesium, like that of glucinum, will have two electrons in the outer layer. Again, aluminium, like boron, will have three; silicon, like carbon, four; phosphorus, like nitrogen, five; sulphur, like oxygen, six; chlorine, like fluorine, seven; and argon, like neon, eight. The sequence will then begin again. Thus the number of electrons, one, two, three, up to eight in the outer layer of the atom, will recur periodically as we proceed from one element to another in the order of their atomic weights, so that any property of an element which depends on the number of electrons in the outer layer of its atom will also recur periodically, which is precisely that remarkable property of the elements which is expressed

by the Periodic Law of Mendeléeff, or the Law of Octaves of Newlands.

The valency of the elements, like their periodicity, is a consequence of the principle that equilibrium becomes unstable when there are more than eight electrons in the outer layer of the atom. For on this view the chemical combination between two atoms, A and B, consists in the electrons of A getting linked up with those of B. Consider an atom like that of neon, which has already eight electrons in its outer layer; it cannot find room for any more, so that no atoms can be linked to it, and thus it cannot form any compounds. Now take an atom of fluorine, which has seven electrons in its outer layer; it can find room for one, but only one, electron, so that it can unite with one, but not with more than one, atom of an element like hydrogen, which has one electron in the outer layer. Fluorine, accordingly, is monovalent. The oxygen atom has six electrons; it has, therefore, room for two more, and so can link up with two atoms of hydrogen: hence oxygen is divalent. Similarly nitrogen, which has five electrons and three vacant places, will be trivalent, and so on. On this view an element should have two valencies, the sum of the two being equal to eight. Thus, to take oxygen as an example, it has only two vacant places, and so can only find room for the electrons of two atoms; it has, however, six electrons available for filling up the vacant places in other atoms, and as there is only one vacancy to be filled in a fluorine atom the electrons in an oxygen atom could fill up the vacancies in six fluorine atoms, and thereby attach these atoms to it. A fluoride of oxygen of this composition remains to be discovered, but its analogue, SF_6, first made known by Moissan, is a compound of this type. The existence of two valencies for an element is in accordance with views put forward some time ago by Abegg and Bödlander. Professor Lewis and Mr. Irving Longmuir have developed, with great ingenuity and success, the consequences which follow from the hypothesis that an octet of electrons surrounds the atoms in chemical compounds.

The term ‘atomic weight’ has thus acquired for the chemist an altogether new and much wider significance. It has long been recognised that it has a far deeper import than as a constant useful in chemical arithmetic. For the ordinary purposes of quantitative analysis, of technology, and of trade, these constants may be said to be now known with supreme importance. Their determination and study must now be of the essential nature of matter and on the ‘superlatively grand question, What is the inner mechanism of the atom?’ they become of supreme importance Their determination and study must now be approached from entirely new standpoints and by the conjoint action of chemists and physicists. The existence of isotopes has enormously widened the horizon. At first sight it would appear that we should

require to know as many atomic weights as there are isotopes, and the chemist may well be appalled at such a prospect. All sorts of difficulties start up to affright him, such as the present impossibility of isolating isotopes in a state of individuality, their possible instability, and the inability of his quantitative methods to establish accurately the relatively small differences to be anticipated. All this would seem to make for complexity. On the other hand, it may eventually tend towards simplification. If, with the aid of the physicist we can unravel the nature and configuration of the atom of any particular element, determine the number and relative arrangement of the constituent protons and electrons, it may be possible to arrive at the atomic weight by simple calculation, on the assumption that the integer rule is mathematically valid. This, however, is almost certainly not the case, owing to the influence of 'packing.' The little differences, in fact, may make all the difference. The case is analogous to that of the so-called gaseous laws in which the departures from their mathematical expression have been the means of elucidating the physical constitution of the gases and of throwing light upon such variations in their behaviour as have been observed to occur. There would appear, therefore, ample scope for the chemist in determining with the highest attainable accuracy the departures from the whole-number rule, since it is evident that much depends upon their exact extent.

These considerations have already engaged the attention of chemists. For some years past, a small International Committee, originally appointed in 1903, has made and published an annual report in which it has noted such determinations of atomic weight as have been made during the year preceding each report, and it has from time to time made suggestions for the amendment of the Tables of Atomic Weights, published in text-books and chemical journals, and in use in chemical laboratories. In view of recent developments, the time has now arrived when the work of this International Committee must be reorganised and its aims and functions extended. The mode in which this should be done has been discussed at the meeting in Brussels, in June last, of the International Union of Chemistry Pure and Applied, and has resulted in strengthening the constitution of the Committee and in a wide extension of its scope.

The crisis through which we have recently passed has had a profound effect upon the world. The spectacle of the most cultured and most highly developed peoples on this earth, armed with every offensive appliance which science and the inventive skill and ingenuity of men could suggest, in the throes of a death struggle must have made the angels weep. That dreadful harvest of death is past, but the aftermath remains. Some of it is evil, and the evil will persist for, it may be

generations. There is, however, an element of good in it, and the good, we trust, will develop and increase with increase of years. The whole complexion of the world—material, social, economic, political, moral, spiritual—has been changed, in certain aspects immediately for the worse, in others prospectively for the better. It behoves us, then, as a nation to pay heed to the lessons of the War.

The theme is far too complicated to be treated adequately within the limits of such an address as this. But there are some aspects of it germane to the objects of this Association, and I venture, therefore, in the time that remains to me, to bring them to your notice.

The Great War differed from all previous internecine struggles in the extent to which organised science was invoked and systematically applied in its prosecution. In its later phases, indeed, success became largely a question as to which of the great contending parties could most rapidly and most effectively bring its resources to their aid. The chief protagonists had been in the forefront of scientific progress for centuries, and had an accumulated experience of the manifold applications of science in practically every department of human activity that could have any possible relation to the conduct of war. The military class in every country is probably the most conservative of all the professions and the slowest to depart from tradition. But when nations are at grips, and they realise that their very existence is threatened, every agency that may tend to cripple the adversary is apt to be resorted to—no matter how far it departs from the customs and conventions of war. This is more certain to be the case if the struggle is protracted. We have witnessed this fact in the course of the late War. Those who, realising that in the present imperfect stage of civilisation wars are inevitable, and yet strove to minimise their horrors, and who formulated the Hague Convention of 1899, were well aware how these horrors might be enormously intensified by the applications of scientific knowledge, and especially of chemistry. Nothing shocked the conscience of the civilised world more than Germany's cynical disregard of the undertaking into which she had entered with other nations in regard, for instance, to the use of lethal gas in warfare. The nation that treacherously violated the Treaty of Belgium, and even applauded the action, might be expected to have no scruples in repudiating her obligations under the Hague Convention. April 25, 1915, which saw the clouds of the asphyxiating chlorine slowly wafted from the German trenches towards the lines of the Allies, witnessed one of the most bestial episodes in the history of the Great War. The world stood aghast at such a spectacle of barbarism. German *kultur* apparently had absolutely no ethical value. Poisoned weapons are employed by savages, and noxious gas had been used in Eastern warfare in early times, but its use was hitherto unknown among European nations.

How it originated among the Germans—whether by the direct unprompted action of the Higher Command, or, as is more probable, at the instance of persons connected with the great manufacturing concerns in Rhineland—has, so far as I know, not transpired. It was not so used in the earlier stages of the War, even when it had become a war of position. It is notorious that the great chemical manufacturing establishments of Germany had been, for years previously, sedulously linked up in the service of the war which Germany was deliberately planning—probably, in the first instance, mainly for the supply of munitions and medicaments. We may suppose that it was the tenacity of our troops, and the failure of repeated attempts to dislodge them by direct attack, that led to the employment of such foul methods. Be this as it may, these methods became part of the settled practice of our enemies, and during the three succeeding years, that is from April 1915 to September 1918, no fewer than eighteen different forms of poison—gases, liquids, and solids—were employed by the Germans. On the principle of Vespasian's law, reprisals became inevitable, and for the greater part of three years we had the sorry spectacle of the leading nations of the world flinging the most deadly products at one another that chemical knowledge could suggest and technical skill contrive. Warfare, it would seem, has now definitely entered upon a new phase. The horrors which the Hague Convention saw were imminent, and from which they strove to protect humanity, are now, apparently, by the example and initiative of Germany, to become part of the established procedure of war. Civilisation protests against a step so retrograde. Surely comity among nations should be adequate to arrest it. If the League of Nations is vested with any real power, it should be possible for it to devise the means, and to ensure their successful application. The failure of the Hague Convention is no sufficient reason for despair. The moral sense of the civilised world is not so dulled but that, if roused, it can make its influence prevail. And steps should be taken without delay to make that influence supreme, and all the more so that there are agencies at work which would seek to perpetuate such methods as a recognised procedure of war. The case for what is called chemical warfare has not wanted for advocates. It is argued that poison gas is far less fatal and far less cruel than any other instrument of war. It has been stated that 'amongst the "mustard gas" casualties the deaths were less than 2 per cent., and when death did not ensue complete recovery generally ultimately resulted. . . . Other materials of chemical warfare in use at the Armistice do not kill at all; they produce casualties which, after six weeks in hospital, are discharged practically without permanent hurt.' It has been argued that, as a method of conducting war, poison-gas is more humane than preventive medicine. Preventive medicine has increased the unit dimension of an

army, free from epidemic and communicable disease, from 100,000 men to a million. 'Preventive medicine has made it possible to maintain 20,000,000 men under arms and abnormally free from disease, and so provided greater scope for the killing activities of the other military weapons. . . . Whilst the surprise effects of chemical warfare aroused anger as being contrary to military tradition, they were minute compared with those of preventive medicine. The former slew its thousands, whilst the latter slew its millions and is still reaping the harvest.' This argument carries no conviction. Poison gas is not merely contrary to European military tradition; it is repugnant to the right feeling of civilised humanity. It in no wise displaces or supplants existing instruments of war, but creates a new kind of weapon, of limitless power and deadliness. 'Mustard gas' may be a comparatively innocuous product as lethal substances go. It certainly was not intended to be such by our enemies. Nor, presumably, were the Allies any more considerate when they retaliated with it. Its effects, indeed, were sufficiently terrible to destroy the German *moral.* The knowledge that the Allies were preparing to employ it to an almost boundless extent was one of the factors that determined our enemies to sue for the Armistice. But if poisonous chemicals are henceforth to be regarded as a regular means of offence in warfare, is it at all likely that their use will be confined to 'mustard gas,' or indeed to any other of the various substances which were employed up to the date of the Armistice? To one who, after the peace, inquired in Germany concerning the German methods of making 'mustard gas,' the reply was:—'Why are you worrying about this when you know perfectly well that this is not the gas we shall use in the next war?'

I hold no brief for preventive medicine, which is well able to fight its own case. I would only say that it is the legitimate business of preventive medicine to preserve by all known means the health of any body of men, however large or small, committed to its care. It is not to its discredit if, by knowledge and skill, the numbers so maintained run into millions instead of being limited to thousands. On the other hand, 'an educated public opinion' will refuse to give credit to any body of scientific men who employ their talents in devising means to develop and perpetuate a mode of warfare which is abhorrent to the higher instincts of humanity.

This Association, I trust, will set its face against the continued degradation of science in thus augmenting the horrors of war. It could have no loftier task than to use its great influence in arresting a course which is the very negation of civilisation.

SECTIONAL ADDRESSES.

PROBLEMS OF PHYSICS.

ADDRESS TO SECTION A (MATHEMATICS AND PHYSICS) BY

PROFESSOR O. W. RICHARDSON, D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

My predecessor in office a year ago reminded you that the theoretical researches of Einstein and Weyl suggest that not merely the material universe but space itself is perhaps finite. As to the probabilities I do not wish to express an opinion; but the statement is significant of the extent of the revolution in the conceptions and fundamental principles of physics now in progress. That space need not be infinite has, I believe, long been recognised by geometricians, and appropriate geometries to meet its possible limitations have been devised by ingenious mathematicians. I doubt, however, whether these inventive gentlemen ever dreamed that their schemes held any objective validity such as would assist the astronomer and the physicist in understanding and classifying material phenomena. It is not certain that they will; but the possibility is definite. Apart from this, the whole development of relativity is an extraordinary triumph for pure mathematics. Had Einstein not found his entire calculus ready to hand, owing to the purely mathematical work of Christoffel, Riemann, and others, it seems certain that the development of generalised relativity would have been much slower. It is a pleasure to be able to acknowledge this indebtedness of physics and astronomy to pure mathematics.

Relativity is the revolutionary movement in physics which has caught the public eye, perhaps because it deals with familiar conceptions in a manner which for the most part is found pleasantly incomprehensible. But it is only one of a number of revolutionary changes of comparable magnitude. Among these we have to place the advent of the quantum, the significance of which I hope we shall thoroughly discuss early next week. The various consequences of the electronic structure of matter are still unfolding themselves to us, and are increasing our insight into the most varied phenomena at a rate which must have appeared incredible only a few decades ago.

The enormous and far-reaching importance of the discoveries being made at Cambridge by Sir Ernest Rutherford cannot be over-emphasised. These epoch-making discoveries relate to the structure and properties of the nuclei of atoms. At the present time we have, I think, to accept it as a fact that the atoms consist of a positively charged nucleus of minute size, surrounded at a fairly respectful distance

by the number of electrons requisite to maintain the structure electrically neutral. The nucleus contains all but about one-two-thousandth part of the mass of the atom, and its electric charge is numerically equal to that of the negative electron multiplied by what is called the atomic number of the atom, the atomic number being the number which is obtained when the chemical elements are enumerated in the order of the atomic weights; thus hydrogen = 1, helium = 2, lithium = 3, and so on. Consequently the number of external electrons in the atom is also equal to the atomic number. The evidence, derived from many distinct and dissimilar lines of inquiry, which makes it necessary to accept the foregoing statements as facts, will be familiar to members of this Section of the British Association, which has continually been in the forefront of contemporary advances in physical science. But I would remind you in passing that one of the important pieces of evidence was supplied by Professor Barkla's researches on the scattering of X-rays by light atoms.

The diameters of the nuclei of the atoms are comparable with one-millionth of one-millionth part of a centimetre, and the problem of finding what lies within the interior of such a structure seems at first sight almost hopeless. It is to this problem which Rutherford has addressed himself by the direct method of bombarding the nuclei of the different atoms with the equally minute high-velocity helium nuclei (alpha-particles) given off by radioactive substances, and examining the tracks of any other particles which may be generated as a result of the impact. A careful and critical examination of the results shows that hydrogen nuclei are thus expelled from the nuclei of a number of atoms such as nitrogen and phosphorus. On the other hand, oxygen and carbon do not eject hydrogen under these circumstances, although there is evidence in the case of oxygen and nitrogen of the expulsion ? of other sub-nuclei whose precise structure is a matter for further inquiry. See Millikan

The artificial transmutation of the chemical elements is thus an established fact. The natural transmutation has, of course, been familiar for some years to students of radioactivity. The philosopher's stone, one of the alleged chimeras of the mediæval alchemists, is thus within our reach. But this is only part of the story. It appears that in some cases the kinetic energy of the ejected fragments is greater than that of the bombarding particles. This means that these bombardments are able to release the energy which is stored in the nuclei of atoms. Now, we know from the amount of heat liberated in radioactive disintegration that the amount of energy stored in the nuclei is of a higher order of magnitude altogether, some millions of times greater, in fact, than that generated by any chemical reaction such as the combustion of coal. In this comparison, of course, it is the amount of energy per unit mass of reacting or disintegrating matter which is under consideration. The amounts of energy which have thus far been released by artificial disintegration of the nuclei are in themselves small, but they are enormous in comparison with the minute amounts of matter affected. If these effects can be sufficiently intensified there appear to be two possibilities. Either they will prove

uncontrollable, which would presumably spell the end of all things,[1] or they will not. If they can be both intensified and controlled then we shall have at our disposal an almost illimitable supply of power which will entirely transcend anything hitherto known. It is too early yet to say whether the necessary conditions are capable of being realised in practice, but I see no elements in the problem which would justify us in denying the possibility of this. It may be that we are at the beginning of a new age, which will be referred to as the age of sub-atomic power. We cannot say; time alone will tell.

Thermionic Emission.

With your permission, I will now descend a little way from the summit of Mount Olympus, and devote the rest of my address to a sober review of the present state of some of the questions with which my own thoughts have been more particularly occupied. At the Manchester meeting of the Association in 1915 I had the privilege of opening a discussion on thermionic emission—that is to say, the emission of electrons and ions by incandescent bodies. I recall that the opinion was expressed by some of the speakers that these phenomena had a chemical origin. That view, I venture to think, is one which would find very few supporters now. It is not that any new body of fact has arisen in the meantime. The important facts were all established before that time, but they were insufficiently appreciated, and their decisiveness was inadequately realised.

It may be worth while to revert for a moment to the issues in that controversy, already moribund in 1915, because it has been closely paralleled by similar controversies relating to two other groups of phenomena—namely, photoelectric emission and contact electromotive force—which, as we shall see, are intimately connected with thermionic emission. The issue was not as to whether thermionic emission may be looked upon simply as a type of chemical reaction. Such an issue would have been largely a matter of nomenclature. Thermionic electron emission has many features in common with a typical reversible chemical reaction such as the dissociation of calcium carbonate into lime and carbon dioxide. There is a good deal to be said for the point of view which regards thermionic emission as an example of the simplest kind of reversible chemical action, namely, that kind which consists in the dissociation of a neutral atom into a positive residue and a negative electron, inasmuch as we know that the negative electron is one of the really fundamental elements out of which matter is built up. The issue in debate was, however, of a different character. It was suggested that the phenomenon was not primarily an emission of electrons from the metallic or other source, but was a secondary phenomenon, a kind of by-product of an action which was primarily a chemical reaction between the source of electrons and some other material substance such as the highly attenuated

[1] To reassure the nervous I would, however, interpolate the comforting thought that this planet has held considerable quantities of radioactive matter for a very long time without anything very serious happening so far as we know.

gaseous atmosphere which surrounded it. This suggestion carried with it either implicitly or explicitly the view that the source of power behind the emission was not the thermal energy of the source, but was the chemical energy of the postulated reactions.

This type of view has never had any success in elucidating the phenomena, and I do not feel it necessary at this date to weary you with a recital of the facts which run entirely counter to it, and, in fact, definitely exclude it as a possibility. They have been set forth at length elsewhere on more than one occasion. I shall take it to be established that the phenomenon is physical in its origin and reversible in its operation.

Establishing the primary character of the phenomenon does not, however, determine its nature or its immediate cause. Originally I regarded it as simply kinetic, a manifestation of the fact that as the temperature rose the kinetic energy of some of the electrons would begin to exceed the work of the forces by which they are attracted to the parent substance. With this statement there is, I think, no room for anyone to quarrel, but it is permissible to inquire how the escaping electrons obtain the necessary energy. One answer is that the electrons have it already in the interior of the substance by virtue of their energy of thermal agitation. But thermal agitations now appear less simple than they used to be regarded, and in any event they do not exhaust the possibilities.

We know that when light of short enough wave-length falls on matter it causes the ejection of electrons from it—the so-called photoelectric effect. Since the formula for the radiation emitted by a body at any given temperature contains every wave-length without limitation, there must be some emission of electrons from an incandescent body as the result of the photoelectric effect of its own luminosity. Two questions obviously put themselves. Will this photoelectric emission caused by the whole spectrum of the hot body vary as the temperature of the incandescent body is raised in the way which is known to characterise thermionic emission? A straightforward thermodynamic calculation shows that this is to be expected from the theoretical standpoint, and the anticipation has been confirmed by the experiments of Professor W. Wilson. Thus the autophotoelectric emission has the correct behaviour to account for the thermionic emission. The other question is: Is it large enough? This is a question of fact. I have considered the data very carefully. There is a little uncertainty in some of the items, but when every allowance is made there seems no escape from the conclusion that the photoelectric effect of the whole spectrum is far too small to account for thermionic emission.

This question is an important one, apart from the particular case of thermionic emission. The same dilemma is met with when we seek for the actual *modus operandi* of evaporation, chemical action, and a number of other phenomena. These, so far as we know, might be fundamentally either kinetic or photochemical or a mixture of both. In my judgment the last of these particular alternatives is the most probable. (I am using the term photochemical here in the wide sense

of an effect of light in changing the composition of matter, whether the parts affected are atoms, groups of atoms, ions, or electrons.) For example, the approximation about boiling points known as Trouton's rule is a fairly obvious deduction from the photochemical standpoint. The photochemical point of view has recently been put very strongly by Perrin, who would make it the entire *motif* of all chemical reaction, as well as of radioactivity and changes of state. In view of the rather minor part it seems to play in thermionic emission, where one would *a priori* have expected light to be especially effective, this is probably claiming too much for it, but the chemical evidence contains one item which is certainly difficult to comprehend from the kinetic standpoint. The speed of chemical decomposition of certain gases is independent of their volume, showing that the decomposition is not due to molecular collisions. The speed does, however, increase very rapidly with rising temperature. What the increased temperature can do except increase the number and intensity of the collisions, factors which the independence of volume at constant temperature show to be without effect, and increase the amount of radiation received by the molecules, is not too obvious. It seems, however, that, according to calculations by Langmuir,[2] the radiation theory does not get us out of this difficulty; for, just as in the ordinary photoelectric case, there is nothing like enough radiation to account for the observed effects. It seems that in the case of these mono-molecular reactions the phenomena cannot be accounted for either by simple collisions, or by radiation, or by a mixture of both, and it is necessary to fall back on the internal structure of the decomposing molecule. This is complex enough to afford material sufficient to cover the possibilities; but, from the standpoint of the temperature energy relations of its parts, it cannot at present be regarded as much more than a field for speculation.

Contact Electricity.

A controversy about the nature of the contact potential difference between two metals, similar to that to which I have referred in connection with thermionic emission, has existed for over a century. In 1792 Volta wrote: 'The metals . . . can by themselves, and of their own proper virtue, excite and dislodge the electric fluid from its state of rest.' The contrary position that the electrical manifestations are inseparably connected with chemical action was developed a few years later by Fabroni. Since that time electrical investigators have been fairly evenly divided between these two opposing camps. Among the supporters of the intrinsic or contact view of the type of Volta we may recall Davy, Helmholtz, and Kelvin. On the other side we have to place Maxwell, Lodge, and Ostwald. In 1862 we find Lord Kelvin[3] writing: 'For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is very near Volta's, which fell into discredit because Volta or his followers neglected the principle of the conservation of force.' On the other hand, in 1896 we find Ostwald[4] referring to Volta's views as

[2] *Journ. Am. Chem. Soc.*, vol. xlii., p. 2190 (1920).

[3] Papers on Electrostatics and Magnetism, p. 318.

[4] *Elektrochemie, Ihre Geschichte und Lehre*, p. 65, Leipzig (1896).

the origin of the most far-reaching error in electrochemistry, which the greatest part of the scientific work in that domain has been occupied in fighting almost ever since. These are cited merely as representative specimens of the opinions of the protagonists.

Now, there is a close connection between thermionic emission and contact potential difference, and I believe that a study of thermionic emission is going to settle this little dispute. In fact, I rather think it has already settled it, but before going into that matter I would like to explain how it is that there is a connection between thermionic emission and contact potential difference, and what the nature of that connection is.

Imagine a vacuous enclosure, either impervious to heat or maintained at a constant temperature. Let the enclosure contain two different electron-emitting bodies, A and B. Let one of these, say A, have the power of emitting electrons faster than the other, B. Since they are each receiving as well as emitting electrons, A will acquire a positive and B a negative charge under these circumstances. Owing to these opposite charges A and B will now attract each other, and useful work can be obtained by letting them come in contact. After the charges on A and B have been discharged by bringing them in contact, let the bodies be quickly separated and moved to their original positions. This need involve no expenditure of work, as the charges arising from the electron emission will not have had time to develop. After the charges have had time to develop the bodies can again be permitted to move together under their mutual attraction, and so the cycle can be continued an indefinite number of times. In this way we have succeeded in imagining a device which will convert all the heat energy from a source at a uniform temperature into useful work.

Now, the existence of such a device would contravene the second law of thermodynamics. We are therefore compelled either to deny the principles of thermodynamics or to admit that there is some fallacy as to the pretended facts in the foregoing argument. We do not need to hesitate between these alternatives, and we need only look to see how the alleged behaviour of A and B will need to be modified in order that no useful work may appear. There are two alternatives. Either A and B necessarily emit equal numbers (which may include the particular value zero) of electrons at all temperatures, or the charges which develop owing to the unequal rate of emission are not discharged, even to the slightest degree, when the two bodies are placed in contact.

The first alternative is definitely excluded by the experimental evidence, so I shall proceed to interpret the second. It means that bodies have natural states of electrification whereby they become charged to definite potential differences whose magnitudes are independent of their relative positions. There is an intrinsic potential difference between A and B which is the same, at a given temperature, whether they are at a distance apart or in contact. In the words of Volta, which I have already quoted, 'the metals can by themselves, and of their own proper virtue, excite and dislodge the electric fluid from its state of rest.'

Admitting that the intrinsic potentials exist, a straightforward calculation shows that they are intimately connected with the magnitudes of the thermionic emission at a given temperature. The relation is, in fact, governed by the following equation: If A and B denote the saturation thermionic currents per unit area of the bodies A and B respectively, and V is the contact potential difference between them at the absolute temperature T, then $V = \frac{kT}{e} \log \frac{A}{B}$ where k is the gas constant calculated for a single molecule (Boltzmann's constant), and e is the electronic charge.

I have recently, with the help of Mr. F. S. Robertson, obtained a good deal of new information on this question from the experimental side. We have made measurements of the contact potential difference between heated filaments and a surrounding metallic cylinder, both under the high-vacuum and gas-free conditions which are now attainable in such apparatus, and also when small known pressures of pure hydrogen are present. As is well known, both contact potentials and thermionic emission are very susceptible to minute traces of gas, but we find that under the best conditions as to freedom from gas there is a contact potential of the order of one volt between a pure tungsten filament and a thoriated filament. We also find that changes of a similar magnitude in the contact potential difference between a thoriated tungsten filament and a copper anode take place when the filament is heated. These changes are accompanied by simultaneous changes in the thermionic currents from the filament; and we find that the change in the contact potential calculated from change in the currents with the help of the foregoing equation is within about 20 per cent. of the measured value. Considering the experimental difficulties, this is a very substantial agreement. Whilst the evidence is not yet as complete as I hope to make it, it goes a long way towards disproving the chemical view of the origin of contact potential difference.

From what has been said you will realise that the connection between contact potentials and thermionic emissions is a very close one. I would, however, like to spend a moment in developing it from another angle. To account for the facts of thermionic emission it is necessary to assume that the potential energy of an electron in the space just outside the emitter is greater than that inside by a definite amount, which we may call w. The existence of this w, which measures the work done when an electron escapes from the emitter, is required by the electron-atomic structure of matter and of electricity. Its value can be deduced from the temperature variation of thermionic emission, and, more directly, from the latent heats absorbed or generated when electrons flow out of or into matter. These three methods give values of w which, allowing for the somewhat considerable experimental difficulties, are in fair agreement for any particular emitter. The data also show that in general different substances have different values of w. This being so, it is clear that when uncharged bodies are placed in contact the potential energies of the electrons in one will in general be different from those of the electrons in the other. If, as in the case of the metals, the electrons are able to move freely they will

so move until an electric field is set up which equilibrates this difference of potential energy. There will thus be an intrinsic or contact difference of potential between metals which is equivalent to the difference in the values of w and is equal to the difference in w divided by the electronic charge.[5]

Photoelectric Action.

We have seen that there is a connection on broad lines between thermionic emission and both contact potentials on the one hand and photoelectric emission on the other. The three groups of phenomena are also related in detail and to an extent which up to the present has not been completely explored. In order to understand the present position, let us review briefly some of the laws of photoelectric action as they have revealed themselves by experiments on the electrons emitted from metals when illuminated by visible and ultra-violet light.

Perhaps the most striking feature of photoelectric action is the existence of what has been called the threshold frequency. For each metal whose surface is in a definite state there is a definite frequency n_0, which may be said to determine the entire photoelectric behaviour of the metal. The basic property of the threshold frequency n_0 is this: When the metal is illuminated by light of frequency less than n no electrons are emitted, no matter how intense the light may be. On the other hand, illumination by the most feeble light of frequency greater than n_0 causes some emission. The frequency n_0 signalises a sharp and absolute discontinuity in the phenomena.

Now let us inquire as to the kinetic energy of the electrons which are emitted by a metal when illuminated by monochromatic light of frequency, let us say, n. Owing to the fact that the emitted electrons may originate from different depths in the metal, and may undergo collisions at irregular intervals, it is only the maximum kinetic energy of those which escape which we should expect to exhibit simple properties. As a matter of fact, it is found that the maximum kinetic energy is equal to the difference between the actual frequency n and the threshold frequency n_0 multiplied by Planck's constant h. In mathematical symbols, if v is the velocity of the fastest emitted electron, m its mass, e its charge, and V the opposing potential required to bring it to rest,

$$eV = \tfrac{1}{2}\, m\, v^2 = h\,(n - n_0).$$

From this equation we see that the threshold frequency has another property. It is evidently that frequency for which kinetic energy and stopping potential fall to zero. This suggests strongly, I think, that the reason the electron emission ceases at n_0 is that the electrons are not able to get enough energy from the light to escape from the metal, and not that they are unable to get any energy from the light.

The threshold frequencies have another simple property. If we measure the threshold frequencies for any pair of metals, and at the

[5] This statement is only approximately true. In order to condense the argument certain small effects connected with the Peltier effect at the junction between the metals have been left out of consideration.

same time we measure the contact difference of potential K between them, we find that K is equal to the difference between their threshold frequencies multiplied by this same constant h divided by the electronic charge e.

These results, as well as others which I have not time to enumerate, admit of a very simple interpretation if we assume that when illuminated by light of frequency n the electrons individually acquire an amount of energy hn. We have seen that in order to account for thermionic phenomena it is necessary to assume that the electrons have to do a certain amount of work w to get away from the emitter. There is no reason to suppose that photoelectrically emitted electrons can avoid this necessity. Let us suppose that this work is also definite for the photoelectric electrons and let us denote its value by hn_0. Then no electron will be able to escape from the metal until it is able to acquire an amount of energy at least equal to hn_0 from the light—that is to say, under the suppositions made—until n becomes at least as great as n_0. Thus n_0 will be identical with the frequency which we have called the threshold frequency, and the maximum energy of any electron after escaping will be $h\ (n-n_0)$.

The relation between threshold frequencies and contact potential difference raises another issue. We have seen that the contact potential difference between two metals must be very nearly equal to the difference between the amounts of work w for the electrons to get away from the two metals by thermionic action, divided by the electronic charge e. The photoelectric experiments show that the contact electromotive force is also nearly equal to the differences of the threshold frequencies multiplied by h/e. It follows that the photoelectric work hn_0 must be equal to the thermionic work w to the same degree of accuracy. We have to except here a possible constant difference between the two. I do not see, however, how any value other than zero for such a constant could be given a rational interpretation, as it would have to be the same for all substances and frequencies. The photoelectric and thermionic works are known to agree to within about one volt. To decide how far they are identical needs better experimental evidence than we have at present. The indirect evidence for their substantial identity is stronger at the moment than the direct evidence.

I do not think that the complete identity of the thermionic work w and the photoelectric hn_0 is a matter which can be inferred *a priori*. What we should expect depends to a considerable extent on the condition of the electrons in the interior of metals. We cannot pretend to any real knowledge of this at present; the various current theories are mere guesswork. Unless the electrons which escape all have the same energy when inside the metal we should expect the thermionic value to be an average taken over those which get out. The photoelectric value, on the other hand, should be the minimum pertaining to those internal electrons which have most energy. The apparent sharpness of the threshold frequency is also surprising from some points of view. There seems to be scope for a fuller experimental examination of these questions.

I have spoken of the threshold frequency as though it were a

perfectly definite quantity. No doubt it is when the condition of the body is or can be definitely specified, but it is extraordinarily sensitive to minute changes in the conditions of the surface, such as may be caused, for example, by the presence of extremely attenuated films of foreign matter. For this reason we should accept with a certain degree of reserve statements which appear from time to time that photoelectric action is some parasitic phenomenon, inasmuch as it can be made to disappear by improvement of vacuum or other change in the conditions. What has generally happened in these investigations is that something has been done to the illuminated surface which has raised its threshold frequency above that of the shortest wave-length in the light employed in the test. Unless they are accompanied by specific information about the changes which have taken place in the threshold frequency, such statements are of little value at the present stage of development of this subject.

Interesting calculations have been made by Frenkel which bring surface tension into close connection with the thermionic work w. Broadly speaking, there can be little doubt that a connection of this nature exists, but whether the relation is as simple as that given by the calculations is open to doubt. It should be possible to answer this question definitely when we have more precise information about the disposition of the electrons in atoms such as the continuous progress in X-ray investigation seems to promise.

Light and X-Rays.

One of the great achievements of experimental physics in recent years has been the demonstration of the essential unity of X-rays and ordinary light. X-rays have been shown to be merely light of particularly high frequency or short wave-length, the distinction between the two being one of degree rather than of kind. The foundations of our knowledge of X-ray phenomena were laid by Barkla, but the discovery and development of the crystal diffraction methods by v. Laue, the Braggs, Moseley, Duane, and de Broglie have established their relations with ordinary light so clearly that he who runs may read their substantial identity. The actual gap in the spectrum of the known radiations between light and X-rays is also rapidly disappearing. The longest stride into the region beyond the ultra-violet was made by Lyman with the vacuum grating spectroscope which he developed. For a short time Professor Bazzoni and I held the record in this direction with our determination of the short wave limit of the helium spectrum, which is in the neighbourhood of 450 Ångstrom units. More recently this has been passed by Millikan, who has mapped a number of lines extending to about 200 Ångstrom units—that is to say, more than four octaves above the violet limit of the visible spectrum. I am not sure what is the longest X-ray which has been measured, but I find a record of a Zinc L-ray by Friman[6] of a wave-length of 12.346 Ångstrom units. There is thus at most a matter of about four octaves

[6] *Phil. Mag.*, vol. xxxii., p. 494 (1916).

still to be explored. In approaching this unknown region from the violet end the most characteristic property of the radiations appears to be their intense absorption by practically every kind of matter. This result is not very surprising from the quantum standpoint. The quantum of these radiations is in excess of that which corresponds to the ionising potential of every known molecule, but it is of the same order of magnitude. Furthermore, it is large enough to reach not only the most superficial, but also a number of the deeper-seated electrons of the atoms. There is evidence, both theoretical and experimental, that the photoelectric absorption of radiation is most intense when its quantum exceeds the minimum quantum necessary to eject the absorbing electron but does not exceed it too much. In the simplest theoretical case the absorption is zero for radiations whose frequencies lie below the minimum quantum, rises to a maximum for a frequency comparable with the minimum, and falls off to zero again at infinite frequency. This case has not been realised in practice, but, broadly judged, the experimental data are in harmony with it. On these general grounds we should expect intense absorption by all kinds of matter for the radiation between the ultra-violet and the X-ray region.

The closeness of the similarity in the properties of X-rays and light is, I think, even yet inadequately realised. It is not merely a similarity along broad lines, but it extends to a remarkable degree of detail. It is perhaps most conspicuous in the domains of photoelectric action and of the inverse phenomenon of the excitation of radiation or spectral lines by electron impacts. Whilst there may still be room for doubt as to the precise interpretation of some of the experimental data, the impression I have formed is that each important advance tends to unify rather than to disintegrate these two important groups of phenomena.

THE LABORATORY OF THE LIVING ORGANISM.

ADDRESS TO SECTION B (CHEMISTRY) BY

M. O. FORSTER, D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

Many and various are the reasons which have been urged, at different periods of its history, for stimulating the study of chemistry. In recent years these have been either defensive or frankly utilitarian, in the latter feature recalling the less philosophic aspects of alchemy; moreover, it is to be feared that a substantial proportion of those who have lately hastened to prepare themselves for a chemical career have been actuated by this inducement. It is the duty, therefore, of those who speak with any degree of experience to declare that the only motive for pursuing chemistry which promises anything but profound disappointment is an affection for the subject sufficiently absorbing to displace the attraction of other pursuits. Even to the young chemist who embarks under this inspiration the prospect of success as recognised by the world is indeed slender, but, as his knowledge grows and the consequent appreciation of our ignorance widens, enthusiasm for the beauty and mystery of surrounding nature goes far in compensating for the disadvantage of his position. On the other hand, he who has been beguiled into embracing chemistry on the sole ground of believing it to be a ' good thing ' will either desert it expeditiously or almost surely starve and shower purple curses upon his advisers.

In one respect chemistry resembles measles—every boy and girl should have it, lest an attack in later life should prove more serious. Moreover, whilst it is not only unnecessary, but most undesirable, to present the subject as if every boy and girl were going to be a chemist, it is most important to present it in such a manner that every educated citizen may realise the intimate part which chemistry plays in his daily life. Not only do chemical principles underlie the operations of every industry, but every human being—indeed, every living plant and animal—is, during each moment of healthy life, a practical organic and physical chemist, conducting analytical and synthetical processes of the most complex order with imperturbable serenity. No other branch of knowledge can appeal for attention on comparable grounds; and without suggesting that we should all, individually, acquire sufficient chemical understanding fully to apprehend the changes which our bodies effect so punctually and so precisely—for this remains beyond the power of trained chemists—it may be claimed that an acquaintance with the general outlines of chemistry would add to the mental equipment of our people a source of abundant intellectual pleasure which is now unfairly denied them. We have been told that the world shall be made a fit place for heroes to live in; but is not the preliminary

to this ideal an exposition to those heroes of the wonder and beauty of the world which they already occupy, on the principle that if you cannot have what you like, it is elementary wisdom to like what you have? In following the customary practice of surveying matters of interest which have risen from our recent studies, therefore, it is the purpose of this address to emphasise also those æsthetic aspects of chemistry which offer ample justification for the labour which its pursuit involves.

What is breakfast to the average man? A hurried compromise between hunger and the newspaper. How does the chemist regard it? As a daily miracle which gains, rather than loses, freshness as the years proceed. For just think what happens. Before we reach the table frizzled bacon, contemplated or smelt, has actuated a wonderful chemical process in our bodies. The work of Pavlov has shown that if the dog has been accustomed to feed from a familiar bowl the sight of that bowl, even empty, liberates from the appropriate glands a saliva having the same chemical composition as that produced by snuffing the food. This mouth-watering process, an early experience of childhood, is known to the polite physiologist as a 'psychic reflex,' and the various forms assumed by psychic reflex, responding to the various excitations which arise in the daily life of a human being, must be regarded by the chemical philosopher as a series of demonstrations akin to those which he makes in the laboratory, but hopelessly inimitable with his present mental and material resources. For, extending this principle to the other chemical substances poured successively into the digestive tract, we have to recognise that the minute cells of which our bodies are co-ordinated assemblages possess and exercise a power of synthetic achievement contrasted with which the classical syntheses, occasionally enticing the modern organic chemist to outbursts of pride, are little more than hesitating preliminaries. Such products of the laboratory, elegant as they appear to us, represent only the fringe of this vast and absorbing subject. Carbohydrates, alkaloids, glucosides and purines, complex as they seem when viewed from the plane of their constituent elements, are but the molecular debris strewing the path of enzyme action and photochemical synthesis, whilst the enzymes produced in the cells, and applied by them in their ceaseless metamorphoses, are so far from having been synthesised by the chemist as to have not even yet been isolated in purified form, although their specific actions may be studied in the tissue-extracts containing them.

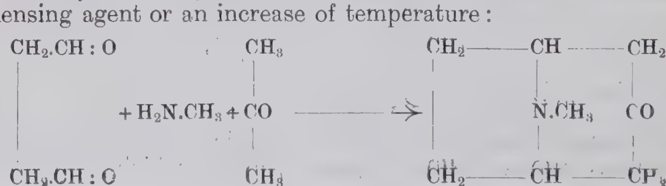
Reflect for a moment on the specific actions. The starch in our toast and porridge, the fat in our butter, the proteins in our bacon, all insoluble in water, by transformations otherwise unattainable in the laboratory are smoothly and rapidly rendered transmissible to the blood, which accepts the products of their disintegration with military precision. Even more amazing are the consequences. Remarkable as the foregoing analyses must appear, we can dimly follow their progress by comparison with those more violent disruptions of similar materials revealed to us by laboratory practice, enabling such masters of our craft as Emil Fischer to isolate the resultant individuals. Concurrently with such analyses, however, there proceed syntheses which we can scarcely

visualise, much less imitate. The perpetual elaboration of fatty acids from carbohydrates, of proteins from amino-acids, of zymogens and hormones as practised by the living body are beyond the present comprehension of the biochemist; but their recognition is his delight, and the hope of ultimately realising such marvels provides the dazzling goal towards which his efforts are directed.

The Vegetable Alkaloids.

The joyous contemplation of these wonders is an inalienable reward of chemical study, but it is denied to the vast majority of our people. The movements of currency exchange, to which the attention of the public has been directed continuously for several years, are clumsy contortions compared with the chemical transformations arising from food exchange. It should not be impossible to bring the skeleton of these transformations within the mental horizon of those who take pleasure in study and reflection; and to those also the distinction between plants and animals should be at least intelligible. The wonderful power which plants exercise in building up their tissues from carbonic acid, water and nitrogen, contrasted with the powerlessness of animals to utilise these building materials until they have been already assembled by plants, is a phenomenon too fundamental and illuminating to be withheld, as it now is, from all but the few. For by its operation the delicate green carpet, which we all delight in following through the annual process of covering the fields with golden corn, is accomplishing throughout the summer months a vast chemical synthesis of starch for our benefit. Through the tiny pores in those tender blades are circulating freely the gases of the atmosphere, and from those gases—light, intangible nothingness, as we are prone to regard them—this very tangible and important white solid compound is being elaborated. The chemist cannot do this. Plants accomplish it by their most conspicuous feature, greenness, which enables them to put solar energy into cold storage; they are accumulating fuel for subsequent development of bodily heat energy. Side by side with starch, however, these unadvertised silent chemical agencies elaborate molecules even more imposing, in which nitrogen is interwoven with the elements of starch, and thus are produced the vegetable alkaloids.

In this province the chemist has been more fortunate, and successive generations of students have been instructed in the synthesis of piperine, coniine, trigonelline, nicotine, and extensions from the artificial production of tropine; but until quite recently his methods have been hopelessly divergent from those of the plant. Enlightening insight into these, however, was given just four years ago by R. Robinson, who effected a remarkably simple synthesis of tropinone by the mere association of succindialdehyde, methylamine, and acetone in water, unassisted by a condensing agent or an increase of temperature:



Although the yield was very small, it reached 42 per cent. when acetone was replaced by a salt of its dicarboxylic acid, which might easily arise from citric acid as one of the intermediate compounds used by plants in their synthetical exercises.

Based upon this experiment, R. Robinson (1917) has developed an attractive explanation of the phytochemical synthesis of alkaloids, in which the genesis of a pyrrolidine, piperidine, quinunuclidine, or *iso*-quinoline group is shown to be capable of proceeding from the association and interaction of an amino-acid, formaldehyde, acetonedicarboxylic acid and the intermediate products of these, taking place under the influence of oxidation, reduction, and condensation such as the plant is known to effect. It would scarcely be fair to the resourceful skill embodied in this theory to attempt an abbreviated description of the methods by which molecules as complex even as those of morphine and narcotine may be developed. Ornithine ($\alpha\delta$-diaminovaleric acid) is represented as the basis of hygrine, cuschygrine, and the tropine alkaloids, whilst the coniine group may spring from lysine ($\alpha\epsilon$-diaminocaproic acid). A particularly interesting application of these principles has been made with reference to the vital synthesis of harmine, which W. H. Perkin and R. Robinson (1919) represent as arising from a hydroxytryptophan as yet undiscovered; meanwhile they have shown that harman is identical with the base obtained by Hopkins and Cole on oxidising tryptophan itself with ferric chloride. Thus it may be claimed that Robinson's theory represents a notable advance in our conception of these vital changes, and that by means of the carbinolamine and aldol condensations involved fruitful inquiries into constitution and the mechanism of synthesis will follow.

The Nucleic Acids.

Owing to the venerable position occupied by alkaloids in the systematic development of chemical science, and to the success which has attended elucidation of their structure, many of us have become callous to the perpetual mystery of their elaboration. Those who seek fresh wonders, however, need only turn to the nucleic acids in order to satisfy their curiosity. For in the nucleic acid of yeast the chemist finds a definite entity forming a landmark in the path of metabolic procedure, a connecting link between the undefined molecules of living protein and the crystallisable products of katabolic disintegration.

Let us review this remarkable substance. With an empirical formula, $C_{38}H_{49}O_{29}N_{15}P_4$, it has a molecular weight (1303) exceeding that of the octadecapeptide (1213) synthesised by Fischer (1907), although considerably below those of the penta-(penta-acetyl-*m*-digalloyl)-β-glucose (2136) produced by Fischer and Bergmann (1918), and of the hepta-(tribenzoylgalloyl)-*p*-iodophenylmaltosazone (4021) elaborated by Fischer and Freudenberg (1912). Nevertheless, its intrinsic importance is transcendent. In the language of chemistry it is a combination of four nucleotides, linked with one another through the pentose molecule, *d*-ribose, which is common to each, and owing its acid character to phosphoric acid, also common to the component

nucleotides. The latter differ from one another in respect of their nitrogenous factors, which are guanine (2-amino-6-oxypurine), adenine (6-aminopurine), uracil (2 : 6-dioxypyrimidine), and cytosine (2-oxy-6-aminopyrimidine), giving their names to the four nucleotides linked with each other in the following manner:

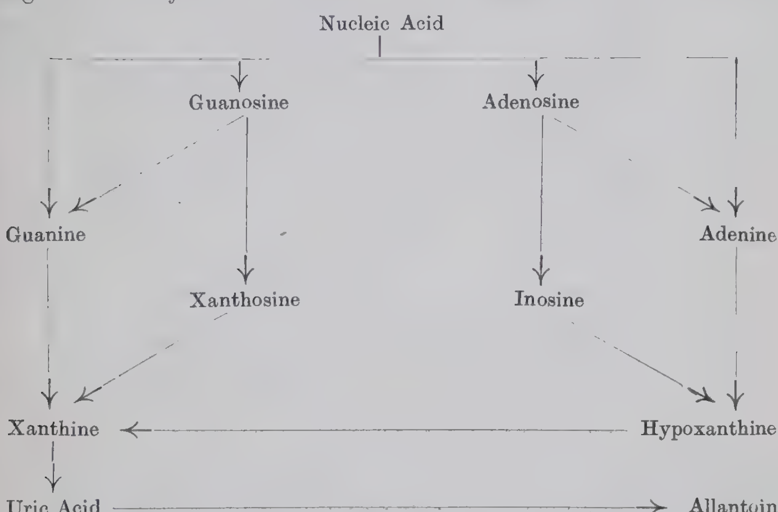
$$
\begin{array}{lll}
(HO)_2PO.O.C_5H_7O_2.C_5H_4ON_5 & & \text{(guanine)} \\
\qquad\qquad\quad | & & \\
\qquad\qquad\quad O & & \\
\qquad\qquad\quad | & & \\
(HO)_2PO.O.C_5H_6O.C_4H_4ON_3 & & \text{(cytosine)} \\
\qquad\qquad\quad | & & \\
\qquad\qquad\quad O & & \\
\qquad\qquad\quad | & & \\
(HO)_2PO.O.C_5H_6O.C_5H_4N_5 & & \text{(adenine)} \\
\qquad\qquad\quad | & & \\
\qquad\qquad\quad O & & \\
\qquad\qquad\quad | & & \\
(HO)_2PO.O.C_5H_7O_2.C_4H_3O_2N_2 & & \text{(uracil)}
\end{array}
$$

We owe this picture of plant nucleic acid to the combined researches of many chemists, conspicuous amongst whom is A. Kossel; he derived purine bases from nucleins in the early eighteen-eighties, and subsequently identified the products of completely hydrolysing the nucleic acids from yeast and from the thymus gland. Characterisation of intermediate products in such hydrolyses—namely, the nucleotides of guanine, cytosine, adenine, and uracil—with the corresponding nucleosides, guanosine, cytidine, adenosine, and uridine is due chiefly to W. Jones and to P. A. Levene, with whom was later associated W. A. Jacobs; but the most picturesque of all contributions to the subject was made by the earliest of the systematic investigators, Friedrich Miescher, who followed his isolation of nuclein (nucleic acid) from pus cells (1868) by the remarkable discovery that the spermatozoa heads of Rhine salmon consist almost entirely of protamine nucleate (1874), and that this must have arisen, not directly from food, but from muscle protein.

Whilst the yeast cell and the wheat embryo have the power to synthesise nucleic acid of the structure represented above, the thymus gland elaborates another nucleic acid in which a hexose is substituted for *d*-ribose, and uracil is replaced by thymine, its methyl derivative (5-methyl-2 : 6-dioxypyrimidine); the order and mode of nucleotide linkage are also different. These nucleic acids, although deriving their carbohydrate and phosphoric acid from the nourishment on which the organism thrives, do not owe the purine factors to the same source; in other words, the tissues must have power to synthesise a purine ring. The mechanism by which they exercise this power is one of the many problems which await elucidation, but arginine (α-amino-δ-guanidinevaleric acid) has been indicated as one possible origin, whilst histidine (α-amino-β-imidazolylpropionic acid) may be a source of the pyrimidine nucleus.

The transformations undergone by nucleic acid in contact with tissue-extracts have provided the subjects of numerous investigations extending over thirty years. In fact, the experimental material is of

such voluminous complexity as to be unintelligible without the guidance of an expert, and in this capacity W. Jones has rendered valuable service by his recent lucid arrangement of the subject (1921). From this it is comparatively easy to follow the conversion of nucleic acid into uric acid through the agency of enzymes, and a review of these processes can serve only to increase our admiration for the precision and facility with which the chemical operations of the living body are conducted. Regarding for the sake of simplicity only the purine nucleotides, these are probably the first products of hydrolysing nucleic acid, and from them there may be liberated either phosphoric acid by a phospho-nuclease, or the purine-base by a purine-nuclease, giving rise to guanine and adenine, with their nucleosides, guanosine and adenosine. Thereafter the procedure is less obscure. The four products exchange their amino-group for hydroxyl under the influence of their respective deaminase—namely, guanase, adenase, guanosine-deaminase or adenosine-deaminase. The two original nucleosides, with their corresponding derivatives, xanthosine and inosine, are then hydrolysed by their appropriate hydrolase, and the resulting oxypurines, xanthine and hypoxanthine, are further oxidised by xanthine-oxidase to uric acid. This is the concluding phase of purine metabolism in man and apes, but other animals are able to transform uric acid into allantoin by means of uricase. The changes may be represented diagrammatically as follows:—



Considerable progress has been made also in localising the various enzymes among the organs of the body, particularly those of animals. Into the results of these inquiries it is not the purpose of this address to enter further than to indicate that they reveal a marvellous distribution, throughout the organism, of materials able to exert at the proper moment those chemical activities appropriate to the changes which they are required to effect. The contemplation of such a system

continuously, and in health unerringly, completing a series of chemical changes so numerous and so diverse, must produce in every thoughtful mind a sensation of humble amazement. The aspect of this miraculous organisation which requires most to be emphasised, however, is that an appreciation of its complex beauty can be gained only by those to whom at least the elements of a training in chemistry have been vouchsafed. Such training has potential value from an ethical standpoint, for chemistry is a drastic leveller; in the nucleic acids man discovers a kinship with yeast-cells, and in their common failure to transform uric acid into allantoin he finds a fresh bond of sympathy with apes. The overwhelming majority of people arrive at the grave, however, without having had the slightest conception of the delicate chemical machinery and the subtle physical changes which, throughout each moment of life, they have methodically and unwittingly operated.

Chlorophyll and Hæmoglobin.

To those who delight in tracing unity among the bewildering intricacies of natural processes, and by patient comparison of superficially dissimilar materials triumphantly to reveal continuity in the discontinuous, there is encouragement to be found in the relationship between chlorophyll and hæmoglobin. Even the most detached and cynical observer of human failings must glow with a sense of worship when he perceives this relationship, and thus brings himself to acknowledge the commonest of green plants among his kindred. Because, just as every moment of his existence depends upon the successful performance of its chemical duties by the hæmoglobin of his blood corpuscles, so the life and growth of green plants hinge on the transformations of chlorophyll.

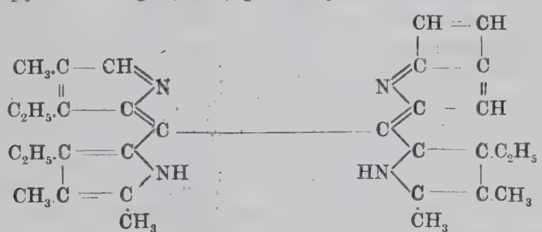
The persevering elucidation of chlorophyll structure ranks high in the achievements of modern organic chemistry, and in its later stages is due principally to Willstätter and his collaborators, whose investigations culminated in 1913. Eliminating the yellow and colourless companions of the substance by a regulated system of partition among solvents, they raised the chlorophyll content to 70 per cent. from the 8 to 16 per cent. found in the original extract, completing the separation by utilising the insolubility of chlorophyll in petroleum ether. By such means, 1 kilogram of dried stinging-nettles gave 6.5 grams of the purified material, representing about 80 per cent. of the total amount which the leaf contains, and application of the process to fresh leaves has established the identity of the product from both sources. Thus the isolation of chlorophyll from plants is now no more difficult than that of alkaloids or of sugars, and may actually be demonstrated as a lecture-experiment.

As a consequence of these operations the dual nature of leaf-green was brought to light in 1912. The focus of main phytochemical action is thus revealed as a system composed of chlorophyll-*a*, bluish-green in solution, and of the yellowish-green chlorophyll-*b*, representing different stages of oxidation as indicated by the formulæ,

$$(a)\ C_{32}H_{30}ON_4Mg(CO_2.CH_3)\ (CO_2.C_{20}H_{39}) = C_{55}H_{72}O_5N_4Mg$$
$$(b)\ C_{32}H_{28}O_2N_4Mg(CO_2.CH_3)\ (CO_2.C_{20}H_{39}) = C_{55}H_{70}O_6N_4Mg\ .$$

In the solid form these products are micro-crystalline powders, bluish-black and greenish-black respectively. They are accompanied by two non-nitrogenous yellow pigments, the unsaturated hydrocarbon, carrotene, $C_{40}H_{56}$, and its oxide, xanthophyll, $C_{40}H_{56}O$, both of which readily absorb oxygen and are allied to a third carrotinoid substance, fucoxanthin, $C_{40}H_{56}O_6$, associated with them in brown algæ and isolated in 1914. Based upon the experience indicated above, Willstätter and his colleagues have examined upwards of 200 plants drawn from numerous classes of cryptogams and phanerogams. The leaf-green of these is identical, and the proportion of *a* to *b* almost invariably approaches 3 : 1 excepting in the brown algæ, in which *b* is scarcely recognisable.

This is not an occasion to follow, otherwise than in the barest outline, the course of laboratory disintegration to which the chlorophyll molecules have been subjected by the controlled attack of alkalis and acids. The former agents reveal chlorophyll in the twofold character of a lactam and a dicarboxylic ester of methyl alcohol and phytol, an unsaturated primary alcohol, $C_{20}H_{39}.OH$, of which the constitution remains obscure in spite of detailed investigation of its derivatives; but the residual complex, representing two-thirds of the original molecule, has been carefully dissected. The various forms of this residual complex, when produced by the action of alkalis on chlorophyll, have been called ‘phyllins’; they are carboxylic acids of nitrogenous ring-systems, which retain magnesium in direct combination with nitrogen. The porphyrins are the corresponding products arising by the action of acids; they are carboxylic acids of the same nitrogenous ring-systems from which the magnesium has been removed. The phyllins and the porphyrins have alike been degraded to the crystalline base, ætioporphyrin, $C_{31}H_{36}N_4$, into the composition of which four variously substituted pyrrole rings enter, probably as follows:—

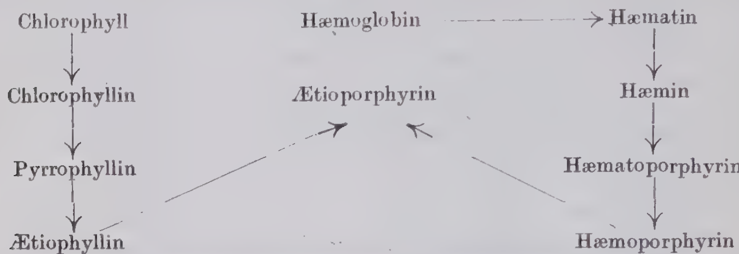


Ætioporphyrin, $C_{31}H_{36}N_4$.

It is this assemblage of substituted pyrroles which, according to present knowledge, is the basic principle also of the blood-pigment, in which iron plays the part of magnesium in chlorophyll. Fundamental as is the difference between hæmoglobin and chlorophyll, relationship can be claimed through this connecting-link, because the same compound, ætioporphyrin, has been produced from hæmoporphyrin, $C_{33}H_{36}O_4N_4$, which is thus its dicarboxylic acid. Hæmoporphyrin arises from hæmatoporphyrin, $C_{33}H_{38}O_6N_4$, produced by the action of hydrobromic acid on hæmin, $C_{33}H_{32}O_4N_4FeCl$, which in turn is derived by exchanging chlorine for hydroxyl in hæmatin

F 2

$C_{33}H_{33}O_5N_4Fe$, the non-albuminoid partner of globin in hæmoglobin. Thus, omitting many intermediate stages, the relationship between chlorophyll and hæmoglobin may be sketched by the following diagram:—



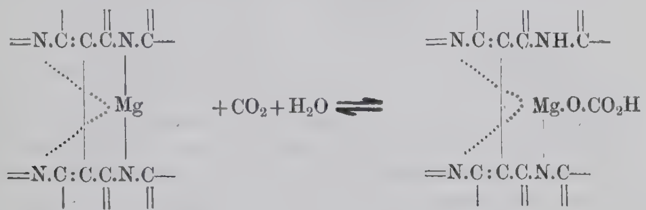
It must be remembered, however, that although recent years have witnessed great progress in elucidating the nature of chlorophyll and hæmoglobin, the mechanism by which they act remains unrevealed. The famous assimilation hypothesis of von Baeyer, according to which it is formaldehyde which represents the connecting-link in the phytochemical synthesis of carbohydrate from carbon dioxide, was enunciated in 1870, and arose from Butlerow's preparation of methylenitan. In spite of numerous criticisms, some of which are quite recent, it remains unshaken. The line of such criticism has taken two directions. On the one hand, H. A. Spoehr (1913), from experiments suggested by the fact that the morning acidity of plant juices diminishes or disappears on exposure to light, has shown that this change is photochemical only, and may be independent of enzymes, the volatile products including formaldehyde. Emil Baur (1908, 1910 and 1913) has urged the claims of oxalic acid to be regarded as the first product of assimilation, and shows how this may lead to the other plant-acids, glycollic, malic and citric, the first-named being a possible stepping-stone to the carbohydrates by resolution into formaldehyde (and formic acid), incidentally assuming towards malic and citric acids the relationship which glucose bears to starch. On the other hand, K. A. Hofmann and Schumpelt (1916), preceded by Bredig (1914), have attacked the hypothesis on the ground of kinetics, and imagine an electrolytic resolution of water under the influence of light, which liberates oxygen and effects the reduction of carbon dioxide by hydrogen to formaldehyde through formic acid.

All these arguments have been weighed by Willstätter and Stoll (1917), who dismiss them on comparing the volume of carbon dioxide absorbed by leaves with the corresponding volume of oxygen liberated. They point out that this assimilatory quotient, CO_2/O_2, which should be unity in the case of formaldehyde, becomes 1.33, 2 and 4 in the case of glycollic, formic and oxalic acids respectively. Proceeding to determine this quotient experimentally they found it to be unity, whether the temperature is 10° or 35°, whether the atmosphere is rich in carbon dioxide or free from oxygen, and alike with ordinary foliage or cactus. Furthermore, they found (1917) that whilst organic liquids holding chlorophyll in solution do not absorb more carbon dioxide than the

liquids themselves, this gas is absorbed much more freely by chlorophyll hydrosols than by other colloidal solutions, a maximum assimilation of two molecular proportions to one magnesium atom being reached, when phæophytin is precipitated:—

$$C_{55}H_{72}O_5N_4Mg + 2CO_2 + 2H_2O = C_{55}H_{74}O_5N_4 + Mg(HCO_3)_2.$$

Prior to this change, which is the first stage appearing in a controlled disruption of chlorophyll-*a* by mineral acids, there is produced an intermediate compound resembling a hydrogen carbonate in which the metal retains a partial grip on the nitrogen:—



It is suggested that leaf-green unites with carbon dioxide by similar mechanism, and that the action of light on the above compound transforms the carbonic acid into an isomeride having the nature of a peroxide such as per-formic acid, H.CO.O.OH, or formaldehyde peroxide, $HO.CH\left\langle\begin{matrix} O \\ | \\ O \end{matrix}\right.$.

Anthocyans, the Pigments of Blossoms and Fruits.

Since the days of Eden, gardens have maintained and extended their silent appeal to the more gentle emotions of mankind. The subject possesses a literature, technical, philosophical, and romantic, at least as voluminous as that surrounding any other industrial art, and the ambition to cultivate a patch of soil has attracted untold millions of human beings. Amongst manual workers none maintains a standard of orderly procedure and patient industry higher than that of the gardener. Kew and La Mortola defy the power of word-painters to condense their soothing beauty into adequate language, whilst that wonderful triangle of cultivation which has its apex at Grasse almost might be described as industry with a halo.

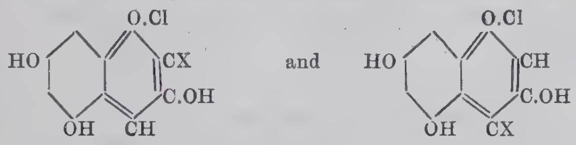
To the countless host of flower-lovers, however, it is probable that Grasse is the only connecting-link between chemistry and their cherished blossoms, they being dimly aware that the ingredients of some natural perfumes have been imitated in the laboratory. The circumstance that identical products of change are generated by the plant, however, and form but one section of the numberless chemical elaborations which proceed before their eyes escapes them because it has been ordained that chemistry is to occupy a backwater in the flood of knowledge. Let us hope that before another century has passed this additional charm to the solace of a garden may be made more generally accessible.

Even to chemists it is only during the last decade that the mechanism of blossom-chemistry has been revealed. The subject has indeed excited their attention since an early period in the history of the organic branch, and the existing class-name for blossom-pigments was first used by Marquart in 1835 to distinguish blue colouring-matters occurring in flowers. It is also interesting to us to notice that in the following year Dr. Hope, who presided over the birth of the Chemistry Section at the Edinburgh meeting in 1834, described experiments conducted with blossoms representing many different orders, and devised a classification of the pigments which they contain. The recognition of glucosides amongst the anthocyans appears to have been first made as recently as 1894, by Heise; about that period, also, it gradually became clear that the various colours assumed by flowers are not variations of a single substance common to all, but arise from a considerable number of non-nitrogenous pigments. Prior to 1913 the most fruitful attempt to isolate a colouring-matter from blossoms in quantity sufficient for detailed examination had been made by Grafe (1911), but the conclusions to which it led were inaccurate. In the year mentioned, however, Willstätter began to publish with numerous collaborators a series of investigations, extending over the next three years, which have brought the subject within the realm of systematic chemistry. For the purpose of distinguishing glucosidic and non-glucosidic anthocyans the names anthocyanin and anthocyanidin respectively were applied. The experimental separation of anthocyanins from anthocyanidins was effected by partition between amyl alcohol and dilute mineral acid, the latter retaining the diglucosidic anthocyanins in the form of oxonium salts and leaving the anthocyanidins quantitatively in the amyl alcohol, from which they are not removed by further agitation with dilute acid; the monoglucosidic anthocyanins were found in both media, but left the amyl alcohol when offered fresh portions of dilute acid.

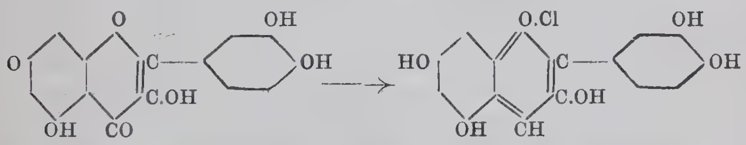
The earliest of these papers, published in conjunction with A. E. Everest, dealt with cornflower pigments, and indicated that the distinct shades of colour presented by different parts of the flower are caused by various derivatives of one substance; thus the blue form is the potassium derivative of a violet compound which is convertible into the red form by oxonium salt-formation with a mineral or plant acid. Moreover, as found in blossoms, the chromogen was observed to be combined with two molecular proportions of glucose and was isolated as crystalline cyanin chloride; hydrolysis removed the sugar and gave cyanidin chloride, also crystalline. Applying these methods more generally, Willstätter and his other collaborators have examined the chromogens which decorate the petals of rose, larkspur, hollyhock, geranium, salvia, chrysanthemum, gladiolus, ribes, tulip, zinnia, pansy, petunia, poppy, and aster, whilst the fruitskins of whortleberry, bilberry, cranberry and cherry, plum, grape, and sloe have also been made to yield the pigment to which their characteristic appearance is due.

The type of structural formula by which the anthocyanidins are now represented was proposed in 1914, simultaneously and

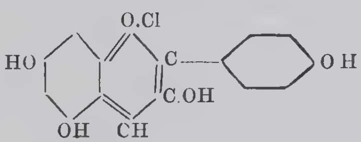
independently by Willstätter and by Everest; incidentally their separate memoirs afford an unusual example of synchronous publication, each having been communicated to the respective academies on the same day, March 26th. Willstätter identified phloroglucinol (1 : 3 : 5-trihydroxybenzene) as a common product of hydrolysing anthocyanidins with alkali, obtaining also *p*-hydroxybenzoic acid from pelargonidin, protocatechuic (3 : 4-dihydroxybenzoic) acid from cyanidin, and gallic (3 : 4 : 5-trihydroxybenzoic) acid from delphinidin. Accordingly he suggested for the anthocyanidin chlorides two alternative formulæ,



in which X represents the substituted benzene ring which appears in the form of a phenolcarboxylic acid on hydrolysis. Later in the same year he confirmed (with Mallison) the former of these representations by reducing quercitin to cyanidin chloride,



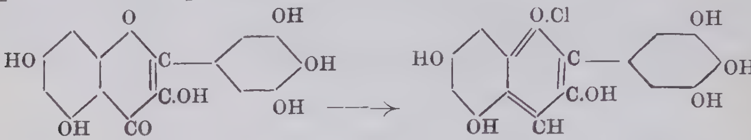
and (with Zechmeister) by effecting a complete synthesis of pelargonidin chloride,



from 2 : 4 : 6-trihydroxybenzaldehyde.

Everest reached the same conclusion by recognising the significance of the fact that a flavone, e.g., luteolin, and a flavonol, e.g., morin, yield red pigments on reduction; he therefore reduced quercitrin (the rhamnoside of quercitin) to cyanidin, and rutin (the rhamnoglucoside of quercitin, and identical with osyritin, myrticolorin, and violaquercitrin) to cyanin. Moreover, he showed that the petals of many yellow flowers, e.g., daffodil, wallflower, tulip, crocus, jasmin, primrose, and viola, or the white blossoms of narcissus, primula, and tulip, all yield red pigments on careful reduction, and in subsequent papers (e.g., with A. J. Hall, 1921) has indicated reduction of yellow sap-pigments belonging to the flavonol group as representing the probable course of anthocyan-formation in plants. In this connection it is noteworthy that an association between the pigments of sap and of blossoms was adumbrated in 1855 by Martens, who suggested that a faintly yellow substance in plant sap, when oxidised in presence of alkalis and light, produces the yellow pigments, and that these, by further oxidation,

change into the red colouring-matters. Everest has shown that reduction is the process actually involved and that flavonols are the precursors of anthocyans, not *vice versa* as suggested by other investigators; moreover, he found (1918) that 'Black Knight' petals contain a glucoside of delphinidin, whilst the corresponding flavonol, myricetin, is present in the sap, also as a glucoside:



Myricetin. Delphinidin Chloride.

Hence it will be seen that pelargonidin, cyanidin, and delphinidin, corresponding to the three above-mentioned phenolcarboxylic acids, are the fundamental materials in the group of anthocyan pigments, and that they are derived from the three flavonols, kaempferol, quercitin, and myricetin respectively. The variations upon these types which present themselves in blossoms are twofold, due to (1) the number and position of entrant methyl groups, and (2) the number and character of the aldose molecules which go to form their glucosides. Belonging to the first group are peonidin (monomethylated cyanidin), ampelopsidin, myrtillidin, and petunidin (monomethylated delphinidin) with malvidin and oenidin (dimethylated delphinidin). The second group arise from combination with glucose, galactose or rhamnose, the greatest proportion of pigments occurring as mono- or diglucosides. Thus callistephin and salvinin are the mono- and diglucoside of pelargonidin; asterin and chrysanthemin are monoglucosides and idaein a galactoside of cyanidin, derived from which are the diglucosides cyanin and mekocyanin, and the rhamnoglucoside, keracyanin; violanin is a rhamnoglucoside of delphinidin, whilst delphinin, when hydrolysed with hydrochloric acid, yields delphinidin chloride, glucose and *p*-hydroxybenzoic acid in the molecular proportion, 1 : 2 : 2.

Thus may the chemist find fresh delight in the hedgerow and the garden by reflecting on the processes which lead to molecular structures lying well within his mental horizon, and adorning those familiar models with all the chromatic splendours of snapdragon, pansy, rose, and larkspur. The gustatory and æsthetic thrill engendered in consuming summer pudding and custard is heightened by the soothing blend of egg-yolk lutein and the crimson contributed to the colour scheme by the raspberry and currant anthocyanins.

Micro-Biochemistry.

Amongst the many sources of pleasure to be found in contemplating the wonders of the universe, and denied to those untrained in scientific principles, is an appreciation of infra-minute quantities of matter. It may be urged by some that within the limits of vision imposed by telescope and microscope, ample material exists to satisfy the curiosity of all reasonable people, but the appetite of scientific inquiry is insatiable, and chemistry alone, organic, inorganic, and physical, offers an

instrument by which the investigation of basal changes may be carried to regions beyond those encompassed by the astronomer and the microscopist.

It is not within the purpose of this address to survey that revolution which is now taking place in the conception of atomic structure; contributions to this question will be made in our later proceedings and will be followed with deep interest by all members of the Section. Fortunately for our mental balance the discoveries of the current century, whilst profoundly modifying the atomic imagery inherited from our predecessors, have not yet seriously disturbed the principles underlying systematic organic chemistry, but they emphasise in a forcible manner the intimate connection between different branches of science, because it is from the mathematical physicist that these new ideas have sprung. Their immediate value is to reaffirm the outstanding importance of borderline research and to stimulate interest in submicroscopic matter.

This interest presents itself to the chemist very early in life and dominates his operations with such insistence as to become axiomatic. So much so that he regards the universe as a vast theatre in which atomic and molecular units assemble and interplay, the resulting patterns into which they fall depending on the physical conditions imposed by nature. This enables him to regard micro-organisms as co-practitioners of his craft, and the chemical achievements of these humble agents have continued to excite his admiration since they were revealed by Pasteur. The sixty years which have now elapsed are rich in contributions to that knowledge which comprises the science of microbiochemistry, and in this province, as in so many others, we have to deplore the fact that the principal advances have been made in countries other than our own. On this ground, fortified by the intimate relation of the science to a number of important industries, A. Chaston Chapman, in a series of illuminating and attractive Cantor Lectures in December, 1920, iterated his plea of the previous year for the foundation of a National Institute of Industrial Micro-biology, whilst H. E. Armstrong, in Birmingham a few weeks later, addressed an appeal to the brewing industry, which, although taking the form of a memorial lecture, is endowed with many lively features depicting in characteristic form the manner in which the problems of brewing chemistry should, in his opinion, be attacked.

Lamenting as we now do so bitterly the accompaniments and consequences of war, it is but natural to snatch at the slender compensations which it offers, and not the least among these must be recognised the stimulus which it gives to scientific inquiry. Pasteur's *Études sur la Bière* were inspired by the misfortunes which overtook his country in 1870-71, and the now well-known process of Connstein and Lüdecke for augmenting the production of glycerol from glucose was engendered by parallel circumstances. That acquaintance with the yeast-cell which was an outcome of the former event had, by the time of the latter discovery, ripened into a firm friendship, and those who slander the chemical activities of this genial fungus are defaming a potential benefactor. Equally culpable are those who ignore them. If children

were encouraged to cherish the same intelligent sympathy with yeast-cells which they so willingly display towards domestic animals and silkworms, perhaps there would be fewer crazy dervishes to deny us the moderate use of honest malt-liquors and unsophisticated wines, fewer pitiable maniacs to complicate our social problems by habitual excess.

Exactly how the cell accomplishes its great adventure remains a puzzle, but many parts of the machinery have already been recognised. Proceeding from the discovery of zymase (1897), with passing reference to the support thus given by Buchner to Liebig's view of fermentation, Chapman emphasises the importance of contributions to the subject by Harden and W. J. Young, first in revealing the dual nature of zymase and the distinctive properties of its co-enzyme (1904), next in recognising the acceleration and total increase in fermentation produced by phosphates, consequent on the formation of a hexose-diphosphate (1908):

$$2C_6H_{12}O_6+2Na_2HPO_4=2CO_2+2C_2H_6O+C_6H_{10}O_4(PO_4Na_2)_2+2H_2O.$$

In this connection it will be remembered that a pentose-phosphate is common to the four nucleotides from which yeast nucleic acid is elaborated. The stimulating effect developed by phosphates would not be operative if the cell were not provided with an instrument for hydrolysing the hexose-diphosphate as produced, and this is believed by Harden to be supplied in the form of an enzyme, hexosephosphatase, the operation of which completes a cycle. As to the stages of disruption which precede the appearance of alcohol and carbon dioxide, that marked by pyruvic acid is the one which is now most favoured. The transformation of pyruvic acid into acetaldehyde and carbon dioxide under the influence of a carboxylase, followed by the hydrogenation of aldehyde to alcohol, is a more acceptable course than any alternative based upon lactic acid. Moreover, Fernbach and Schoen (1920) have confirmed their previous demonstration (1914) of pyruvic acid formation by yeast during alcoholic fermentation.

The strict definition of chemical tasks allotted to yeasts, moulds, and bacteria suggests an elaborate system of microbial trades-unionism. E. C. Grey (1918) found that *Bacillus coli communis* will, in presence of calcium carbonate, completely ferment forty times its own weight of glucose in forty-eight hours, and later (1920) exhibited the threefold character of the changes involved which produce (1) lactic acid, (2) alcohol with acetic and succinic acids, (3) formic acid, carbon dioxide, and hydrogen. Still more recent extension of this inquiry by Grey and E. G. Young (1921) has shown that the course of such changes will depend on the previous experience of the microbe. When its immediate past history is anærobic, fermentation under anærobic conditions yields very little or no lactic acid and greatly diminishes the production of succinic acid, whilst acetic acid appears in its place; admission of oxygen during fermentation increases the formation of lactic, acetic, and succinic acids, diminishes the formation of hydrogen, carbon dioxide, and formic acid, but leaves the quantity of alcohol unchanged. The well-known oxidising effect of *Aspergillus niger* has been shown by J. N. Currie (1917) to proceed in three stages marked by

citric acid, oxalic acid, and carbon dioxide, whilst Wehmer (1918) has described the conditions under which citric acid and, principally, fumaric acid are produced by *Aspergillus fumaricus*, a mould also requiring oxygen for its purpose. The lactic bacteria are a numerous family and resemble those producing acetic acid in their venerable record of service to mankind, whilst among the most interesting of the parvenus are those responsible for the conversion of starch into butyl alcohol and acetone. Although preceded by Schardinger (1905), who discovered the ability of *B. macerans* to produce acetone with acetic and formic acids, but does not appear to have pursued the matter further, the process associated with the name of A. Fernbach, and the various modifications which have been introduced during the past ten years are those best known in this country, primarily because of the anticipated connection with synthetic rubber, and latterly on account of the acetone famine arising from the War. The King's Lynn factory was resuscitated and arrangements had just been completed for adapting spirit distilleries to application of the process when, owing to the shortage of raw material in 1916, operations were transferred to Canada and ultimately attained great success in the factory of British Acetones, Toronto.

Much illuminating material is to be found in the literature of 1919-20 dealing with this question in its technological and bacteriological aspects. Ingenuity has been displayed in attempting to explain the chemical mechanism of the process, the net result of which is to produce roughly twice as much butyl alcohol as acetone. The fermentation itself is preceded by saccharification of the starch, and in this respect the bacteria resemble those moulds which have lately been brought into the technical operation of starch-conversion, especially in France. The amyloclastic property of certain moulds has been known from very early times, but its application to spirit manufacture is of recent growth and underlies the amylo-process which substitutes *Mucor Boulard* for malt in effecting saccharification. Further improvement on this procedure is claimed for *B. mesentericus*, which acts with great rapidity on grain which has been soaked in dilute alkali; it has the advantage of inferior proteolytic effect, thus diminishing the waste of nitrogenous matter in the raw material.

Reviewing all these circumstances we find that, just as the ranks of trades-union labour comprise every kind of handicraftsman, the practitioners of micro-biochemistry are divisible into producers of hydrogen, carbon dioxide, formic acid, acetaldehyde, ethyl alcohol, acetic, oxalic, and fumaric acids, acetone, dihydroxyacetone, glycerol, pyruvic, lactic, succinic and citric acids, butyl alcohol, butyric acid. Exhibiting somewhat greater elasticity in respect of overlapping tasks, they nevertheless go on strike if underfed or dissatisfied with their conditions; on the other hand, with sufficient nourishment and an agreeable temperature, these micro-trades-unionists display the unusual merit of working for twenty-four hours a day. One thing, however, they have consistently refused to do. Following his comparison of natural and synthetic monosaccharides towards different families of yeast (1894), Fischer and others have attempted to beguile unsuspecting

microbes into acceptance of molecules which do not harmonise with their own enzymic asymmetry. Various *apéritifs* have been administered by skilled *chefs de cuisine*, but hitherto the little fellows have remained obdurate.

Photosynthesis.

Beyond a placid acceptance of the more obvious benefits of sunshine, the great majority of educated people have no real conception of the sun's contribution to their existence. What proportion of those who daily use the metropolitan system of tube-railways, for instance, could trace the connection between their progress and the sun? Very moderate instruction comprising the elements of chemistry and energy would enable most of us to apprehend this modern wonder, contemplation of which might help to alleviate the distresses and exasperation of the crush-hours.

For many years past, the problem connected with solar influence which has most intrigued the chemist is to unfold the mechanism enabling green plants to assimilate nitrogen and carbon. Although atmospheric nitrogen has long been recognised as the ultimate supply of that element from which phyto-protoplasm is constructed, modern investigation has indicated as necessary a stage involving association of combined nitrogen with the soil prior to absorption of nitrogen compounds by the roots, with or without bacterial co-operation. Concurrently, the agency by which green plants assimilate carbon is believed to be chlorophyll, operating under solar influence by some such mechanism as has been indicated in a preceding section.

Somewhat revolutionary views on these two points have lately been expressed by Benjamin Moore, and require the strictest examination, not merely owing to the fundamental importance of an accurate solution being reached, but also on account of the stimulating and engaging manner in which he presents the problem. Unusual psychological features have been introduced. Moore's 'Biochemistry,' published three months ago, will be read attentively by many chemists, but the clarity of presentation and the happy sense of conviction which pervade its pages must not be allowed to deter independent inquirers from confirming or modifying his conclusions. The book assumes a novel biochemical aspect by describing the life-history of a research. The first two chapters, written before the experiments were begun, suggest the conditions in which the birth of life may have occurred, whilst their successors describe experiments which were conducted as a test of the speculations and are already receiving critical attention from others (e.g., Baly, Heilbron and Barker, Transactions of the Chemical Society, 1921, p. 1025).

It is with these experiments that we are, at the moment, most concerned. The earliest were directed towards the synthesis of simple organic materials by a transformation of light energy under the influence of inorganic colloids, and indicated that formaldehyde is produced when carbon dioxide passes into uranium or ferric hydroxide sols exposed to sunlight or the mercury arc lamp. Moore then declares

that, although since the days of de Saussure (1804) chlorophyll has been regarded as the fundamental agent in the photosynthesis of living matter, there is no experimental evidence that the primary agent may not be contained in the colourless part of the chloroplast, chlorophyll thus being the result of a later synthetic stage. 'The function of the chlorophyll may be a protective one to the chloroplast when exposed to light, it may be a light screen as has been suggested by Pringsheim, or it may be concerned in condensations and polymerisations subsequent to the first act of synthesis with production of formaldehyde' (p. 55). In this connection it is significant that chlorosis of green plants will follow a deficiency of iron even in presence of sunlight (Molisch, 1892), and that development of chlorophyll can be restored by supplying this deficiency, although iron is not a component of the chlorophyll molecule; moreover, green leaves etiolated by darkness and then exposed to light regain their chlorophyll, which is therefore itself a product arising from photosynthesis.

H. Thiele (1907) recorded the swift conversion of nitrate into nitrite by the rays from a mercury quartz lamp, whilst O. Baudisch (1910) observed that daylight effects the same change, and from allied observations was led (1911) to conclude that assimilation of nitrate and nitrite by green plants is a photochemical process. Moore found (1918) that in solutions of nitrate undergoing this reduction green leaves check the accumulation of nitrite, indicating their capacity to absorb the more active compound. Proceeding from the hypothesis that one of the organisms arising earliest in the course of evolution must have possessed, united in a single cell, the dual function of assimilating both carbon and nitrogen, he inquired (1918) whether the simplest unicellular algæ may not also have this power. He satisfied himself that in absence of all sources of nitrogen excepting atmospheric, and in presence of carbon dioxide, the unicellular algæ can fix nitrogen, grow and form proteins by transformation of light energy; the rate of growth is accelerated by the presence of nitrites or oxides of nitrogen, the latter being supplied in gaseous form by the atmosphere. From experiments (1919) with green seaweed (*Enteromorpha compressus*), Moore concluded also that marine algæ assimilate carbon from the bicarbonates of calcium and magnesium present in sea-water, which thereby increases in alkalinity, and further convinced himself that the only source of nitrogen available to such growth is the atmosphere. A description of these experiments, which were carried out in conjunction with E. Whitley and T. A. Webster, has appeared also in the Proceedings of the Royal Society (1920 and 1921).

For the purpose of distinguishing between (1) the obsolete view of a vital force disconnected with such forms of energy as are exhibited by non-living transformers and (2) the existence in living cells of only such energy forms as are encountered in non-living systems, Moore uses the expression 'biotic energy' to represent that form of energy peculiar to living matter. 'The conception, in brief, is that biotic energy is just as closely, and no more, related to the various forms of energy existing apart from life, as these are to one another, and that in presence of the proper and adapted energy transformer, the living

cell, it is capable of being formed from or converted into various of these other forms of energy, the law of conservation of energy being obeyed in the process just as it would be if an exchange were taking place between any two or more of the inorganic forms ' (p. 128). The most characteristic feature of biotic energy, distinguishing it from all other forms, is the power which it confers upon the specialised transformer to proliferate.

Conclusion.

In 'The Salvaging of Civilisation,' H. G. Wells has lately directed the attention of thoughtful people to the imperative need of reconstructing our outlook on life. Convinced that the state-motive which, throughout history, has intensified the self-motive must be replaced by a world-motive if the whole fabric of civilisation is not to crumble in ruins, he endeavours to substitute for a League of Nations the conception of a World State. In the judgment of many quite benevolent critics his essay in abstract thought lacks practical value because it underestimates the combative selfishness of individuals. Try to disguise it as one may, this quality is the one which has enabled man to emerge from savagery, to build up that most wonderful system of colonial organisation, the Roman Empire, and to shake off the barbaric lethargy which engulfed Europe in the centuries following the fall of Rome. The real problem is how to harness this combative selfishness. To eradicate it seems impossible, and it has never been difficult to find glaring examples of its insistence among the apostles of eradication. Why cry for the moon? Is it not wiser to recognise this quality as an inherent human characteristic, and whether we brand it as a vice or applaud it as a virtue endeavour to bend it to the elevation of mankind? For it could so be bent. Nature ignored or misunderstood is the enemy of man; nature studied and controlled is his friend. If the attacking force of this combative selfishness could be directed, not towards the perpetuation of quarrels between different races of mankind, but against nature, a limitless field for patience, industry, ingenuity, imagination, scholarship, aggressiveness, rivalry, and acquisitiveness would present itself; a field in which the disappointment of baffled effort would not need to seek revenge in the destruction of our fellow-creatures: a field in which the profit from successful enterprise would automatically spread through all the communities. Surely it is the nature-motive, as distinct from the state-motive or the world-motive, which alone can salvage civilisation.

Before long, as history counts time, dire necessity will have impelled mankind to some such course. Already the straws are giving their proverbial indication. The demand for wheat by increasing populations, the rapidly diminishing supplies of timber, the wasteful ravages of insect pests, the less obvious, but more insidious depredations of cur microscopic enemies, and the blood-curdling fact that a day must dawn when the last ton of coal and the last gallon of oil have been consumed, are all circumstances which, at present recognised by a small number of individuals comprising the scientific community, must inevitably thrust themselves upon mankind collectively. In the

campaign which then will follow, chemistry must occupy a prominent place because it is this branch of science which deals with matter more intimately than any other, revealing its properties, its transformations, its application to existing needs, and its response to new demands. Yet the majority of our people are denied the elements of chemistry in their training, and thus grow to manhood without the slightest real understanding of their bodily processes and composition, of the wizardry by which living things contribute to their nourishment and to their æsthetic enjoyment of life.

It should not be impossible to bring into the general scheme of secondary education a sufficiency of chemical, physical, mechanical, and biological principles to render every boy and girl of sixteen possessing average intelligence at least accessible by an explanation of modern discoveries. One fallacy of the present system is to assume that relative proficiency in the inorganic branch must be attained before approaching organic chemistry. From the standpoint of correlating scholastic knowledge with the common experiences and contacts of daily life this is quite illogical; from baby's milk to grandpapa's Glaxo the most important things are organic, excepting water. Food (meat, carbohydrate, fat), clothes (cotton, silk, linen, wool), and shelter (wood) are organic, and the symbols for carbon, hydrogen, oxygen and nitrogen can be made the basis of skeleton representations of many fundamental things which happen to us in our daily lives without first explaining their position in the periodic table of all the elements. The curse of mankind is not labour, but waste; misdirection of time, of material, of opportunity, of humanity.

Realisation of such an ideal would people the ordered communities with a public alive to the verities, as distinct from irrelevancies of life, and apprehensive of the ultimate danger with which civilisation is threatened. It would inoculate that public with a germ of the nature-motive, producing a condition which would reflect itself ultimately upon those entrusted with government. It would provide the mental and sympathetic background upon which the future truthseeker must work, long before he is implored by a terrified and despairing people to provide them with food and energy. Finally, it would give an unsuspected meaning and an unimagined grace to a hundred commonplace experiences. The quivering glint of massed bluebells in broken sunshine, the joyous radiance of young beech-leaves against the stately cedar, the perfume of hawthorn in the twilight, the florid majesty of rhododendron, the fragrant simplicity of lilac, periodically gladden the most careless heart and the least reverent spirit; but to the chemist they breathe an added message, the assurance that a new season of refreshment has dawned upon the world, and that those delicate syntheses, into the mystery of which it is his happy privilege to penetrate, once again are working their inimitable miracles in the laboratory of the living organism.

EXPERIMENTAL GEOLOGY.

ADDRESS TO SECTION C (GEOLOGY) BY

J. S. FLETT, D.Sc., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

Among the citizens of Edinburgh in the closing years of the eighteenth century there was a brilliant little group of scientific, literary, and philosophical writers. These were the men who founded the Royal Society of Edinburgh in the year 1783, and many of their important papers appear in the early volumes of its Transactions. Among them were Adam Ferguson, the historian and philosopher; Black, the chemist who discovered carbonic acid and the latent heat of water; Hope, who proved the expansion of water on cooling; Clerk of Eldin, who made valuable advances in the theory of naval tactics, and his brother, Sir George Clerk; Hutton, the founder of modern geology; and Sir James Hall, the experimental geologist. These men were all intimate friends keenly interested in one another's researches. Quite the most notable member of this group was Hutton, who, not mainly for his eminence in geology, but principally for his social gifts, his bonhomie, and his versatility, was regarded as the centre of the circle. Hutton showed an extraordinary combination of qualities. His father was Town Clerk of Edinburgh. After starting as an apprentice to a Writer to the Signet, he took up the study of medicine at the Universities of Edinburgh and Paris, and graduated at Leyden. He then became a farmer on his father's property in Berwickshire, and also carried on chemical manufactures in Leith in partnership with Mr. Davie. He studied methods of agriculture in England and elsewhere, and was an active supporter of the movement for improving Scottish agriculture by introducing the best methods of other countries. A burning enthusiast in geology, especially in the 'theory of the earth,' he travelled extensively in Scotland, England, and on the Continent making geological observations.

His interests were not confined to geology, for he wrote a treatise on metaphysics, which seems to have been more highly esteemed in his day than in ours, and in his last years he produced a work on agriculture which was never published. The manuscript of this work is now in the library of the Edinburgh Geological Society. He also made interesting contributions to meteorology. Hutton's writings are as obscure and involved as his conversation was clear and persuasive, and it is only from the accounts of his friends, and especially Playfair's 'Life of Hutton,' that we can really ascertain what manner of man he was.

It could easily have happened that when Hutton died his unreadable writings might have passed out of notice, to be rediscovered at a subsequent time, when their value could be better appreciated. But Playfair's 'Explanations of the Hutton Theory,' as attractive and

convincing still as when it was originally published, established at once the true position of Hutton as one of the founders of geology. Sir James Hall undertook a different task: he determined to put Hutton's theories to the test of experiment, and in so doing he became the virtual founder of modern experimental geology. It is my purpose in this address to show what were the problems that Hall attacked, by what methods he attempted to solve them, and what were his results. I shall also consider how far the progress of science has carried us since Hall's time regarding this department of geological science.

Hutton was a friend of Hall's father: they were proprietors of adjacent estates in the county of Berwick, and much interested in the improved practice of agriculture, and though the elder Hall (Sir John Hall of Dunglass) has apparently left no scientific writings, he was one of those who were familiar with Hutton's theories and a member of the social group in which Hutton moved. Sir James Hall was the eldest son; born in 1761, he succeeded to the estate on his father's death in 1776. Educated first at Cambridge and then at Edinburgh University, at an early age he became fascinated by Hutton's personality, though repelled by his theories. He tells us how for three years he argued with Hutton daily, rejecting his principles. Hutton prevailed in the long run, and Sir James Hall was convinced. Hall's objection to Hutton's theories is not difficult to understand, though he has not himself explained it. The world was sick of discussions on cosmogony in which rival theorists appealed to well-known facts as proof of the most extravagant speculations. Serious-minded men were losing interest in these proceedings. The Geological Society of London was founded in 1807, and one of its objects is stated to be the avoidance of speculation and the patient accumulation of facts. No doubt Hall also was greatly influenced by the discoveries that Black and Hope had made by pure experimental investigation. His bent of mind was towards chemical, physical, and experimental work, while Hutton was not only a geologist but also a metaphysician.

Foreign travel was then an essential part of the education of a Scottish gentleman, and the connection between France, Holland, and Scotland was closer than it is to-day. Hall travelled widely; in his travels two subjects seem to have especially engrossed him. One was architecture, on which he wrote a treatise which was published in 1813 and is now forgotten. The other was geology. He visited the Alps, Italy, and Sicily. In Switzerland he may have met De Saussure and discussed with him the most recent theories of their time regarding metamorphism and the origin of granites, schists, and gneisses. In Italy and Sicily one of his objects was to observe the phenomena of active volcanoes, and to put to the test of facts the theories of Werner and of the Scottish school regarding the origin of basalt, whinstone, trap, and the older volcanic rocks of the earth's crust. At Vesuvius he made his famous observation of the dykes that rise nearly vertically through the crater wall of Somma, which he held to prove the ascent of molten magma from below through fissures to the surface. This was in opposition to the interpretation of the Wernerians, who regarded them as filled from above by aqueous sediments, and Hall's conclu-

sions, which were strikingly novel at the time, have been abundantly confirmed.

We obtain a pleasant glimpse of Hall's life in Berwickshire in the account of his visit with Hutton and Playfair to Siccar Point in the year 1788. The start was made from Dunglass, where probably the party had spent the night. The great conglomerates of the Upper Old Red Sandstone of that district had much impressed Hutton. He saw in them the evidence of new worlds built out of the ruins of the old, with no sign of a beginning and no prospect of an end—a thesis which was one of the corner-stones of his 'Theory of the Earth.' No doubt Hall knew or suspected that in the cliff-exposures at Siccar Point, where the Old Red rests upon the Silurian, there was evidence which would put this dogma to a critical test.

Hall's first experiments were begun in the year 1790, his object being to ascertain whether crystallisation would take place in a molten lava which was allowed to cool slowly. It was generally believed that the results of fusion of rocks and earths were in all cases vitreous, but glassmakers knew that if glass was very slowly cooled, as sometimes happened when a glass furnace burst, the whole mass assumed a stony appearance. An instance of this had come under Hall's notice in a glassworks in Leith, and its application to geology was clear. Hutton taught that even such highly crystalline rocks as granite had been completely fused at the time of their injection, and their coarse crystallisation was mainly due to slow cooling.

For the purpose of his experiments Hall selected certain whinstones of the neighbourhood of Edinburgh, such as the dolerites of the Dean, Salisbury Crags, Edinburgh Castle, the summit of Arthur's Seat, and Duddingston; but he also used lava from Vesuvius, Etna, and Iceland. He made choice of graphite crucibles, and conducted his experiments in the reverberatory furnace of an ironfoundry belonging to Mr. Barker. As had been shown by Spallanzani, to whose experiments Hall does not refer, lavas are easily fusible under these conditions. Hall had no difficulty in melting the whinstones and obtaining completely glassy products by rapid cooling. He now proceeded to crystallise the glass by melting it again, transferring it from the furnace to a large open fire, where it was kept surrounded by burning coals for many hours, and thereafter very slowly cooled by allowing the fire to die out. He succeeded in obtaining a stony mass in which crystals of felspar and other minerals could be clearly seen. Some of his specimens were considered to be very similar in appearance to the dolerites on which his experiments were made.

The only means of measuring furnace temperatures available at that time were the pyrometers which had recently been invented by Wedgwood. Hall found that a temperature of 28 to 30 Wedgwood yielded satisfactory results. This seems to be about the melting-point of copper, approximately 1000° C.

Whether by design or accident, Hall chose for his experiments precisely the rocks which were most suitable for his purpose. If granite had been selected no definite results would have been obtained. De Saussure had already made fusion experiments on granite. Ninety

years afterwards the problem was completely solved by Fouqué and Lévy, who used a gas furnace and a nitrogen thermometer. They found that it was possible to obtain either porphyritic or ophitic structure by modifying the conditions, and that the minerals had exactly the characters of those of the igneous rocks. Some of Hall's recrystallised dolerites were examined microscopically by Fouqué and Lévy, and, as might be expected, they proved to be only partly crystallised, showing skeleton crystals of olivine and felspar with grains of iron ore in a glassy base.

Some curious observations made by Hall in his experimental work were also confirmed by Fouqué and Lévy. The crystalline whinstones were more difficult to melt than the glasses which were obtained from them, and the glass crystallised best when kept for a time at a temperature a little above its softening point. It is not possible to assign a definite melting-point to the Scottish whinstones with which Hall worked. Many of them contain zeolites, which fuse readily. Minerals are also present that decompose on heating, such as calcite, dolomite, chlorite, and serpentine. The whole process is very complex, and probably takes place by several stages not sharply distinct. Similarly the glasses cannot be said to have a melting-point. They are really super-cooled liquids. A full explanation of what took place in Hall's crucibles cannot be given at the present day, but there is no room for doubt that his experiments were good and his inferences accurate. His friend Kennedy, who had recently discovered the presence of alkalis in igneous rocks, furnished valuable support to Hall's conclusions by showing that the chemical composition of whinstone and of basalt were substantially identical.

Apparently the results of Hall's work were not received with unmixed approbation. Hutton was distinctly uneasy, and it has been suggested that he feared if experimental work turned out unsuccessful it might bring his theories into discredit. The Wernerians frankly scoffed; they preferred argument to experiment, and the endless discussion went on. Gregory Watt repeated Hall's experiments by fusing Clee Hill dolerite, a hundredweight or two at a time, in a blast-furnace. But there can be no doubt that among those who were not already committed to the principles of Werner the new evidence produced a strong impression, and helped to widen the circle of Hutton's supporters.

Hall's most famous experiments were on the effect of heat combined with pressure on carbonate of lime. The problem was, Can powdered chalk be converted into firm limestone or into marble by heating it in a confined space? In this case Hutton's theories were in apparent conflict with experimental facts; from general observations he held it proved that heat and pressure had consolidated limestones and converted them into marbles. It was well known, of course, that limestone, when heated in an open vessel, was transformed into quicklime, and Black had shown that the explanation was that carbonic acid had been expelled in the form of a gas.

The experiments were begun in 1790, but deferred till 1798 after Hutton's death. Hutton quite openly disapproved of experiments. His

famous apophthegm has often been quoted about those who 'judge of the great operations of the mineral kingdom by kindling a fire and looking in the bottom of a crucible.' In deference to the feelings of his master and his father's friend, Sir James Hall, with admirable self-restraint, decided not to undertake experimental investigations in opposition to Hutton's expressed opinion. With a few months' interruption in 1800 they were continued till 1805. A preliminary account of the results was communicated to the Royal Society of Edinburgh on August 30, 1804, and the final papers submitted on June 3, 1805. Hall states that he made over 500 individual experiments and destroyed vast numbers of gun-barrels in this research.

The method adopted was to use a muffle-furnace burning coal or coke and built of brick. No blast seems to have been employed. The chalk-powder was enclosed in a gun-barrel cut off near the touch-hole and welded into a firm mass of iron. The other end of the barrel could be kept cool by applying wet cloths, and as it was not in the furnace its temperature was always comparatively low. Various methods of plugging the barrel were adopted; at first he used clay, sometimes with powdered flint. Subsequently a fusible metal which melted at a temperature below that of boiling water was almost always preferred. Borax glass with sand was used in some of the experiments, but it was liable to cracking when allowed to cool, and consequently was not always gas-tight. It was essential, of course, that in sealing up the gun-barrel, and in subsequently removing the plug, the temperatures should never be so high as to have any sensible effect on the powdered chalk or limestone. Hall tried vessels with screwed stoppers or lids at first, but never found them satisfactory.

In the gun-barrel there was always a certain amount of air enclosed with the chalk. Very early in the experiments it was shown that if no air-space was provided the fusible metal burst the barrel. No means was found to measure the size of the air-space accurately, but approximately it was equal to that of the powdered chalk used in the experiment. If the air-space was too large, or if there was an escape of gas, part of the chalk was converted into lime.

As each experiment lasted several hours the temperature of the chalk was approximately equal to that of the part of the muffle in which it was placed. Pyrometry was as yet in its infancy. Wedgwood had invented pyrometric cones and Hall had heard of them, but apparently at first he was not in possession of a set. He made his own cones, as nearly similar as possible to those of Wedgwood, and subsequently obtaining a set of Wedgwood's cones he standardised his own by comparison with them. His gun-barrels of Swedish and Russian iron ('Old Sable') were softened, but seldom gave way except when the internal pressures were of a high order. Some of the gun-barrels seem to have been used for many experiments without failure occurring. As Hall made his own pyrometric cones, and we have no details of their composition and the method of preparation, it is not possible to do more than guess at the temperatures to which his powdered lime and chalk were exposed. There is no doubt that by constant practice and careful observation he was able to regulate the temperature within fairly wide limits.

Hall began his experiments as already stated in 1798. They were interrupted for about a year (March 1800 to March 1801), and on March 31, 1801, he had obtained a considerable measure of success. A charge of forty grains of powdered chalk was converted into a firm granular crystalline mass of limestone. The loss on weighing was approximately 10 per cent. Another charge of eighty grains was converted into marble (on March 3, 1801), with a loss of approximately 5 per cent., and the crystalline mass showed distinct rhombohedral cleavage.

Though it cannot be said that his success was easily won he was by no means satisfied, and for another four years he continued his researches. Many different methods were tried in order to ascertain the most satisfactory and reliable; his ambition was to attain complete control of the process so that he could always be certain of the result. Porcelain tubes were tried, which he obtained from Wedgwood. They were very liable, however, to allow escape of the gases through pores. Many different methods of obtaining gas-tight stoppers were experimented on, but he does not seem to have found anything really better than the fusible metal. A slight loss of weight in the chalk used seemed inevitable, and the amount of loss varied irregularly; after long trials he ultimately succeeded in reducing this to less than 1 per cent. Various kinds of carbonate of lime were used, including chalk, limestone, powdered spar, oyster shells, periwinkles, and each of these was crystallised in turn. Many experiments showed that a reaction might take place between the chalk powder and the glass of the tube in which it was contained. The result was a white deposit often crystalline, and a certain amount of uncombined carbonic acid gas which escaped when the tube was opened. No doubt the white mineral was wollastonite. Hall proved that it was a silicate of lime which dissolved in acid and left a cloud of gelatinous silica. Thereafter he used platinum vessels instead of glass to contain the charge of carbonate of lime which he wanted to fuse. The effect of impurities in the material used was also investigated. Critics had urged that his limestone was not pure. Hall aptly replied that this was so much the better; natural limestones were seldom pure, and his point was that limestone might be fused under heat and pressure. He obtained the purest precipitated carbonate of lime, and used also perfectly transparent crystalline spar; the results were, as we might expect, that the pure substances and the fairly coarse crystalline powder were more difficult to fuse than the very finely ground natural chalk. These results show that Hall had very complete control of his experimental processes, and that even small differences in fusibility did not escape his observation.

As natural limestones are always moist, Hall's attention was next directed to the influence of water on the crystallisation of his powders. This added greatly to the difficulty of the experiments, but by wonderful skill he succeeded in using a few grains of water (apparently up to 5 per cent. of the weight of the chalk). The result was to improve the crystallisation, for the reason, as Hall believed, that the pressure was increased. He noticed at the same time that hydrogen was produced, which took fire when the gun-barrel was discharged. Probably there

was also some carbonic oxide. About this time he was using bars of Russian iron into which a long cylindrical cavity had been bored. He then tried other volatile ingredients such as nitrate of ammonia, carbonate of ammonia, and gunpowder. In January 1804 he was able to convert chalk into firm limestone at a temperature about 960° (melting-point of silver) in presence of small quantities of water with a loss of less than one-thousandth part of the chalk used.

Finally he attempted to measure the pressure which was necessary to effect re-crystallisation under the conditions of his experiments. No pressure gauges were available at that date, and after many trials he employed a stopper faced with leather and forced against the mouth of his iron tube by means of weights acting either directly or through a lever. He ultimately succeeded in obtaining gas-tight junctions under pressures ranging from 52 up to 270 atmospheres, and concluded that 52 atmospheres was the least pressure which could be satisfactory. This is equal to the pressure of a column of water 1,700 feet high or to a column of rock 700 feet high. A 'complete marble' was formed at a pressure of 86 atmospheres and carbonate of lime 'absolutely fused' under a pressure of 173 atmospheres.

In reviewing these classic experiments after a lapse of 120 years we feel that there are many points on which we should have liked more detailed information. One essential, for example, is exact chemical analysis of all the materials employed. Even chalk is variable in composition to a by no means negligible extent. Oyster shells and periwinkle shells contain organic matter, which would account for the considerable loss in weight they always exhibited. The use of glass tubes was a defect in the early experiments, afterwards remedied by employing platinum vessels. Although in all the experiments the charge was weighed it seems clear that at first at any rate the materials were not carefully dried. In the experiments with water it was seldom possible to provide absolutely against the escape of moisture when the fusible metal was introduced. Most of all we may regret the inadequate means of measuring the temperatures at which the experiments were conducted. The measurements of pressure were made by the simplest possible means, and it was only by great experimental skill and care that even approximate results could be obtained.

Such criticisms, however, do not mar the magnificent success of Hall's experiments. For nearly a hundred years, in spite of the advance of physical and chemical science, no substantial improvement on his results was attained. His work was immediately recognised as trustworthy and conclusive, and became a classic in the literature of experimental geology. Although not exactly the founder of this school of research, for Spallanzani and De Saussure had made fusion experiments on rocks before his time, he placed the subject in a prominent position among the departments of geological investigation, and did great service in supporting Hutton's theories by evidence of a new and unexpected character.

As Hall himself has told us, there were critics who before the complete account of his researches in carbonate of lime was published had challenged the accuracy of some of his conclusions. The ground seems

to have been that the materials he worked with were impure, and that the glass or porcelain vessels in which the powdered chalk was placed were visibly acted on during the experiment. Hall recognised the justice of these conclusions in so far that he made further experiments on the purest precipitated carbonate of lime that he could obtain, and he used platinum vessels instead of glass. These changes admittedly made success more difficult to attain, but he considered that he ultimately was able to fuse the pure chemical in platinum vessels with only a negligible loss of weight by escape of carbonic acid. This seems to have silenced criticism, and with the gradual acceptance of most of Hutton's theories the controversy died down for a time.

Many attempts were made to repeat Hall's experiments during the next eighty years with varying degrees of success. No one was able to secure perfectly gas-tight stoppage of porcelain or iron tubes as Hall did, though they had the record of his experiments to help them, a fact which shows how extremely skilful Hall was in experimental practice. But various authors found that chalk, powdered limestone, and even pure Iceland spar powder or precipitated carbonate of lime could be converted into a firm coherent mass by heating in an open furnace. It was also claimed that lithographic limestone became a crystalline rock resembling marble when heated before a blowpipe under certain conditions. Whether the mass was actually fused was not expressly proved by any of these experiments, and in time it came to be recognised that to make a limestone from powdered chalk it was not necessary that melting should take place. On the other hand, it was contended that when Hall's experiments had resulted in the production of a vesicular or frothy mass which showed evidence that it had dripped or flowed, or that it had been spattered in drops about his apparatus, as he concluded from the appearance presented in certain of his experiments, there was some reason to believe that chemical action had taken place between the silicates of his tubes of glass or porcelain, or the pipeclay stems in which the drops of water were contained, and the carbonate of lime, with the formation of readily fusible compounds. Hall, of course, was perfectly aware that his carbonate of lime combined with the ingredients of glass, porcelain, pipeclay, refractory cones, and silica. He had noticed that in many experiments. What was necessary was a complete quantitative chemical analysis of some of his fused masses, to show that they were carbonate of lime and nothing else. This he never performed. He had his specimens of artificial marble cut and polished, thus testing their hardness, their crystalline structure, and their transparency. He noted also how far the specimens were permanent in dry air, and found that very frequently they disintegrated owing to the presence of a considerable proportion of caustic lime. In many cases also he threw part of the mass into acid and observed complete solution with effervescence of carbonic acid gas. He trusted apparently to determining whether there had been loss of weight, by escape of carbonic acid gas either during the experiment or subsequently on opening the gun-barrel, and argued that if the tube and its contents had the same weight after and before the experiment there could have been no chemical

change. This argument is sound, but confirmatory evidence by quantitative analysis would have rendered the matter certain.

The other question under dispute, viz.: whether actual fusion had taken place or only re-crystallisation in a pasty mass, was of a higher order of difficulty, and neither in Hall's time nor for a century later were means available definitely to settle it.

During the whole of the nineteenth century this controversy lasted and no satisfactory conclusions were reached. Those who tried to repeat Hall's experiments with porcelain tubes, gun-barrels, and iron cylinders met the same difficulties as he did, and were on the whole less successful in overcoming them. In most cases their gun-barrels burst or the method of stopping them failed. It became clear that a coherent mass could be obtained from powdered chalk or pure carbonate of lime at a red heat without great pressure, but no one obtained really convincing evidence of fusion. The problem remained practically as Hall had left it.

The twentieth century, however, has witnessed a tremendous improvement in our methods of tackling such questions as these, and the result has been that a new department of physico-chemical or experimental petrology has been opened up and already possesses a large and most interesting literature. It is really the old experimental geology of Sir James Hall, developed almost beyond recognition. Essentially three factors have produced this result. One is the application of the electric furnace, so powerful and at the same time so compact and easily managed. By its means temperatures from 1000° to 1600° C. are easily obtained, and as many silicates and other minerals have fusion points between those limits their behaviour in the molten state and during crystallisation and cooling becomes accurately observable. The second factor is the invention of the electric pyrometer, by which temperatures up to the melting-point of platinum can be observed instantaneously and continuously with an accuracy of one or two degrees centigrade. The third important element which has determined the recent progress of knowledge in this field is the theoretical mathematical researches of such men as Willard Gibbs, Roozeboom, Schreinemakers, and Smits, which are so full and clear that in many respects they are far in advance of the experimental results. To these we may add the continual improvement in microscopic methods of determining minerals, and the advance in knowledge of crystallography, optics, and analytical chemistry.

Among workers in this field it is generally agreed that only the purest chemicals or minerals should be used, as the presence even of traces of impurity may greatly modify the phenomena, and the interpretation of the results is so difficult that unnecessary complications must be studiously avoided.

The behaviour of $CaCO_3$ under heat and pressure is really a question of two components, CaO and CO_2, one of these being solid and very infusible, the other a gas at ordinary temperatures. We may simplify it by regarding the system for our present purposes as consisting of CaO and $CaCO_3$ with CO_2 as a volatile constituent, arising from the dissociation of $CaCO_3$ at certain temperatures and pressures

Of the components CaO, lime, is a solid fusible in the electric arc at a temperature about 2570° C. as measured by the optical pyrometer. There are reasons for believing that lime exists in two forms; one of these is nearly isotropic and perhaps amorphous, and is obtained by the dissociation of $CaCO_3$ at low temperatures; the other is cubic with good cleavage and generally occurs in rounded crystals; at high temperatures this is probably the only form met with.

$CaCO_3$ as a mineral and as a chemical compound has been so extensively studied that the literature would fill a considerable library. At least eight forms of it have been described. Four of these, ktypeite, conchite, lublinite, and vaterite are rare and doubtful, and are by no means satisfactorily known. Recently a form known as $\mu CaCO_3$ has been described, but it is not believed to be of importance as a mineral. Two others are the well-known minerals calcite and aragonite. Calcite is the stable form under ordinary conditions. Aragonite is transformed into calcite slowly in presence of moisture and carbonic acid, and rapidly if heated to a temperature from 400° to 500° C., but is practically stable in dry air at ordinary temperatures and pressures. There is some reason for believing that aragonite would be the stable form in temperatures 100° C. or so below the freezing-point. It has a higher specific gravity than calcite. In all experiments in which well-formed crystals of aragonite have been heated they changed to granular crystalline aggregates of calcite before dissociation or melting began. This transition so far as is known is irreversible.

At temperatures between 450° and 970° C. calcite is the result of heating every known form of $CaCO_3$, but above that point it is believed to change to another mineral, $\alpha CaCO_3$, not very different in crystallographic and optical characters. The transition is reversible, and as the temperature falls calcite is again formed. The existence of this transition is indicated by the heating and cooling curves of calcite, which show a discharge of heat delaying the fall of temperature about 970° C. The change is very small. Optical studies have been made with the help of an electric furnace closed with transparent quartz-glass plates, but no measurements were obtained of the optical constants of the mineral, and of its crystallographic form nothing is known except that it is probably trigonal. The change in fact is very similar to that by which quartz passes into α-quartz at a temperature of 575° C., and it has been proposed to use calcite like quartz as a geological thermometer.

Carbonate of lime when heated in a closed vessel melts at a temperature of 1289°. A pressure of not less than 110 atmospheres of carbonic acid gas is necessary to prevent dissociation at the melting temperature. It forms quite a liquid melt which will readily flow through cracks in the platinum vessel that contains it. On cooling the melt crystallisation takes place readily, and the resulting mass when cold is finely granular and completely crystalline.

The dissociation pressure of $CaCO_3$ when heated has been studied by several investigators and the results are somewhat discordant. The most reliable results obtained by actual experiment show that at 587° dissociation has only begun, the pressure of CO_2 being only one millimetre of mercury. At 700° it is 25mm., at 800° about 160mm.,

and at 900° about 720mm., so that it increases rapidly with rise of temperature. The dissociation pressure at the melting-point stated above was determined experimentally. It is quite probable that the high pressure of carbonic gas favours fluidity in the melt, and also accelerates crystallisation.

If the pressure be less than the figures given above, a certain amount of dissociation will take place, and lime, CaO, will be present in the melt. We have then a binary system CaO and $CaCO_3$ and a binary eutectic point is to be expected. Boeke has investigated this system, and finds that the eutectic mixture has a melting temperature approximately 1218° C., and consists of 91 per cent. $CaCO_3$ and 9 per cent. CaO. Small additions of $CaCO_3$ raise the melting-point only slowly, but the presence of additional lime makes the melt far more infusible. Mixtures of CaO and $CaCO_3$ with more than 9 per cent. CaO show on microscopic examination a finely crystalline first generation of lime crystals followed by a second generation of $CaCO_3$ and CaO well crystallised. On the other hand, mixtures containing less CaO than the eutectic proportion show branching skeleton crystals of early $CaCO_3$ which have a development indicating trigonal symmetry, and a ground mass consisting of CaO and $CaCO_3$. The large early skeleton crystals often continue to grow during the consolidation of the eutectic so as to give a coarsely crystalline appearance to the aggregate. Hence melts containing little lime often yield semi-transparent crystalline masses having the appearance of marble though not its minute structure. The refractive index of CaO obtained in this way is about 1.83, and it is optically isotropic, so that there is no difficulty in recognising it in the microscopic slides.

So far as research has yet gone, no evidence has been found that there are intermediate compounds between CaO and $CaCO_3$, and the two substances do not appear to form solid solutions to a perceptible extent.

To indicate how far experimental methods have advanced since the days of Sir James Hall, a brief account of the apparatus used will not be without interest. The carbonate of lime was either true Iceland spar, which is quite as free from admixture as the best prepared carbonate, or specially purified precipitated $CaCO_3$. It was heated in a platinum vessel, as in Hall's experiments. This vessel was placed in a small electric resistance furnace with walls of fireclay and magnesia and a platinum spiral. This furnace could attain a temperature of 1600° in a few minutes, and maintain it perfectly steadily for days if required. The small furnace containing the platinum tube was now placed in a steel vessel less than six inches in diameter with thick walls. The lid of the container was fastened with bolts and nuts and a lead washer used to prevent escape of gas. By this arrangement the small internal furnace was alone heated; the steel enclosing vessel could be kept cold if necessary by a water-cooling arrangement, and it was a fairly simple matter to obtain gas-tight connections, and to obviate any risk of bursting. The space between the steel vessel and the furnace was packed with purified asbestos, to prevent convection currents in the carbonic acid gas from affecting the temperature of the electric furnace.

Temperature was measured by a platinum-platinum-rhodium electric pyrometer of which the sensitive part was immersed in the $CaCO_3$ which was being experimented on. Carbonic acid gas was provided in an ordinary steel cylinder such as is used for trade purposes; these can easily stand higher pressures (at ordinary temperatures) than those it was necessary to employ. The gas in these cylinders is at fifty atmospheres pressure, but by immersing the cylinder in hot water the pressure could be raised sufficiently for the purposes of the experiment. All pressures were measured by an ordinary Bourdon gauge, such as is used for many purposes in the arts. Through the walls of the vessel the insulated wires of the electric furnace and pyrometer and the tube carrying carbonic acid gas were led by gas-tight junctions. The whole apparatus worked perfectly smoothly. It is a type of experimental plant which is already employed in many researches into the behaviour of substances at high temperatures under considerable gas pressures, and seems likely to play a large part in the progress of experimental geology in the near future. By slow stages it has reached its present development, and when we remember how many advantages we enjoy in experimental work to-day as compared with Sir James Hall, who was a real pioneer and had to invent all his apparatus and solve every difficulty for himself, we can appreciate more thoroughly the masterly ingenuity he displayed.

As the outstanding uncertainty about Hall's experiments is the question whether his carbonate of lime was actually melted or not, we may pause to consider what evidence is accepted as sufficient on this point at the present day. In ordinary cases the proof of fusion would be that the mass became liquid, but as the charge is contained in a furnace inside a closed steel vessel it is impossible to examine it till the apparatus cools down and is opened up. In many cases also it is possible to rapidly cool the melt, by dropping it into water or mercury, and if it solidifies as a pure glass the proof of fusion is complete. The carbonate of lime, however, could not be chilled either directly or indirectly, and, furthermore, it seems clear that this substance crystallises so readily that to obtain solidification as a vitreous mass might be quite impossible. Reliance accordingly must be placed on a third experimental method, that of reading the heating and cooling curves as recorded by the pyrometer. Change of state involves either the liberation or absorption of heat, and these may be ascertained without any difficulty. Fortunately the behaviour of carbonate of lime in this respect is quite satisfactory; it melts sharply at a definite temperature and crystallises very readily on cooling, so that the exact fusion point is not difficult to observe. Moreover, the microscopic appearance of the crystalline masses produced is entirely in accordance with the belief that complete fusion had taken place.

We may now consider what light modern research has thrown on the vexed question whether Hall succeeded in melting carbonate of lime, and on the value and accuracy of his experimental work generally. It is clear that Hall in his best experiments was able to prevent escape of gas from his gun-barrels. The fact that there was no significant loss of weight in the materials he used seems to prove this satisfactorily.

His latest experiments with arrangements for measuring the pressure were conducted in a manner far less likely to obtain complete retention of the gas than his early experiments, but they show that he had obtained a useful first approximation to the pressures involved, and that somewhere between 100 and 150 atmospheres was the dissociation pressure of $CaCO_3$ on melting. So crude was his apparatus, to modern ideas, that it is wonderful he obtained any results at all.

The necessity of providing an air space to prevent bursting of the gun-barrels by expansion of the fusible metal makes it certain that lime was always present in his melt, and as the air space was never accurately measured it is not certain to what extent dissociation took place. He was working therefore with mixtures of CaO and $CaCO_3$, and the temperature of the fusion at which he aimed was the eutectic point at 1218°C. In most cases he probably had excess of lime in his melt, but in his best experiments he was either very near the eutectic mixture or on the carbonate of lime branch of the curve. The effect of the water which he introduced may have been to lower the melting-point slightly, though there are no very exact experimental results at the present time to indicate the magnitude of this effect; probably it was not very great. If then he was able to reach a temperature of 1218° C. he may be said to have succeeded. Everything depends on the conditions in his furnace, and this was determined by the design of the furnace, the nature of the fuel, and the draught. Apparently he did not use a blast, and we are not informed as to the chimney. His furnace and muffle seem to belong to a pattern which has been long employed for refining silver and gold and for assaying copper. Now copper melts at 1082°C., and it is open to doubt whether a muffle-furnace of this type will give a temperature of 1218°C. It seems just possible that the melting-point of carbonate of lime may have been actually reached under the best working conditions. The question will never be settled. Hall's pyrometers were the least satisfactory part of his apparatus, and all his critics are agreed that it is impossible to interpret the results that they gave. Without an exact knowledge of the materials, structure, and dimensions of his furnace, his fuel and his draught, we cannot reproduce the conditions under which he was working, and his descriptions of his methods are too incomplete to settle the point.

The determination of the actual melting-point and vapour pressure of $CaCO_3$ is a question, however, which interests the physical chemist more than the geologist, and there is little evidence to show that the eutectic mixture of lime and carbonate of lime has a distinct importance as a component of rocks. Though Sir James Hall may not have clearly realised it, he had established a truth of far higher value to geologists. He had shown that at comparatively low temperatures such as the melting-point of silver, which is 960°C., a fine grained aggregate of calcite will readily recrystallise. If the mineral is an incoherent powder it will agglutinate into a firm coherent mass. At slightly higher temperatures it becomes plastic, so as to assume the shape of the vessel which contains it, and loses any angularities or irregularities of its surface. These temperatures are about the same as those exhibited by ordinary lavas, and must be quite common in the vicinity of under-

ground intrusions. The whole of the phenomena of the contact alteration of limestone, including the disappearance of original structures and organic remains, find a simple explanation through his experiments. Granted only a temperature about 1000°C. and sufficient pressure to retain the carbonic acid evolved (less than 1000 feet of average rock) any limestone will recrystallise completely. As a matter of fact, there is little evidence that the complete fusion of limestone is a common phenomenon, and a liquid limestone magma sending intrusive veins into the surrounding rocks has only seldom been postulated. The Huttonians thought that the calcareous amygdales of many of the basaltic lavas were fragments of limestone that had been involved in the igneous rock and completely fused, but this is no longer believed. Furthermore, Sir James Hall proved that under the same conditions limestone would react on silica, forming silicates, and would attack glass, porcelain, pipe-clay, and the material of his pyrometric cones; thus he explained the origin of accessory minerals of many limestones, such as wollastonite, garnet, vesuvianite, diopside, and scapolite. Edinburgh geologists, for example, know well the altered limestone which occurs at the margin of the teschenite-picrite sill at Davidson's Mains railway station. There is no need to believe that it was ever completely melted, and the preservation of many traces of the original bedding makes it very improbable that complete fusion took place.

Recrystallisation and the growth of crystals *in solido* were observed also by many of those who endeavoured to repeat Sir James Hall's experiments during the nineteenth century, and have been fully confirmed by more recent researches. In fact this process is now regularly applied in the investigation of minerals that refuse to crystallise well from igneous melts or undergo transformation into other forms below a certain transition temperature. From the pure chemical components a glass is prepared as homogeneous and free from bubbles as possible, and this glass is then heated for many hours to a temperature below its melting-point, but within the field of stability of the crystalline form which it is desired to investigate. Crystals are thus produced which may be sufficiently large to have their optical characters, cleavage, hardness, and other properties satisfactorily determined. This is, of course, a case of devitrification, a process which Hall was familiar with, as he had studied it in the glass furnace at Leith which first suggested to him the advisability of making furnace experiments on rocks. He recognised it also in certain varieties of porcelain which he had employed in his experiments. But even when devitrification is sensibly complete and a finely crystalline aggregate replaces the original glass the process will go on, and the crystals become larger and larger if subjected for a considerable time to a temperature not far below the fusion point.

It was characteristic of Hall that having set himself an object he pursued it with undeviating persistence. For four years he continued his experiments on the crystallisation of the carbonate of lime by heat modified by compression. In that time he made nearly five hundred experiments, and considering how elaborate they were and how all his apparatus was made by himself or by ordinary mechanics we can see that little time was left for the ordinary pursuits of a country

gentleman. A few subsidiary investigations, however, received attention, one being the action of organic matter when heated under pressure, including the formation of coal and the origin of the bituminous materials found where igneous rocks are intrusive into coal seams or beds of shale rich in organic matter. The other was the action of carbonate of lime on 'silex.'

It is not quite clear what the 'silex' was, as Hall employs the term for the material of which his Wedgwood porcelain tubes were made, while others used it to designate precipitated silica and various siliceous minerals. If it were porcelain, Hall was experimenting with the system $CaO\text{-}Al_2O_3\text{-}SiO_2$ on which the beautiful researches of Rankin and Wright executed at the Geophysical Laboratory in Washington were published in 1915. This work may be taken as an example of the highest type of investigations of the class which Hall initiated. About 7000 individual tests were made. The complete ternary diagram contains fourteen separate stability fields, each for a definite chemical compound. Some of these are well-known minerals such as cristobalite, tridymite, sillimanite, anorthite, but many are new compounds, or minerals under forms which do not occur in rocks (such as pseudo-wollastonite). In addition to the three components there are nine binary compounds; three are ternary, but of these only two are stable. Between the stability fields are thirty boundary lines, which show under what conditions two minerals may exist simultaneously in the presence of liquid melt of a definite composition. The fields meet three together, in twenty-one quintuple points, eight of which are ternary eutectics, while thirteen are transition points. The lowest temperature at which liquids appear is 1170°C. ± 5°; no possible mixture of these three substances is completely fused below that temperature.

Many binary systems and quite a number of ternary systems have now been explored, some of them very fully. The seed which Hall planted is growing into a mighty tree. It is bearing fruit most precious to petrologists, mineralogists, and physical chemists. The conditions under which certain minerals can form in igneous melts are being gradually determined. But as yet the results appeal to the mineralogist and physical chemist rather than to the geologist. Quartz, tridymite, calcite, pyrites, corundum, and other common minerals of rocks have now had their stability conditions determined when they occur in dry fusions at atmospheric pressure in presence of a limited number of other substances. The accuracy of the determinations is marvellous, and has been confirmed in many cases by independent investigations along different lines. These researches are of even more value to the technologist than to the geologist. In the system above mentioned, for instance, there are only three or four minerals among the compounds determined in the melts. But the whole system and all its compounds have a bearing on practical problems. The three pure substances, for example, are well-known refractories: silica, alumina, and lime. Silica is the material of the quartz-glass industry; alumina forms alundum, an abrasive and a valuable refractory, while the uses of lime are too many to mention. Silica brick is principally quartz and tridymite with a little lime as a bond; ganister brick is mainly silica

and alumina; fireclay contains more alumina with a small and variable amount of alkalis. Portland cement is a mixture of silica, alumina, and lime. All these manufactures are produced by pure dry fusion under atmospheric pressure, and exactly under the conditions and temperatures of the experiments on which the diagram is founded. The presence of small amounts of impurities in the natural minerals employed introduces complications, but these may be neglected if only approximate results are aimed at. During the War the highly trained technical skill of the workers of the Geophysical Institute and their refined apparatus were entirely at the service of American industries, such as the manufacture of optical and chemical glass, and all the practical problems that arose were promptly and satisfactorily solved.

The investigation of the system $Ca\text{-}Al_2O_3\text{-}SiO_2$ which we have taken as an example of the best type of modern work in this field of research is a great contribution to theoretical petrology. It has bearings on the thermal alteration of many rocks such as quartzites, flints, pure limestones, siliceous and argillaceous limestones, bauxites, fireclays, calcareous quartzites, all of which may be regarded as mixtures of silica, clay, and (carbonate of) lime together with their alteration products. But for the geologist as a rule the matter is not quite so simple, and caution is necessary in drawing inferences. Three of the commonest alteration products in this group of rocks, for example, are biotite, garnet, and andalusite, and these minerals have been produced experimentally only under very exceptional conditions.

There are differences between the conditions of the experiments and those that actually obtain in the making of rocks, and these are essentially of three kinds:

(*a*) Experimental work is successful only when the conditions are exactly defined, and necessity compels us at present to restrict experimental work to simple systems of two or three components. The theory of these has been very fully worked out, and this is essential to the interpretation of the experimental results. Systems of four components have hardly yet been touched. If we take, for example, the four common oxides of rocks, CaO, Al_2O_3, MgO, and SiO_2, the six possible binary systems are pretty well known, and the four possible ternary systems have also been thoroughly studied, but little progress has yet been made with the investigation of quaternary mixtures containing all four components. The mathematics of such a system is of the most complex description. Now the common rocks contain seven or more components, and their behaviour in igneous melts is a problem which is at present beyond solution.

To simplify matters we might investigate such a system piecemeal, that is to say, we might take parts of it and treat them as independent systems. For example, the three minerals anorthite, forsterite, quartz, which consist of these four components, have been investigated, and it was proved that this could not be regarded as a simple three-component system, as under certain conditions phenomena appeared which characterised a quaternary mixture. Along these lines, however, there is no doubt that great progress can be made, and the results already obtained are so valuable that they hold out great promise for the future.

(*b*) The only igneous rocks that consolidate from high temperatures at atmospheric pressures with free escape of contained gases are the volcanic lavas. The plutonic and intrusive rocks consolidate under high pressures and with retention of their gases. Hall began his experiments with dry melts in open furnaces; but he realised that under these conditions it was not possible to crystallise a marble. The historical development of research has followed similar lines, and investigations under pressure are now becoming more prominent. In the special field in which Hall worked we owe very important results to Professors Adams and Nicolson, of Montreal. They experimented on the effects of very high pressure (obtained by a hydraulic press) on chalk, limestone, and marble. Columns of limestone were embedded in alum or fusible metal enclosed in a steel tube, and submitted to enormous pressures. In some of the experiments the apparatus was heated to 300° or 400°C., and that the investigation was on 'the effects of heat modified by compression' as stated in the title of Hall's original paper of 1805. They succeeded in obtaining plastic deformation in the solid rock, with development of schistosity and cataclastic structures but without extensive recrystallisation. These experiments illustrate very perfectly the formation of such rocks as calc-schists, mylonites, flaser-gabbros, and augen-gneisses.

It is generally agreed by physicists that increase of pressure makes little difference on the melting-points of solids, and that a slight rise of temperature may have a much greater effect on the stability of a mineral system than a considerable rise of pressure. But this is to some extent altered where volatile substances are concerned, for then pressure modifies the concentration, often to a high degree. In many rocks, and especially the acid plutonic rocks and mineral veins, the importance of volatile mineralisers is abundantly clear. In the crystalline schists, on the other hand, we see the effects of pressure, not only in the structures of the rock masses, but also in the special minerals which characterise this group. The importance, accordingly, of pressure and volatile components cannot be ignored, and experimental petrologists are now directing their attention especially to a study of their influence. The ground has been cleared by a masterly series of mathematical researches, principally by Schreinemakers and Smits, and experimental work along these lines is rapidly advancing.

(*c*) The third agency which nature employs in the making of rocks but is apt to be neglected in the laboratory is *time*. It is not always easy to estimate its importance. A laboratory experiment under exceptional circumstances has been carried on over several months; most of them are finished in a few hours, but nature works with unlimited time. The action of solvents when they occur in very small quantities is favoured in this way and unstable phases tend to disappear. In the deposition of mineral veins this may be a factor of paramount importance, and it also cannot be ignored in all studies of metamorphism and metasomatism.

We learn from Hall's papers that he was continually experimenting, and he delighted to devise means to put geological theories to practical tests.

In 1812, when he was President of the Royal Society of Edinburgh, he read a paper to the Society on ' The Contortions of the Strata.' He described the Silurian rocks of the Berwickshire coast as having been thrown into folds, a great part of which had been removed by denudation. Similar phenomena had been noted in many other places, and many explanations had been offered to account for the tilted, bent, upturned, and distorted rocks. Some geologists held they had been deposited in that position, others that they had been upheaved by earthquakes, or let down by subsidences. Hall's explanation was very simple—the rocks had been affected by lateral pressure. With some pieces of cloth and a door ' which happened to be off its hinges,' and a few stones to act as weights, he was able to reproduce the ' contortions of strata ' experimentally with great perfection. The whole proceeding was so simple that one is reminded of Columbus and the egg. Yet if we are to judge by the discussions in contemporary literature, folding of strata had not yet been recognised, and this suggestion was revolutionary. It contains the germ of many theories of mountain building; no one now doubts that lateral pressure is one of the most powerful agencies in the disturbance of the earth's crust and the production of many special types of rocks. In no district is this better exemplified than in the North-West Highlands of Scotland.

As the source of lateral pressure Hall suggested that igneous intrusions making their way upwards might force asunder the adjacent rocks. This would not now be generally accepted, but of course it was an explanation very likely to occur to a Huttonian. It seems to have been about forty years later that Elie de Beaumont and his school brought forward the hypothesis that secular contraction of the earth's crust might produce lateral compression of rock masses, and might be the cause of folding and of mountain building. The rise of modern theories of mountain structure probably dates from the investigations of H. D. Rogers on the Appalachians.

Hall's final contribution to experimental geology appears in a paper which he read to the Royal Society of Edinburgh in April 1825, and was published in the tenth volume of the ' Transactions.' The title is ' On the Consolidation of the Strata of the Earth.' Modern geology by that time had made great progress, and many of the controversies of Hall's early years had been settled. But Hall remained essentially a Huttonian in his belief in the efficacy of plutonic heat. He aimed at showing that submarine intrusions would consolidate loose overlying beds of sand into firm sandstone. For this purpose he took salt water, or concentrated brine, and heated it in crucibles or iron vessels containing a quantity of sand. He found that it was possible to make the bottom of such an iron vessel red hot, while the brine on top was so cold that the hand could be inserted into it. The sand was in some cases converted into firm coherent mass, no doubt by the action of alkalis at a red heat. It is difficult to perceive what such an experiment proves; it may possibly have some bearing on the induration of sandstones by contact alteration in the vicinity of intrusive sills, and one of the special cases to which Hall refers as suggesting this experiment was the hardening of conglomerate by intrusive dykes. What actually

happened was the production of a vitreous cement, consisting of silicates of alumina and the alkalis, sufficient to bind together the sand grains. We are reminded of the fused Torridon sandstone that is found at the margins of Tertiary dykes in the western isles of Scotland, but in these the alkali was furnished by the felspar originally present in the sandstone. This paper probably contains the last expression of the pure Huttonian philosophy. Hall was now the sole survivor of the original group who established the Huttonian theories. Having begun as an innovator and a radical, regarded askance for his revolutionary tendencies, he was now a conservative holding fast to orthodox opinions, even when they were out of date. His passion for experiment lasted to the end, and he seems to have maintained his furnace in working order for over forty years. He died in 1832. Although he was not the greatest of the trio—Hutton, Playfair, and Hall—who founded modern geology, he was worthy to take his place with the others. Hutton's was the original master-mind who, by sheer induction and abstract reasoning, had read the secrets of the earth. Playfair was the man of balanced judgment who grasped the essentials and placed them in convincing clearness before a sceptical public. Hall had his special field of work in which he excelled all his contemporaries, and for us who are watching with profound interest the rapid progress of experimental investigation into geophysical and petrophysical problems, it is not uncongenial to pay a tribute to his memory.

SOME PROBLEMS IN EVOLUTION.

ADDRESS TO SECTION D (ZOOLOGY) BY

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PRESIDENT OF THE SECTION.

It was nearly 100 years ago that Charles Darwin began his scientific studies in the University of Edinburgh. In this illustrious centre of intellectual activity he met various friends keenly interested in natural history, and attended the meetings of scientific societies, and it was doubtless here that were sown many of the seeds destined to bear such glorious fruit many years later. No more fitting subject, I think, could be found for an address than certain problems relating to his doctrine of evolution. That controversy perpetually rages round it is a healthy sign. For we must take care in science lest doctrine should pass into dogma, unquestioned and accepted merely on authority. So from time to time it is useful to re-examine in the light of new knowledge the very foundations on which our theories are laid.

Perhaps the best way of treating these general subjects is by trying to answer some definite questions. For instance, we may ask: 'Why are some characters inherited and others not?' By characters we mean all those qualities and properties possessed by the organism, and by the enumeration of which we describe it: its weight, size, shape, colour, its structure, composition and activities. Next, what do we mean by 'inherited'? It is most important, if possible, clearly to define this term, since much of the controversy in writings on evolution is due to its use by various authors with a very different significance—sometimes as mere reappearance, at other times as actual transmission or transference from one generation to the next. Now, I propose to use the word inheritance merely to signify the reappearance in the offspring of a character possessed by the ancestor—a fact which may be observed and described, regardless of any theory as to its cause. Our question, then, is: 'Why do some characters reappear in the offspring and others not?'

It is sometimes asserted that old-established characters are inherited, and that newly-begotten ones are not, or are less constant, in their reappearance. This statement will not bear critical examination. For, on the one hand, it has been conclusively shown by experimental breeding that the newest characters may be inherited as constantly as the most ancient, provided they are possessed by both parents.[1] While, on the other hand, few characters in plants can be older than the green colour due to chlorophyll, yet it is sufficient to cut off the light from a germinating seed for the greenness to fail to appear. Again, ever since Devonian

[1] We purposely set aside complications due to hybridisation and Mendelian segregation, which do not directly bear on the questions at issue.

times vertebrates have inherited paired eyes; yet, as Professor Stockard has shown, if a little magnesium chloride is added to the sea-water in which the eggs of the fish Fundulus are developing, they will give rise to embryos with one median cyclopean eye! Nor is the suggestion any happier that the, so to speak, more deep-seated and fundamental characters are more constantly inherited than the trivial or superficial. A glance at organisms around us, or the slightest experimental trial, soon convinces us that the apparently least-important character may reappear as constantly as the most fundamental. But while an organism may live without some trivial character, it can rarely do so when a fundamental character is absent, hence such incomplete individuals are seldom met in Nature.

Yet undoubtedly some characters reappear without fail and others do not. If it is neither age nor importance, what is it that determines their inheritance? The answer is that for a character to reappear in the offspring it is essential that the germinal factors and the environmental conditions which co-operated in its formation in the ancestor should both be present. Inheritance depends on this condition being fulfilled. For all characters are of the nature of responses to environment;[2] they are the products or results of the interaction between the factors of inheritance (germinal factors) and the surrounding conditions or stimuli. This power of response or reaction is no mysterious property of organisms—it is the effect produced, the disturbance brought about by the application of a stimulus. All the special properties and activities of living organisms ultimately depend on their metabolism, of which growth and reproduction are the chief manifestations. The course of metabolism, and, consequently, the development in the individual of a character, is moulded or conditioned by the environmental stimuli under which it takes place. On the other hand, the living substance, protoplasm, which is undergoing metabolism is the material basis of the organism. It has a specific composition and structure peculiar to the particular kind of organism concerned, and this is handed on to the offspring in the germ-cells from which starts the new generation. The inheritance of a character is due, then, not only to the actual transmission or transference of this specific 'germ-plasm' containing the same factors of inheritance (germinal factors) as those from which the parent developed, but also to this factorial complex developing under the same conditions (environmental stimuli), as those under which the parent developed. Any alteration either in the effective environmental stimuli or the germinal factors will produce a new result, will give rise to a new character, will cause the old character to appear no longer.

Now what is actually transmitted from one generation to the next is the complex of germinal factors. Hence we should carefully distinguish between transmission and inheritance. Much of the endless

[2] In a letter to *Nature* Sir Ray Lankester long ago drew attention to the importance of this consideration when discussing inheritance. He also pointed out that Lamarck's first law, that a new stimulus alters the characters of an organism, contradicts his second law, that the effects of previous stimuli are fixed by inheritance. (*Nature*, vol. li. 1894.)

confusion and interminable controversies about the inheritance of so-called 'acquired characters' is due to the neglect of this important distinction. For it is quite clear that whereas factors may be transmitted, characters as such never are. The characters of the adult, being responses, are not present as such in the fertilised ovum from which it develops, they are produced anew at every generation.[3] No distinction in kind or value can be drawn between characters.

If some are inherited regularly and others are not, the distinction lies not in the nature or mode of production of the characters themselves, but in the constancy of the factors and conditions which give rise to them. Thus, although there is only one kind of character, there are two kinds of variation.

Much of the confusion in evolutionary literature is, I think, due to the use of the word variation in a loose manner. Sometimes it is taken to mean the degree of divergence between two individuals; sometimes the character itself in which they differ, such as a colour or spot on a butterfly's wing, at other times a variety or race differing from the normal form of the species. If clearness of thought and expression is to be attained, the word variation should mean the extent or degree of difference between two individuals or between an individual and the average of the species, the divergence of the new form from the old; not a new character or assemblage of characters, but a difference which can be measured or at least estimated. We shall then find that a variation is of one of two kinds (which may, of course, be combined): the first kind is due to some change in the complex of effective environmental stimuli, the second to some change in the complex of germinal factors.

The second kind, to which the name mutation has been applied, will, under constant conditions, be inherited since the new complex of factors will be transmitted to subsequent generations. The first kind of variation, which has been called a modification, will also be inherited, provided, of course, the change of stimulus persists. In either case, new characters will result. But here, again, we must be careful not to apply the terms mutation and modification to the characters themselves as is so often done;[4] for we then reintroduce the confusion already exposed in the popular but misleading distinction between 'acquired' and 'non-acquired' characters. The characters due to mutation or modification are, of course, indistinguishable by mere inspection, and can only be separated by experiment. A mutation once established should give rise, under uniform conditions, to a new heritable character, and may be detected by crossing with normal members of the species.

[3] In other words, all characters are 'acquired during the lifetime of the individual,' and 'inherited' in the sense here defined has just the same meaning. Much the same view was advocated by Professor A. Sedgwick in his address to this Section at Dover in 1899, and it has also been developed by Dr. Archdall Reid and others.

[4] The name 'mutation' might be given to the alteration in the factors instead of the variation due to it. The latter might then be termed a mutational variation and would be opposed to a modificational variation At present the term 'mutation' is applied to three different things: the factorial change, the variation or difference, and the new product response or character.

So far observations and tests have shown that new characters due to modification only reappear so long as the new stimulus persists. The difference lies not in the value or permanence of the new character, but in the causes which give rise to it.[5]

It is little more than a platitude to state that, for the production of an organism or of any of its characters, both germinal factors and environmental stimuli are necessary, and that if evolution is to take place there must be change in one or both. Yet the changes in the factors may be held to be the more important. In an environment which on the whole alters but little, evolution progresses by the cumulation along diverging lines of adaptation of new characters due to mutation. Thus natural selection indirectly preserves those factorial complexes which respond in a favourable manner. In other words, an organism, to survive in the struggle for existence, must present that assemblage of factors of inheritance which, under the existing environmental conditions, will give rise to advantageous characters.

In answer to a further question, let us now try to explain what we mean when we contrast the organism with its environment. In its simplest and most abstract form a living organism may be likened to a vortex. That mixture of highly complex proteins we call protoplasm, the physical basis of life, is perpetually undergoing transformations of matter and energy, so long as life persists. Towards the centre of the vortex the highest compounds are continually being built up and continually being broken down; new material (food, water, oxygen) and energy are brought in at the periphery, and old material and energy (work and heat) thrown out. The principle of the conservation of energy and matter holds good in organised living processes as it does in the inorganic world outside. This is the process we call metabolism, and it is at the base of all the manifestations of life. From the point of view of biological science life is founded on a complex and continuous physico-chemical process of endless duration so long as conditions are favourable; just as a fire will continue to burn so long as fuel is at hand. No one step, no single substance, can be said to be living: the whole chain of substances and reactions, every link of which is essential, constitutes the life-process. A stream of non-living matter with stored-up energy is built up into the living vortex, and again passes out as dead matter, having yielded up the energy necessary for the performance of the various activities of the organism. If more is taken in than is given out it will grow and sub-divide. The complexity of the organism may increase by the formation of subsidiary, more or less interdependent, vortices within it. The perpetual growth and transmission of factors of inheritance, the continuity of the germ-plasm, is but another aspect of the continuity of the metabolic process forming the basis of the continuity of life in evolution.

[5] We might perhaps distinguish the two cases by calling them constant and inconstant characters, or 'natural' and 'acquired,' as is commonly done when describing immunity. It should be meant thereby that one is acquired usually (under normal conditions), the other occasionally (when infection occurs). Error creeps in when the term 'acquired' is opposed to 'non-acquired' or to 'inherited.'

But all environmental stimuli are not external to the organism. Just as the various steps in the metabolic process are dependent on those which preceded them, so when an organism becomes differentiated into parts, when the main process becomes sub-divided into subsidiary ones, these react on each other. What is internal to the whole becomes external to the part. An external stimulus may set up an internal metabolic change, giving rise to a response whose extent and nature depend on the structure of the mechanism and its state when stimulated, that is to say, on the effect of previous responses. Such a response may act as an internal stimulus giving rise to a further response, which may modify the first, and so on. Parts thus become marvellously fitted to set going, inhibit, or regulate each other's action; and thus arises that power of individual adaptation, or self-regulation, so characteristic of living organisms. The processes of temperature regulation, of respiration, of excretion are examples of such delicate self-regulating mechanisms in ourselves. But one of the great advantages thereby gained by organisms is that they can regulate their own growth and ensure their own 'right' development. Whereas the simplest plants and animals are to a great extent, so to speak, at the mercy of their external environment, except in so far as they can move from unfavourable to more favourable surroundings; whereas their characters appear in response to external stimuli which may or may not be present, and over which they have little or no control—the higher organisms (more especially the higher animals), as it were, gradually substitute internal for external stimuli. Food material is provided in the ovum, and the size, structure and time of appearance of various characters are regulated to a great extent by use and by the secretions of various endocrinal glands, the action of which has been so successfully studied, among others, by Sir Sharpey Schafer in this University. Thus, as is well shown in man, the higher animals acquire considerable independence, and are little affected in their development by minor changes of environment. Inheritance is thus made secure by ensuring that the necessary conditions are always present.

We may seem to have wandered far from our original question; but the answer now appears to be that only those characters can be regularly inherited which depend for their appearance on conditions always fulfilled in the normal environment (external or internal); and those characters will not be regularly inherited which depend on stimuli that may or may not be present. Thus, while the offspring of a dark-skinned race will be dark in whatever climate they are born, those of a fair-skinned race will be born fair, but may be darkened by sun-burn, if they spend their holiday in the open.

Now it will be said, and not without some truth, that all this is mere commonplace admitted by all; but, if so, it is, I think, often ignored or misunderstood in discussions on heredity, more especially in semi-popular writings, and sometimes even in scientific works However, I quite willingly admit that the real problems Darwin left to be solved by the evolutionist are the nature of the germinal factors themselves, and more especially the origin of the differences between them, the origin of those changes which give rise to mutations.

That these factors[6] must at least be self-propagating substances, subsidiary vortices in the main stream of metabolising living protoplasm, is certain, since they grow and multiply repeatedly, to be distributed to new generations of germ-cells. That they may be relatively constant and remain unaltered for generations seems also certain, since organisms or their parts can continue almost unchanged for untold ages. That they can act independently, can be separately distributed into different germ-cells, and can be re-combined seems likewise to have been proved by the brilliant work of Mendel and his followers. So independent and constant do they appear to be that modern students of heredity tend to treat them as so many beads in a row, as separate particles themselves endowed with all the properties of independent living organisms, the very properties we wish to explain While not prepared to accept these views without qualification, it seems to me that it can scarcely be doubted that some such units must exist whether in the form of discrete particles or merely of separable substances. But not until these factors have been brought into relation with the general metabolism of the organism, as links in the chain of processes, will the problem of inheritance approach solution. If the theory is to be completed it must attempt to explain how they come to differ, how their orderly behaviour is regulated, in what functional relation they stand to each other, what is the metabolic bond between them. That harmonious processes may be carried out by discrete elements in co-operation is shown in cases of symbiotic combinations such as the lichens, or the green algæ in such animals as Hydra and Convoluta. Here an originally independent organism takes its place and does its work regularly in another organism, and may even be propagated and transmitted from one generation to the next in the germ-cell! Most instructive, also, are the recently studied cases of bacteria and yeasts living regularly in certain special tissues of various species of insects, where they exert a definite influence on the metabolism (see the works of Pierantoni, Buchner, Glaser). These no doubt are mere analogies, but they serve.

In all probability, then, factors of inheritance exist, and the fundamental problem of Biology is how are the factors of an organism changed, or how does it acquire new factors? In spite of its vast importance, it must be confessed that little advance has been made towards the solution of this problem since the time of Darwin, who considered that variation must ultimately be due to the action of the environment. This conclusion is inevitable, since any closed system will reach a state of equilibrium and continue unchanged, unless affected from without. To say that mutations are due to the mixture or reshuffling of pre-existing factors is merely to push the problem a step

[6] Herbert Spencer's 'physiological units,' Darwin's 'pangens,' Weismann's 'determinants,' are all terms denoting factors, but with somewhat different meanings. More recently Professor W. Johannsen (*Elemente der exakten Erblichkeitslehre*, 1909) has proposed the term 'gene' for a factor, 'genotype' for the whole assemblage of factors transmitted by a species, and 'phenotype' for the characters developed from them. This clear system of nomenclature, although much used in America, has not been generally adopted in this country.

farther back, for we must still account for their origin and diversity. The same objection applies to the suggestion that the complex of factors alters by the loss of certain of them. To account for the progressive change in the course of evolution of the factors of inheritance and for the building up of the complex it must be supposed that from time to time new factors have been added; it must further be supposed that new substances have entered into the cycle of metabolism, and have been permanently incorporated as self-propagating ingredients entering into lasting relation with pre-existing factors. We are well aware that living protoplasm contains molecules of large size and extraordinary complexity, and that it may be urged that by their combination in different ways, or by the mere regrouping of the atoms within them, an almost infinite number of changes may result, more than sufficient to account for the mutations which appear. But this does not account for the building up of the original complex. If it must be admitted that such a building process once occurred, what right have we to suppose that it ceased at a certain period? We are driven, then, to the conclusion that in the course of evolution new material has been swept from the banks into the stream of germ-plasm.

If one may be allowed to speculate still further, may it not be supposed that factors differ in their stability?—that whereas the more stable are merely bent, so to speak, in this or that direction by the environment, and are capable of returning to their original condition, as a gyroscope may return to its former position when pressure is removed, other less stable factors may be permanently distorted, may have their metabolism permanently altered, may take up new substance from the vortex, without at the same time upsetting the system of delicate adjustments whereby the organism keeps alive? In some such way we imagine factorial changes to be brought about and mutations to result.

Let it not be thought for a moment that this admission that factors are alterable opens the door to a Lamarckian interpretation of evolution! According to the Lamarckian doctrine, at all events in its modern form, a character would be inherited after the removal of the stimulus which called it forth in the parent. Now of course, a response once made, a character once formed, may persist for longer or shorter time according as it is stable or not; but that it should continue to be produced when the conditions necessary for its production are no longer present is unthinkable. It may, however, be said that this is to misrepresent the doctrine, and that what is really meant is that the response may so react on and alter the factor as to render it capable of producing the new character under the old conditions. But is this interpretation any more credible than the first?

Let us return to the possible alteration of factors by the environment. Unfortunately there is little evidence as yet on this point. In the course of breeding experiments the occurrence of mutations has repeatedly been observed, but what led to their appearance seems never to have been so clearly established as to satisfy exacting critics. Quite lately, however, Professor M. F. Guyer, of Wisconsin, has brought forward a most interesting case of the apparent alteration at will of a

factor or set of factors under definite well-controlled conditions.[7] You will remember that if a tissue substance, blood-serum for instance, of one animal be injected into the circulation of another, this second individual will tend to react by producing an anti-body in its blood to antagonise or neutralise the effect of the foreign serum. Now Professor Guyer's ingenious experiments and results may be briefly summarised as follows. By repeatedly injecting a fowl with the substance of the lens of the eye of a rabbit he obtained anti-lens serum. On injecting this 'sensitised' serum into a pregnant female rabbit it was found that, while the mother's eyes remained apparently unaffected, some of her offspring developed defective lenses. The defects varied from a slight abnormality to almost complete disappearance. No defects appeared in untreated controls, no defects appeared with non-sensitised sera. On breeding the defective offspring for many generations these defects were found to be inherited, even to tend to increase and to appear more often. When a defective rabbit is crossed with a normal one the defect seems to behave as a Mendelian recessive character, the first generation having normal eyes and the defect reappearing in the second. Further, Professor Guyer claims to have shown that the defect may be inherited through the male as well as the female parent, and is not due to the direct transmission of anti-lens from mother to embryo *in utero*.

If these remarkable results are verified, it is clear that an environmental stimulus, the anti-lens substance, will have been proved to affect not only the development of the lens in the embryo, but also the corresponding factors in the germ-cells of that embryo; and that it causes, by originating some destructive process, a lasting transmissible effect giving rise to a heritable mutation.

Professor Guyer, however, goes farther, and argues that, since a rabbit can also produce anti-lens when injected with lens substance, and since individual animals can even produce anti-bodies when treated with their own tissues, therefore the products of the tissues of an individual may permanently affect the factors carried by its own germ-cells. Moreover he asks, pointing to the well-known stimulative action of internal secretions (hormones and the like), if destructive bodies can be produced, why not constructive bodies also? And so he would have us adopt a sort of modern version of Darwin's theory of Pangenesis, and a Lamarckian view of evolutionary change.

But surely there is a wide difference between such a poisonous or destructive action as he describes and any constructive process. The latter must entail, as I tried to show above, the drawing of new substances into the metabolic vortex. Internal secretions are themselves but characters, products (perhaps of the nature of ferments) behaving as environmental conditions, not as self-propagating factors, moulding the responses, but not permanently altering the fundamental structure and composition of the factors of inheritance.

Moreover, the early fossil vertebrates had, in fact, lenses neither larger nor smaller on the average than those of the present day. If

[7] *American Naturalist*, vol. lv. 1921; *Jour. of Exper. Zoology*, vol. xxxi. 1920.

destructive anti-lens had been continually produced and had acted, its effect would have been cumulative. A constructive substance must, then, have also been continually produced to counteract it. Such a theory might perhaps be defended; but would it bring us any nearer to the solution of the problem?

The real weakness of the theory is that it does not escape from the fundamental objections we have already put forward as fatal to Lamarckism. If an effect has been produced, either the supposed constructive substance was present from the first, as an ordinary internal environmental condition necessary for the normal development of the character, or it must have been introduced from without by the application of a new stimulus. The same objection does not apply to the destructive effect. No one doubts that if a factor could be destroyed by a hot needle or picked out with fine forceps the effects of the operation would persist throughout subsequent generations.

Nevertheless, these results are of the greatest interest and importance, and, if corroborated, will mark an epoch in the study of heredity, being apparently the first successful attempt to deal experimentally with a particular factor or set of factors in the germ-plasm.

There remains another question we must try to answer before we close, namely, 'What share has the mind taken in evolution?' From the point of view of the biologist, describing and generalising on what he can observe, evolution may be represented as a series of metabolic changes in living matter moulded by the environment. It will naturally be objected that such a description of life and its manifestations as a physico-chemical mechanism takes no account of mind. Surely, it will be said, mind must have affected the course of evolution, and may indeed be considered as the most important factor in the process. Now, without in the least wishing to deny the importance of the mind, I would maintain that there is no justification for the belief that it has acted or could act as something guiding or interfering with the course of metabolism. This is not the place to enter into a philosophical discussion on the ultimate nature of our experience and its contents, nor would I be competent to do so; nevertheless, a scientific explanation of evolution cannot ignore the problem of mind if it is to satisfy the average man.

Let me put the matter as briefly as possible at the risk of seeming somewhat dogmatic. It will be admitted that all the manifestations of living organisms depend, as mentioned above, on series of physico-chemical changes continuing without break, each step determining that which follows; also that the so-called general laws of physics and of chemistry hold good in living processes. Since, so far as living processes are known and understood, they can be fully explained in accordance with these laws, there is no need and no justification for calling in the help of any special vital force or other directive influence to account for them. Such crude vitalistic theories are now discredited, but tend to return in a more subtle form as the doctrine of the interaction of body and mind, of the influence of the mind on the activities of the body. But, try as we may, we cannot conceive how a physical process can be interrupted or supplemented by non-physical agencies.

Rather do we believe that to the continuous physico-chemical series of events there corresponds a continuous series of mental events inevitably connected with it; that the two series are but partial views or abstractions, two aspects of some more complete whole, the one seen from without, the other from within, the one observed, the other felt. One is capable of being described in scientific language as a consistent series of events in an outside world, the other is ascertained by introspection, and is describable as a series of mental events in psychical terms. There is no possibility of the one affecting or controlling the other, since they are not independent of each other. Indissolubly connected, any change in the one is necessarily accompanied by a corresponding change in the other. The mind is not a product of metabolism as materialism would imply, still less an epiphenomenon or meaningless by-product as some have held. I am well aware that the view just put forward is rejected by many philosophers, nevertheless it seems to me to be the best and indeed the only working hypothesis the biologist can use in the present state of knowledge. The student of biology, however, is not concerned with the building up of systems of philosophy, though he should realise that the mental series of events lies outside the sphere of natural science.

The question, then, which is the more important in evolution, the mental or the physical series, has no meaning, since one cannot happen without the other. The two have evolved together *pari passu*. We know of no mind apart from body, and have no right to assume that metabolic processes can occur without corresponding mental processes, however simple they may be.

Simple response to stimulus is the basis of all behaviour. Responses may be linked together in chains, each acting as a stimulus to start the next; they can be modified by other simultaneous responses, or by the effects left behind by previous responses, and so may be built up into the most complicated behaviour. But, owing to our very incomplete knowledge of the physico-chemical events concerned, we constantly, when describing the behaviour of living organisms, pass, so to speak, from the physical to the mental series, filling up the gaps in our knowledge of the one from the other. We thus complete our description of behaviour in terms of mental processes we know only in ourselves (such as feeling, emotion, will) but infer from external evidence to take place in other animals.

In describing a simple reflex action, for instance, the physico-chemical chain of events may appear to be so completely known that the corresponding mental events are usually not mentioned at all, their existence may even be denied. On the contrary, when describing complex behaviour when impulses from external or internal stimuli modify each other before the final result is translated into action, it is the intervening physico-chemical processes which are unknown and perhaps ignored, and the action is said to be voluntary or prompted by emotion or the will.

The point I wish to make, however, is that the actions and behaviour of organisms are responses, are characters in the sense described in the earlier part of this address. They are inherited, they

vary, they are selected, and evolve like other characters. The distinction so often drawn by psychologists between instinctive behaviour said to be inherited and intelligent behaviour said to be acquired is as misleading and as little justified in this case as in that of structural characters. Time will not allow me to develop this point of view, but I will only mention that instinctive behaviour is carried out by a mechanism developed under the influence of stimuli, chiefly internal, which are constantly present in the normal environmental conditions, while intelligent behaviour depends on responses called forth by stimuli which may or may not be present. Hence, the former is, but the latter may or may not be inherited. As in other cases, the distinction lies in the factors and conditions which produce the results. Instinctive and intelligent behaviour are usually, perhaps always, combined, and one is not more primitive or lower than the other.

It would be a mistake to think that these problems concerning factors and environment, heredity and evolution, are merely matters of academic interest. Knowledge is power, and in the long run it is always the most abstruse researches that yield the most practical results. Already, in the effort to keep up and increase our supply of food, in the constant fight against disease, in education, and in the progress of civilisation generally, we are beginning to appreciate the value of knowledge pursued for its own sake. Could we acquire the power to control and alter at will the factors of inheritance in domesticated animals and plants, and even in man himself, such vast results might be achieved that the past triumphs of the science would fade into insignificance.

Zoology is not merely a descriptive and observational science, it is also an experimental science. For its proper study and the practical training of students and teachers alike, well-equipped modern laboratories are necessary. Moreover, if there is to be a useful and progressive school contributing to the advance of the science, ample means must be given for research in all its branches. Life doubtless arose in the sea, and in the attempt to solve most of the great problems of biology the greatest advances have generally been made by the study of the lower marine organisms. It would be a thousand pities, therefore, if Edinburgh did not avail itself of its fortunate position to offer to the student opportunities for the practical study of marine zoology.

In his autobiography, Darwin complains of the lack of facilities for practical work—the same need is felt at the present time. He would doubtless have been gratified to see the provision made since his day and the excellent use to which it has been put; but what seems adequate to one generation becomes insufficient for the next. We earnestly hope that any appeal that may be made for funds to improve this Department of Zoology may meet with the generous response it certainly deserves.

APPLIED GEOGRAPHY.

ADDRESS BY SECTION E (GEOGRAPHY) BY

D. G. HOGARTH, M.A., D.Litt., C.M.G.,

PRESIDENT OF THE SECTION.

The term which I have taken for the title of my address has been in use for some years as a general designation of lendings or borrowings of geographical results, whether by a geographer who applies the material of his own science to another, or by a geologist or a meteorologist, or again an ethnologist or historian, who borrows of the geographer. Whether Geography makes the loan of her own motion or not, the interest in view, as it seems to me, is primarily that, not of Geography, but of another science or study. The open question whether that interest will be served better if the actual application be made by the geographer or by the other scientist or student does not concern us now.

Such applications are of the highest interest and value as studies, and, still more, as means of education. As studies, not merely are they links between sciences, but they tend to become new subjects of research, and to develop with time into independent sciences. As means of education they are used more generally, and prove themselves of higher potency than the pure sciences from which or to which, respectively, the loans are effected. But, in my view, Geography, thus applied, passes, in the process of application, into a foreign province and under another control. It is most proper, as well as most profitable, for a geographer to work in that foreign field; but, while he stays in it, he is, in military parlance, seconded.

Logical as this view may appear, and often as, in fact, it has been stated or implied by others (for example, by one at least of my predecessors in this chair, Sir Charles Close, who delivered his presidential address to the section at the Portsmouth Meeting in 1911), it does not square with some conceptions of Geography put forward by high authorities of recent years. These represent differently the status of some of the studies, into which, as I maintain, Geography enters as a subordinate and secondary element. In particular, there is a school, represented in this country and more strongly in America, which claims for Geography what, in my view, is an historical or ethnological or even psychological study, using geographical data towards the solution of problems in its own field; and some even consider this not merely a function of true Geography, but its principal function now and for the future. Their 'New Geography' is and is to be the study of 'human response to land-forms.' This is an extreme American statement; but the same idea is instinct in such utterances, more sober and guarded, as that of a great geographer, Dr. H. R. Mill, to the effect that the *ultimate* problem of Geography is 'the demonstrative and quantitative proof of the control exercised by the Earth's crust on

the mental processes of its inhabitants.' Dr. Mill is too profound a man of science not to guard himself, by that saving word 'ultimate,' from such retorts as Professor Ellsworth Huntington, of Yale, has offered to the extreme American statement. If, the latter argued, Geography is actually the study of the human response to land-forms, then, as a science it is in its infancy, or, rather, it has returned to a second childhood; for it has hardly begun to collect exact data to this particular end, or to treat them statistically, or to apply to them the methods of isolation that exact science demands. In this country geographers are less inclined to interpret 'New Geography' on such revolutionary lines; but one suspects a tendency towards the American view in both their principles and their practice—in their choice of lines of inquiry or research and their choice of subjects for education. The concentration on Man, which characterises geographical teaching in the University of London, and the almost exclusive attention paid to Economic Geography in the geographical curricula of some other British Universities make in that direction. In educational practice, this bias does good, rather than harm, if the geographer bears in mind that Geography proper has only one function to perform in regard to Man—namely, to investigate, account for, and state his distribution over terrestrial space—and that this function cannot be performed to any good purpose except upon a basis of Physical Geography—that is, on knowledge of the disposition and relation of the Earth's physical features, so far as ascertained to date. To deal with the effect of Man's distribution on his mental processes or political and economic action is to deal with him geographically indeed, but by applications of Geography to Psychology, to History, to Sociology, to Ethnology, to Economics, for the ends of these sciences; though the interests of Geography may be, and often are, well served in the process by reflection of light on its own problems of distribution. If in instruction, as distinct from research, the geographer, realising that, when he introduces these subjects to his pupils, he will be teaching them not Geography, but another science with the help of Geography, insists on their having been grounded previously or elsewhere in what he is to apply—namely, the facts of physical Distribution—all will be well. The application will be a sound step forward in education, more potent perhaps for training general intelligence than the teaching of pure Geography at the earlier stage, because making a wider and more compelling appeal to imaginative interest and pointing the adolescent mind to a more complicated field of thought. But if Geography is applied to instruction in other sciences without the recipients having learned what it is in itself, then all will be wrong. The teacher will talk a language not understood, and the value of what he is applying cannot be appreciated by the pupils.

If I leave this argument there for the moment, it is with the intention of returning to it before I end to-day. It goes to the root, as it seems to me, of the unsatisfactory nature of much geographical instruction given at present in our islands. The actual policy of the English Board of Education seems to contemplate that Geography should be taught to secondary students only in connection with History. If

this policy were realised in instructional practice by encouragement or compulsion of secondary students to undergo courses of Geography proper, with a view to promotion subsequently to classes in Historical Geography (*i.e.*, if History be treated geographically by application of another science previously studied), it would be sound. But I gather from Sir Halford Mackinder's recent report that such is not the practice. Courses in Geography proper are not encouraged during the secondary period of education at all. Encouragement ceases with the primary period, at an age before which only the most elementary instruction in such a science can be assimilated—when, indeed, not much more can be expected of pupils than the memorising of those summary diagrammatic expressions of geographical results, which are maps. How these results have been arrived at, what sort of causes account for physical Distribution, how multifarious are its facts and features which maps cannot express even on the minutest scale—these things must be instilled into minds more robust than those of children under fourteen; and until some adequate idea of them has been imbibed it is little use to teach History geographically. So, at least, this matter seems to me.

It will be patent enough by now that I am maintaining Geography proper to be the study of the spatial Distribution of all physical features on the surface of this Earth. My view is, of course, neither novel nor rare. Almost all who of late years have discussed the scope of Geography have agreed that Distribution is of its essence. Among the most recent exponents of that view have been two Directors of the Oxford School, Sir Halford Mackinder and Professor Herbertson. When, however, I add that the study of Distribution, rightly understood, is the whole essential function of Geography, I part company from the theory of some of my predecessors and contemporaries, and the practice of more. But our divergence will be found to be not serious; for not only do I mean a great deal by the study of Distribution—quite enough for the function of any one science!—but I claim for Geography to the exclusion of any other science all study of spatial Distribution on the Earth's surface. This study has been its well recognised function ever since a science of that name has come to be restricted to the features of the terrestrial surface—that is, ever since 'Geography' in the eighteenth century had to abandon to its child Geology the study of what lies below that surface even as earlier it had abandoned the study of the firmament to an elder child, Astronomy. Though Geography has borne other children since, who have grown to independent scientific life, none of these has robbed her of that one immemorial function. On the contrary, they call upon her to exercise it still on their behalf.

Let no one suppose that I mean by this study and this function merely what Professor Herbertson so indignantly repudiated for an adequate content of his Science—Physiography *plus* descriptive Topography. Geography includes these things, of course, but she embraces also all investigation both of the actual Distribution of the Earth's superficial features and of the causes of the Distribution, the last a profound and intricate subject towards the solution of which she has to summon assistance from many other sciences and studies. She includes, further,

in her field, for the accurate statement of actual Distribution, all the processes of Survey—a highly specialised function to the due performance of which other sciences again lend indispensable aid; and, also, for the diagrammatic presentation of synthetised results for practical use, the equally highly specialised processes of Cartography. That seems to me an ample field, with more than sufficient variety of expert functions, for any one Science. And I have not taken into account either the part Geography has to play in aiding other sciences, as they aid her, by application of her data, or, again, certain investigations of terrestrial phenomena, at present incumbent upon her, because special sciences to deal with them have not yet been developed—or, at least, fully developed—although their ultimate growth to independence can be foreseen or has already gone far. Such, for the moment, are Geodetic investigations, in this country at any rate. In Germany, I understand, Geodesy has attained already the status of a distinct specialism. Here the child has hardly separate existence. But beyond a doubt it will part from its parent, even as Oceanography has parted. Indeed some day, in a future far too distant to be foreseen now, many, or most, of the investigations which now occupy the chief attention of geographical researchers may cease to be necessary. A time must come when the actual distribution of all phenomena on the Earth's surface will have been ascertained, and all the relief upon it and every superficial feature which Cartography can possibly express in its diagrammatic way will have been set out finally on the map. That moment, however, will not be the end of Geography as a science, for there will still remain the investigation of the causes of Distribution, the scientific statement of its facts, and the application of these to other sciences. Let us not, however, worry about any ultimate restriction of the functions of our Science. The discovery and correlation of all the facts of geographical Distribution and their final presentation in diagrammatic form are not much more imminent than the exhaustion of the material of any other science!

In the meantime, for a wholly indeterminate interval, let us see to it that all means of investigating the phenomena of spatial Distribution on the Earth be promoted, without discouragement of this or that tentative means as unscientific. The exploration of the terrestrial surface should be appreciated as a process of many necessary stages graduated from ignorance up to perfect knowledge. It is to the credit of the Royal Geographical Society that it has always encouraged tentative, and, if you like, unscientific first efforts of exploration, especially in parts of the world where, if every prospect pleases, Man is very vile. Unscientific explorations are often the only possible means to the beginning of knowledge. Where an ordinary compass cannot be used except at instant risk of death it is worth while to push in a succession of explorers unequipped with any scientific knowledge or apparatus at all, not merely to gain what few geographical data untrained eyes may see and uneducated memories retain, but to open a road on which ultimately a scientific explorer may hope to pass and work, because the local population has grown, by intercourse with his unscientific precursors, less hostile and more indifferent to his prying activities. There seems

to me now and then to be too much criticism of Columbus. If he thought America was India he had none the less found America.

I have claimed for the geographer's proper field the study of the causation of Distribution. I am aware that this claim has been, and is, denied to Geography by some students of the sciences which he necessarily calls to his help. But if a Science is to be denied access to the fields of other sciences except it take service under them, what science shall be saved? I admit, however, that some disputes can hardly be avoided, where respective boundaries are not yet well delimited. Better delimitation is called for in the interest of Geography, because lack of definition, causing doubts and questions about her scope, confuses the distinction between the Science and its Application. The doubts are not really symptoms of anything wrong with Geography, but, since they may suggest to the popular mind that in fact something is wrong, they can be causes of disease. Their constant genesis is to be found in the history of a Science, whose scope has not always been the same, but has contracted during the course of ages in certain directions while expanding in others. If, in the third century B.C., Eratosthenes had been asked what he meant by Geography, he would have replied, the science of all the physical environment of Man whether above, upon, or below the surface of the Earth, as well as of Man himself as a physical entity. He would have claimed for its field what lies between the farthest star and the heart of our globe, and the nature and relation of everything composing the universe. Geography, in fact, was then not only the whole of Natural Science, as we understand the term, but also everything to which another term, Ethnology, might now be stretched at its very widest.

Look forward now across two thousand years to the end of the eighteenth century A.D. Geography has long become a Mother. She has conceived and borne Astronomy, Chemistry, Botany, Zoology, and many more children, of whom about the youngest is Geology. They have all existences separate from hers and stand on their own feet, but they preserve a filial connection with her and depend still on their Mother Science for a certain common service, while taking off her hands other services she once performed. Restricting the scope of her activities, they have set her free to develop new ones. In doing this she will conceive again and again and bear yet other children during the century to follow—Meteorology, Climatology, Oceanography, Ethnology, Anthropology, and more. Again, and still more narrowly, this new brood will limit the Mother's scope; but ever and ever fecund, she will find fresh activities in the vast field of Earth knowledge, and once and again conceive anew. The latest child that she has borne and seen stand erect is, as I have said, Geodesy; and she has not done with conceiving.

Ever losing sections of her original field and functions, ever adding new sections to them, Geography can hardly help suggesting doubts to others and even to herself. There must always be a certain indefiniteness about a field on whose edges fresh specialisms are for ever developing towards a point at which they will break away to grow alone into new sciences. The Mother holds on awhile to the

child, sharing its activities, loth to let go, perhaps even a little jealous of its growing independence. It has not been easy to say at any given moment where Geography's functions have ended and those of, say, Geology or Ethnology have begun. Moreover, it is inevitably asked about this fissiparous science from which function after function has detached itself to lead life apart—what, if the process continues, as it shows every sign of doing, will be left to Geography later or sooner? Will it not be split up among divers specialisms, and become in time a venerable memory? It is a natural, perhaps a necessary, question. But what is wholly unnecessary is that any answer should be returned which implies a doubt that Geography has a field of research and study essentially hers yesterday, to-day, and to-morrow; still less which implies any suspicion that, because of her constant parturition of specialisms Geography is, or is likely in any future that can be foreseen, to be moribund.

Since Geography, as I understand it, is a necessary factor in the study of all sciences, and must be applied to all if their students are to apprehend rightly the distribution of their own material, it is a necessary element in all education. Unless, on the one hand, its proper study be supported by such means as the State, the Universities, and the great scientific Societies control, and, on the other, its application to the instruction of youth be encouraged by the same bodies, the general scientific standard in these islands will suffer; our system of education will lack an instrument of the highest utility for both the inculcation of indispensable knowledge and the training of adolescent intelligence; and a vicious circle will be set up, trained teachers being lacking in quantity and quality to train pupils to a high enough standard to produce out of their number sufficient trained teachers to carry on the torch.

The present policy of the English Board of Education, as expressed in its practice, encourages a four-years' break in the geographical training of the young, the break occurring between the ages of fourteen and eighteen, the best years of adolescent receptivity. If students are to be strangers to specifically geographical instruction during all that period, any geographical bent given to their minds before the age of fourteen is more than likely to have disappeared by the time they come to eighteen years. The habit of thinking geographically—that is, of considering group Distribution—cannot have been formed; and the students, not having learned the real nature of the science applied, will not possess the groundwork necessary for the apprehension of the higher applications of Geography. Moreover, as Sir Halford Mackinder has rightly argued, an inevitable consequence of this policy is that the chief prizes and awards offered at the end of school-time are not to be gained by proficiency in Geography. Therefore, few students are likely to enter the University with direct encouragement to resume a subject dropped long before, at the end of the primary period of their education.

It is not, of course, the business of schools, primary and secondary, to train specialists. Therefore one does not ask that pure

geographical science should have more than a small share of the compulsory curriculum. Only, that it have some share. If this is assured, then its applications, which on account of their highly educative influence deserve an equally compulsory but larger place in the curriculum, can be used to full advantage. The meaning and value of the geographical ingredient in mixed studies will stand good chance of being understood, and of exciting the lively interest of young students. In any case, only so will the Universities be likely to receive year by year students sufficiently grounded to make good use of higher geographical courses, and well enough disposed to Geography to pursue it as a higher study, and become in their turn competent teachers.

The obligation upon the Universities is the same in kind, but qualitatively greater. They have to provide not only the highest teaching, both in the pure science and its applications, but also such encouragements as will induce students of capacity to devote their period of residence to this subject. The first part of this obligatory provision has been recognised and met in varying degrees by nearly all British Universities during the past quarter of a century. A valuable report compiled recently by that veteran champion, Sir John Keltie, shows that, in regard to Geography, endowment of professorial chairs, allocations of stipends to Readers, Lecturers, and Tutors, supply of apparatus for research and instruction and organisation of 'Honour' examinations, have made remarkable progress in our University world as a whole. But no single British University has yet provided all that is requisite or desired. Oxford and Cambridge, which have well-equipped geographical laboratories, still lack professorial chairs. Liverpool, maintaining a well-staffed Department of Geography, and London, which, between University College and the School of Economics, provides all the staff and apparatus required for teaching, have endowed Chairs; but they direct the attention of the holders to applications of Geography rather than to the pure Science. So do also the University of Manchester and the University College of Wales, both of which maintain geographical Professors.

All the Universities, with but one or two exceptions, examine in the subject to a high standard, that set by Cambridge being perhaps the highest over the whole field of properly geographical study. This latter University, also, alone (if I am not mistaken), has met the second part of her obligation to Geography by the organisation of an Honours course of instruction and classified examination, which, if pursued throughout a student's residence, is sufficient in itself to secure graduation. At Cambridge, therefore, Geography may be said to stand on a par with any other self-contained Final Subject. Neither in London nor in Manchester (I am not quite sure about Liverpool, but believe its case to be the same) is Geography, in and by itself, all sufficient yet to secure graduation, though at each of these Universities it counts strongly in the Baccalaureate Honours course. Oxford offers distinctly less encouragement at present than any of the Universities just mentioned. Her teaching and her examination standard are as advanced as the best of theirs, and the highest award which she

gives for proficiency in Geography, her Diploma 'with distinction' counts towards the B.A. degree in at least as great a proportion as at any other University except Cambridge—it counts, in fact, as two-thirds of the whole qualification; but—and here's the rub!—the balance has to be made up by proficiency in some other subject up to a pass, not an Honours standard. Therefore the resultant degree does not stand before the world as one taken in Honours; and, although some candidates are notified as distinguished and some not in the geographical part of her examinations, the distinction is not advertised in the form to which the public is accustomed—namely, an Honours list divided into classes. The net result is that an Oxford diploma, however brilliantly won, commands less recognition in the labour market than would a class in an Honour School or Tripos. It should, however, be mentioned—though an infrequent occurrence, not advertised by a class list, makes little impression on public opinion—that special geographical research, embodied in a thesis, can qualify at Oxford for higher degrees than the B.A.—viz., for the B.Litt. and B.Sc.—without the support of other subjects.

The reason of this equivocal status of Geography at Oxford is simply that, so far as the actual Faculties which control the courses for ordinary graduation are concerned, Geography is, in fact, an equivocal subject. No one Faculty feels that it can deal with the whole of it. The Arts Faculties will not accept responsibility for the elements of Natural and Mathematical Science which enter into its study and teaching—for example, into the investigation of the causes of Distribution, into the processes of Surveying, into Cartography, and into many other of its functions. Moreover, the traditional Oxford requirement of a literary basis for Arts studies is hard, if not impossible, to satisfy in Geography. The Faculty of Natural Science, on the other hand, is equally loth to be responsible for a subject which admits so much of the Arts element, especially into those applications of its data which enter most often into the instructional curriculum of adolescents—for example, its applications to History and to Ethnology.

At this moment, then, there is an *impasse* at Oxford similar to that (it is caused by the same reason) which prevents the election of a Geographer, as such, either to the Royal Society on the one hand, or to the British Academy on the other. But ways out can be found if there be good will towards Geography, and such general recognition of the necessity of bringing it into closer relation with the established studies, as was implied by the examiners in the Oxford School of Literae Humaniores last year, when, in an official notice, they expressed their sense of a lack of it in the historical work with which they had to deal. Faculties are comparatively modern organisations at Oxford as at Cambridge for the control of teaching and examining. Before them existed Boards of Studies, appropriated to narrower subjects; and, indeed, such Boards have been constituted since Faculties became the rule and side by side with them. The Board, which at the first controlled at Oxford the Final Honour School of English, is an example and a valid precedent.

Cambridge has found it possible to organise a mixed Board of Studies to manage a Final School of Geography, the Board being composed of representatives of both the Arts subjects and the Natural and Mathematical Sciences; and this acts apparently to the general satisfaction even in the absence of a Professor of the special subject, for whose teaching and testing it was formed. Why, then, should Oxford not do likewise? If Cambridge has not waited for the endowment of a Professorial Chair in Geography, need Oxford wait? I am well aware that, when at the latter University the School of English came into existence, there were already two Chairs appropriated to its subject; and I grant that Oxford will not have the very best of all guarantees that a high standard will be maintained in the instructional courses and the examinations in Geography, until there is a Professor *ad hoc*. But guarantees sufficient for all practical purposes she could obtain to-morrow by composing a Board out of her existing teachers of Geography and kindred sciences.

For the last time, then, let me rehearse the too familiar 'vicious circle.' The supply of good students depends on a supply of good teachers; the supply of good teachers depends on a supply of good students. If either supply fails, it is not Geography alone, but all sciences and studies that will be damnified; for all require the best of the help she can give in proportion as her science grows and improves. History will be able to call but indifferent Geography to her assistance, if this science has been understaffed and discouraged by official reluctance to allow it a place of its own in the sun. Is there not still some such reluctance on the part of the Board of Education, of some of our Universities, and of the Civil Service Commissioners?

THE PRINCIPLES BY WHICH WAGES ARE DETERMINED.

ADDRESS TO SECTION F (ECONOMIC SCIENCE AND STATISTICS) BY

W. L. HICHENS,

PRESIDENT OF THE SECTION.

I HAVE chosen as the subject of my address 'The Principles by which Wages are Determined' because I think the most burning question in the industrial world to-day is the proper apportionment of the proceeds of industry between labour and capital. A strong feeling exists in the minds of many that the share of capital is too large and that labour is, in consequence, underpaid. There are those, of course, who hold with Mr. Tawney that capital is functionless and therefore entitled to no reward. It is not my intention to examine the grounds for this statement, for no one who has any experience of business can fail to recognise that under the existing organisation of business capital has a very definite function—that it is indeed essential to any industrial organisation. If there is anyone in this room who has had dealings in the City for the purpose of raising a loan he will feel acutely—not merely the unpleasant consequences that would have awaited him had none of this functionless capital been placed at his disposal—but also the fact that the capitalist has a very definite idea of the importance of his own function. The capitalist would, indeed, automatically cease to exist if he were not needed and fulfilled no function, and the fact that every industrialist is obliged to go to him—often on bended knee—is sufficient answer to the proposition that he performs no useful service. Acquisitive he may be, but he only acquires wealth because he supplies something for which there is a real need. Capital must be paid for just as much as any other commodity, and if any given industry is unable to pay sufficient to attract it, that industry must inevitably languish and ultimately perish.

Many of our industrial troubles to-day arise from the fact that people concentrate on one aspect of the industrial problem only and refuse to consider it as a whole. They are so intent on the rights of labour or of capital that they overlook the fact that each is necessary to the other, and that neither can exist in isolation from the other. It is clearly important, therefore, that both capital and labour should understand, and, what is more, sympathise with, each other's point of view. And if I may venture a criticism it is that the capitalist has usually an intellectual grasp of the point of view of labour, but fails to bring a sympathetic understanding to bear on its aspirations. Labour, on the other hand, apart from some of the leaders whose opinions are in consequence suspect, neither understands nor sympathises with the capitalist standpoint. This is a real misfortune, for the two are indissolubly bound together, and no progress is possible so long as they

quarrel and pull in different directions. Clubs for the discussion of economic questions should be started all over the country, and every section of opinion should be represented in order that problems may be considered from every point of view.

The wage problem is in essentials simple to grasp; it is the problem of the division of the proceeds of industry between labour and capital. How are we to ensure that neither the capitalist nor the worker gets too large a share of the proceeds of industry? How are we to provide that one class of labour does not get too much in relation to another? How are we to secure that the consumer is not robbed by the exaction of too heavy a toll for services rendered? But if the problem is easy to state the solution is by no means simple. Some people would cut the Gordian knot by referring every dispute as it arises to compulsory arbitration. But there are certain difficulties which must be overcome before arbitration can be successfully applied. In the first place, the principle of arbitration must be generally accepted by both sides. You cannot compel large bodies of men to work for a given wage, for there is no penalty that can be enforced against them if they refuse. Nor can you force employers on a large scale to pay a prescribed wage if they prefer to close their factories. The right to strike and the right to lock out must always lurk in the background, and nothing can prevent the exercise of both if enough men feel sufficiently strongly on one side or the other. The time may come when public opinion will recognise so acutely the evils of the strike and the lock-out that both sides will be prepared to accept the principle of arbitration. It is not perhaps too much to hope that one day the principles of reason and justice will triumph over the prevailing theory that might is right and that the only effective criterion of justice is what a man is strong enough to take and to hold. But that day has not yet dawned, and any scheme of compulsory arbitration would, under present conditions, be foredoomed to failure. Meanwhile, an important step in the right direction has already been taken. The Government has been given powers to institute an inquiry into any trade dispute and to summon witnesses. Hence, although the decision of any such court of inquiry will not be binding on the two parties, yet the proceedings can be published and the public will be enabled to pass judgment on the actual facts. The development of these inquiries will be watched with great interest, and there are strong grounds for hoping that they will exercise an effective influence on the side of peace. It is important to notice that these inquiries will only be held after the two parties have met and failed to reach agreement. For by far the best method of settling a dispute is that the representatives of both sides should meet and negotiate with the object of finding some solution that is mutually satisfactory. It is only in the last resort, when all other means have failed, that recourse should be had to a higher authority. I must add that it is, in my opinion, regrettable that arbitration is not voluntarily accepted by both sides more often. There is a tendency at present for certain groups of employers to refuse arbitration, although the representatives of the Trade Unions concerned are prepared to do so, and I feel that this is a short-sighted policy. It must be confessed also that the feeling amongst employers

that the rank and file of labour would refuse to be bound by an unfavourable arbitration award, even if their leaders had agreed to accept it, is not without foundation.

A second objection to arbitration that has to be met is that the arbitrators must command the confidence of both sides. For arbitration, in the sense of an award made by Government nominees, has long ago been tried and found wanting. Attempts have been made in times past to regulate wages and conditions of work either by Acts of Parliament or by particular orders of the justices of the peace, but, on the whole, the results have been bad, and the well-known criticisms of Adam Smith appear to have been justified. For the laws were made and administered by the employing classes and took little account of the aims and aspirations of the workers. They were essentially conservative in character and aimed rather at preserving the ancient privileges of the ruling classes than at developing the liberties of the ruled. Fortunately the growth of the labour movement has now made it possible to secure both a fair hearing and adequate representation for working-class interests. One result of the development of Trade Unionism has been to create a body of highly trained experts who can be relied on to do full justice to the cause of those whom they have been chosen to represent. The difficulty to-day is that the Trade Union leader, whose education and training have given him a wider grasp of economic problems than is possessed by his constituents, is often not treated with the confidence that he deserves and is not allowed that freedom and power to settle which is essential to the success of all negotiations.

A third objection to arbitration, which leads up to the subject with which I am to deal more particularly to-day, is that there are at present no generally accepted principles governing industrial problems which the arbitrator has to interpret, and yet the success or failure of arbitration as a method of settling industrial disputes depends ultimately on whether there are certain clear principles which the great majority are prepared to accept as just and reasonable. The function of an arbitrator is to interpret and apply accepted principles just as that of a judge is to interpret the principles embodied in the laws. It is not his business to lay down principles, and if he attempts to do so he will probably fail. That is why arbitration has so often miscarried and why the Hague tribunal was foredoomed to failure. For the disputes between nations are not as a rule differences as to the interpretation of a principle; they arise from a conflict of principles. Hence the danger that arbitrators will base their verdict on the wording of treaties and agreements, on precedent and tradition, and serve merely to protect the *status quo*. This is a very real danger in industrial questions, for the industrial machine is extremely sensitive and complex, and needs continually to be adjusted to an ever-changing environment. What is good for to-day will perhaps be wholly unsuited for to-morrow, and no worse fate can befall industry than that it should be fast bound in the tyranny of precedent. Another danger of special application to wages disputes is that, in the absence of real principles, an arbitrator may simply split the difference between the contentions of the disputants,

and there is a widespread feeling that, in the past, wages awards have largely been made by this rule-of-thumb method. It is of the first importance, therefore, that clear and well-recognised principles should be established, and in considering the question one would naturally expect to find guidance in the laws of those States which have adopted compulsory arbitration for wages disputes. Unfortunately they have shirked the difficulty, and left it to the arbitrator to make and interpret his own code of rules. According to some Australian Acts reasonable wages are defined as 'the average prices of payment paid by reputable employers to employees of average capacity.' But the 'reputable employers' clause has proved a broken reed, and embodies no principle of practical value. For, in the first place, there are industries in which a standard wage is paid by *all* employers so that in the event of a dispute there are either no reputable or else no disreputable employers—whichever you please. In the second place, even if there are certain employers in an industry who pay higher wages than others, it does not follow either that the employers who pay less are not reputable or that the higher-paid employees are of average capacity. The probability is that they are not—that they are above the average. The justification for paying higher wages is that you get the pick of the basket by so doing. And efficiency is so important that it is worth the while of any given firm to adopt this course if it can be sure that others will not follow suit. If, unfortunately for it, they do, it no longer gets the pick, and the game is spoiled. Henry Ford is only able to pay higher wages than his rivals because this policy enables him to adopt the strictest tests of efficiency.

The weakness of this clause has led the Australian Commonwealth to adopt another principle for the guidance of arbitrators, namely, that the conditions as to the remuneration of labour are to be such as the President of the Commonwealth Court of Conciliation and Arbitration shall declare to be fair and reasonable. That is not a very illuminating or helpful principle, and Mr. Higgins, the President of the Court, has complained very bitterly that the Legislature has left to him what it ought to have done itself by defining what is meant by 'fair and reasonable.' Everyone, even the disreputable employer, will agree that wages must be fair and reasonable, but with this meaningless proposition our unanimity comes to an abrupt end, for we find the most divergent views as to what constitutes fairness or reasonableness. One school—and a powerful one, too—holds that a fair wage is one that is determined by the higgling of the market, that it is established by the law of supply and demand. 'The money rate of wages,' says Walter Bagehot, 'is a case of supply and demand—that is, it is determined by the amount of money which the owners of it wish to expend in labour, by the eagerness with which they want that labour, by the amount of labour in the market which wishes to sell itself for money, and by the eagerness with which the labourers desire that money.' No doubt in a perfect world, and if everyone were a perfectly free agent, the law of supply and demand might safely be left to take its own course. And even in the imperfect world in which we live it has its value as a criterion in the determination of wages, and must always be regarded

as one of the decisive factors, though not by any means the only one. It is true that the law may be harsh and inhuman in its operation when applied by harsh and inhuman men, but it has often proved of more advantage to the workers themselves than the solicitude of Parliament or its agents. There is a familiar ring about the history of a wages award in the London tailoring trade made by the justices of the peace in 1771. It can be paralleled in any country in any age. The wages of London tailors were settled at 2*s*. 6*d*. a day, 'but many master tailors gave some of their men 3*s*. a day; they paid the 15*s*. at the end of the week openly, and then put 3*s*. more for a man in some place where he knew where to find it. And if this money was not laid up for him on the Saturday night the master might be certain not to see his face on the Monday morning.'

But it must be remembered that in very many cases we are not free economic agents, and the open competition which is essential to the successful functioning of the law of supply and demand is conspicuously absent. It is absent, for example, in the case of a general coal strike or a railway strike, where the whole community is threatened with disaster. It is absent, too, if the employers in any big industry threaten a lock-out as the alternative to a reduction in wages, because there is no reasonable chance for the men to find other employment, and starvation may stare them in the face. In such cases the law of supply and demand should not be allowed to decide the issue, for the economic wage would be either too high or too low from the standpoint of what is fair and reasonable. State intervention therefore becomes necessary. But the law of supply and demand always has been one of the chief factors in determining the price of labour, and will continue to be so as long as the existing industrial system lasts. For it is merely another way of stating that a free exchange of services, as of commodities, is the foundation of all trade. Indeed, no other practicable method has ever been devised for determining the relative value of certain services. When a new industry is started, for example, it is necessary to attract workers from those already in existence, and this can only be done by offering higher wages or better conditions. Similarly, if there is a shortage of workers in any established industry owing to increased prosperity, the *personnel* must be drawn from outside by the offer of greater inducements unless the shortage can be made good from the ranks of the unemployed. Again, if there is a general expansion of industry throughout the country, accompanied as it must be by a general increase of wealth, the greater demand for workers will cause wages to rise. Indeed, as Adam Smith has pointed out, it is in a progressive society, in which the demand for labour continually rises, that wages are highest. 'It is not the actual greatness of national wealth,' he says truly—and we shall do well to take his words to heart in these days—'but its continued increase which occasions a rise in the wages of labour.' In a stationary or declining society wages are bound to fall.

But, important though the part played by the law of supply and demand is in determining wages, there is another equally important

principle which governs them—namely, that all men must be paid a living wage. The former is easy to understand and works automatically, though not always satisfactorily. It is important to remember, however, that if the law of supply and demand works badly the fault lies not with political economy but with ourselves. The fact that wages postulate a willing buyer and a willing seller of labour does not justify the employer in driving the hardest bargain he can. The interpretation of this law must be consistent with the higher moral law of our duty towards our neighbour, and the many shortcomings in our industrial life may, in my opinion, be attributed entirely to the fact that we have failed to apply the moral law. It is not the system which is wrong, but those who work it—employers, employed, and consumers alike; it is the hearts of men that must be changed, not the forms of industrial organisation, if we are to cure industrial unrest.

But, if the law of supply and demand is easily intelligible, the principle of the living wage has given rise to many controversies. It is obvious that a man must be paid at least enough to keep body and soul together, otherwise the human race would cease to exist. But we mean by a living wage something more than a bare pittance sufficient to maintain a man's physical health at a proper standard. This is the criterion for an ox or an ass, not a human being. We mean a wage suitable to the development of the physical, moral, and intellectual attributes of mankind. This is what Mr. Clynes, one of the clearest thinkers in the labour world to-day, means by a living wage. He defines it as one which should ensure to the human being a condition of life 'equal to the expectations and tastes of a civilised population of this age.' This is an ideal which we should all, I think, readily accept. But I must emphasise the point that it is an *ideal*, and that therefore it may not be capable of realisation in all times and in all places. Wages, as I have pointed out above, depend on the accumulated wealth of a community, which is obviously greater in times of progress and development than during a period of stagnation or retrogression. Clearly, therefore, wages must vary from time to time, and it is idle to pretend that any country can guarantee permanently a wage equal to the expectations and tastes of a civilised population. We are now living in a period of industrial stagnation, following upon one of intense activity. It is inevitable, therefore, that wages should fall. It is inevitable, too, that the wages in one industrial country should approximate to those of others where competition for foreign markets is concerned. Unless we have greater natural advantages than our foreign rivals, or are more industrious, or have superior mechanical contrivances, we cannot pay higher wages here than there, for if we do they will underquote us and take away our foreign trade, which is essential to our existence. And this is just what is happening to-day. It is clear, therefore, that a lowered standard of civilisation in one country will react disastrously on others, and that if the more fortunate are not willing to lend a helping hand to their poorer neighbours they will themselves be dragged down to the same level. Instead of trying to keep Germany under, we ought,

therefore, to try to put her on her feet, not merely as a moral duty, but on the lowest grounds of self-interest.

I have dwelt at some length on the point that a civilised wage such as we all desire may be unattainable, because it is of critical importance to-day, and because, obvious though it may appear, it is widely ignored. We are continually being told that the standard wage should be the 1914 wage, plus a percentage equivalent to the increase in the cost of living since that date. And yet we are obviously poorer than we were in 1914, and it is equally obvious that our foreign trade is slipping from our grasp, owing to the competition of Germany and America. From a practical point of view what is necessary is not to work out a standard wage which we should like to pay if we could, but to determine what wages we can afford to pay in each industry without losing our foreign markets. This can only be settled by a frank discussion between employers and employed, and it is essential that employers should disclose all the facts. This would reveal that in many industries prices have fallen faster than costs, and that work is being taken at a loss. This is, I believe, right as a temporary measure, because it is not reasonable that all the sacrifices should be borne by the workers. But it can be only temporary, otherwise fresh capital will not be forthcoming, and our industries will perish for the want of it.

It is clear, therefore, that in accepting the principle of a civilised wage we must have due regard to the progress, maintenance, and well-being of the industry under consideration.

But it may be that, whilst the great majority of trades and industries in the country can afford to pay what may fairly be regarded as a civilised wage, some few industries may be unable to do so. One way of meeting the difficulty is by the imposition of a special tariff on imported goods, on the lines of the Safeguarding of British Industries Bill. I must not be led too far astray into the byways of controversy, but I confess that I think this is a thoroughly bad solution. I do not object to the protection of infant industries, but if a full-grown industry cannot walk without crutches we are better without it, even if its absence may embarrass us in a world war once every hundred years. As a matter of fact, however, sweated wages are usually the result of inefficiency—absence of labour-saving devices and bad organisation. So that often the real remedy for a trade in which wages are depressed is an expert inquiry into its methods of working, and State-aided scientific research, which has an important field ahead of it.

If it is accepted that the basic wage of a worker must be a living wage and that this term should be interpreted as liberally as possible consistently with the progress, maintenance, and well-being of the industries of the country, a further question arises. What do we mean by a worker? Do we mean a single man, a childless married man, or a man with a family? Obviously, I think, the cost of living for a married man with a family is greater than for a single man, although I have heard the opposite argued, very unconvincingly. Is a living wage to cover the expenses of a married man with an average family of, say, three children? Or should it merely cover the man, some other means being found to provide for the wife and family? The case for a

single-man wage plus family allowances has recently been put forward with great ability by Miss E. F. Rathbone. She points out that 27 per cent. of the men workers over twenty in England are bachelors or widowers without dependent children; 24.7 per cent. are married couples without children or with no dependent child below fourteen; 16.6 per cent. have one dependent child; 13 per cent. have two dependent children; 8.8 per cent. have three dependent children; and 9.9 per cent. have more than three dependent children. Hence a living wage based on the five-member family is adapted to the needs only of one of the smallest actual groupings. She argues, therefore, that the childless man is getting too high a wage in relation to the man with a family, and that the distribution of the wage fund is uneconomical. Her suggestion is that the wives and children should be provided for out of a separate fund maintained by contributions from employers calculated according to the number of their male employees, whether married or single. Thus the employer will have no inducement to prefer single to married men, whilst every wage-earner and his family will be assured of an income at least adequate to the needs of healthy physical subsistence, and at the same time the wages bill of the country will be substantially reduced, thus relieving industry of a burden that is threatening to strangle it. In support of her proposals, she points out that, so far as the children are concerned, this plan has already been embodied in the New South Wales 'Maintenance of Children Bill,' and that Mr. Hughes has foreshadowed the intention of the Federal Government of Australia to introduce a similar Bill into the Federal Parliament. It may be added that a scheme similar to that outlined by Miss Rathbone has actually been adopted by the textile industries of the Roubaix-Tourcoing district of France.

Even if it be assumed that Miss Rathbone's scheme would have the results that are claimed for it, I am strongly of opinion that the remedy would prove far worse than the disease. In the first place, it will, I am convinced, prove impossible to confine this scheme to the wage-earners; it must be extended to the salaried classes, and, in fact, to the whole community. It will thus inevitably fall to be administered by the State, and I confess that I can imagine no more detestable form of State Socialism. For it will involve a State interference in the home life which will make the war-time activities of the Government fade into insignificance. In the second place, however much we may attempt to disguise the fact, the effect of a family fund must be to subsidise families at the expense of the childless. I can see no justification for the argument that a wife and family are a burden which no man can reasonably be expected to bear, and from which, therefore, he must be relieved by the State or his more fortunate celibate fellow-citizens. Nor is his marriage necessarily a benefit to the community to be gratefully acknowledged by a dole. In fact, I can imagine the Dean of St. Paul's, for example, arguing that a premium should be paid to those who do not increase the population of the country. All virile and healthy nations have recognised that the husband is responsible for the maintenance of his family, that he must be regarded as the bread-winner, and that only thus can the family be so closely knit together that it is

in the true sense of the word a unit. In my opinion the State has already gone too far in the direction of taking over the duties of parents, and I regard as deplorable the present tendency to throw more and more of the burden in respect of housing, medical attendance, dentistry, food, clothing, and education on the State, thus relieving parents of what should in large measure be their own responsibility. This policy breeds up a race of slaves—not free men.

I have dealt so far with two principles by which wages are determined—the law of supply and demand, and the principle of a living wage. I will now pass on to a third—the principle that wages should be proportioned to the service rendered. In some respects this result is achieved through the operation of the law of supply and demand, and in the last resort the price that one man is prepared to take and another to give for labour is the only practical criterion of its value. But the value of the service rendered is not merely the result of a haggling match. It is clearly just, for example, that a good worker should receive higher wages than a bad one, that the man who produces much should be paid more than the man who produces little. By far the best way of securing this end is through the establishment wherever possible of a piece-work or premium bonus system. Under such a system not only does the payment received bear a direct relation to the results attained, it also acts as an incentive to greater effort. It might naturally be expected therefore that the Trade Union Movement would encourage a system that has such obvious advantages. Unfortunately, however, some of the leading Trade Unions are opposed to payment by results, and, if they cannot suppress it altogether, are successful in preventing its extension. The explanation of their attitude, of course, is a fear, often well justified by past experience, that payment by results will lead to speeding-up followed by an arbitrary reduction in the rates. It will lead me too far afield if I attempt a detailed discussion of this question, and I will merely say the objections, though serious, are by no means insuperable. Some firms, for example, have agreed that if an alteration is made in their piece-work list the saving shall always be used to increase the wages of some other section of their workers. In other cases a piece-work list is mutually agreed by representatives of employers and workers, and can only be altered after negotiation. I should like to emphasise the importance to our national industries of encouraging payment by results, because it is one of the surest ways of increasing efficiency. If the principle were accepted by both labour and capital, as it should be, a frank discussion would disclose the means of overcoming the abuses that experience has proved to exist.

But there will always be many classes of work where payment by results is not practicable, and where a time rate must be adhered to. Should some differentiation be attempted in respect of time rates? The period of maximum efficiency lies between the ages of thirty and fifty, and it has been suggested that wages should be based on a sliding scale according to age, reaching a maximum at the age of thirty and tapering off after the age of fifty. Any cut-and-dried rule of this description would be most objectionable and work with great harshness. There are many men over fifty who are far more efficient than younger men, and

it would be unjust to place them on a lower scale of wages. Moreover, the result of such a sliding scale would be to give an undue preference to older men, when employment is scarce, at the expense of younger men with growing families. At the same time, something should be done to meet the case of the old, the infirm, and the maimed, although the matter cannot be left to the sole discretion of employers without serious risks. In Australia the arbitration laws empower arbitrators to license old, slow or infirm workers at lower rates, and I think the same principles should be adopted here. In particular, wounded soldiers in receipt of a pension should be licensed to accept lower rates where their working efficiency has been impaired, since it is an injustice that men who have been wounded in the defence of their country should be handicapped in getting work.

The principle of equal pay for the same work leads on to the consideration of women's wages. We are all familiar with the battle-cry of 'equal pay for equal work' which has a convincing ring about it. But when one considers it more closely one realises that it is extremely difficult to define what is meant by equal work. How are you to compare the work of a hospital nurse and a stockbroker, or the services of a charwoman and a sailor? A comparison between the work of men and women is possible where both are doing identically similar jobs, though even here there are many different factors which make it difficult. But in practice the tendency is for men and women to do different types of work. During the war women to a large extent replaced men in the factories, but their introduction nearly always led to a reclassification of the work. Except in the earliest days of the war, men and women did not work indiscriminately at the same jobs; certain classes of work were allocated to them, and, as time went on, they were usually collected into separate shops. Since the war men have largely reverted to their old jobs, replacing women, and the tendency to differentiate between the spheres of men and women grows more and more marked. It is the exception that they do the same work as men, and a comparison between the value of their work and man's in his different sphere serves no useful purpose. Their wages must largely be governed by the law of supply and demand, and since the openings for women are relatively fewer than for men; since for a variety of reasons their cost of living is lower; and since their average term of service is shorter and therefore their experience is less than that of men, because they usually cease work on marriage, it is inevitable that, on the whole, their wages should be lower than those paid to men.

The three main principles governing wages, then, are the law of supply and demand implying freedom of contract or a willing buyer and a willing seller, the cost of living, and the principle that wages shall be proportioned to the service rendered. There are certain other considerations referred to by Adam Smith as leading to inequality of wages which may be briefly mentioned. They are: (1) The agreeableness or disagreeableness of the work to be done; (2) The expense of learning a trade or profession; (3) Constancy of employment; (4) Responsibility; (5) The risk of failure.

It has been suggested that another factor in determining wages in

any given industry should be the financial prosperity of that industry, that wages should bear some definite relation to profits and presumably to losses, although this fact is seldom emphasised. Profit-sharing may, or may not, form a valuable adjunct to the wages system, but no form of co-partnership or of the co-operative movement can ever replace the wage system, for the simple reason that you cannot keep body and soul together on a minus quantity of food; there must always be some guaranteed minimum. The essence of the wage system is that the employee is assured of his wage whether the business makes a profit or a loss, and the fundamental wage on which those of all higher grades are based—namely, the wage of the unskilled worker—must be determined by the cost of living, not solely by considerations of profit and loss. I can see no other practicable basis for a wage system, and even under Guild Socialism or State Socialism this principle must be operative—however much the pill may be gilded by high-sounding phrases.

Wages, of course, do tend to rise in any industry when trade conditions improve, and in that sense profits are shared, but the exclusive enjoyment of the improvement does not remain for long. If, for example, the tinplate industry is prosperous, the workers in that trade are the first to feel the benefits of improved conditions. But soon the miner who supplies the coal on which the industry depends, the railway worker who transports it, the butcher and the baker who feed the tinplate worker, and so on, will also require a share. Moreover, the coal-owner and the railway company will expect consideration, so that in the end the prosperity of one industry tends to become generally diffused. And this tendency is natural, partly because of the dependence of one trade on another, and partly because the wages of one trade or employment tend to bear a definite relation to those of other trades. Hence there are strong forces always at work in the direction of equalisation, both as regards wages and profits. Again, labour, in this country, is organised on a craft, not an industrial, basis. There are fitters, turners, carpenters, joiners, boilermakers, employed in a number of different industries, and their wages are those of their craft; they are not fixed in relation to the industry for which they happen to be working. It is one of the cardinal principles of craft unionism that there should be a uniform wage for all able-bodied members of the same craft. A railway porter on the London and North Western Railway, for example, claims the same wage as his brother on the Great Central, and the latter would be outraged at the suggestion that he should accept a lower wage than the former merely because the London and North Western Railway happens to be more prosperous than the Great Central.

I think that the importance which the workers themselves attach to the maintenance of a definite relation between the wages of different classes of employment has been underrated by those who advocate profit-sharing as a panacea for all our industrial troubles. Mr. Cramp put the case very well last year when presenting the demand of the railway men for increased wages before the National Wages Board. 'So far as the workers are concerned,' he said, 'their status is

determined to a greater degree than that of any other section of society by the amount of wages they receive. In other walks of life a man's titles or his learning or his particular standing frequently are not related to the amount of income he receives, but with the workers a man's standing is almost entirely related to his income. Men, women, and their children are judged, and their social conditions are determined, by the amount of wages or salary which they are able to earn and the consequent standard of life that they are able to maintain.' He proceeded to make a comparison between the wages of railway men and those of other callings—in particular dockers, miners, policemen, and municipal workers—with the object of showing that the pay of railway men was low in comparison with that of other walks of life.

The fact that any particular industry is making large profits is not—*per se*—a ground for increasing the remuneration of the workers any more than the fact of a Budget surplus is a justification for increasing the salaries of Civil Servants all round. We feel that the general taxpayer is entitled to the saving, and it may be that the general public is entitled to participate in the prosperity of an industry either through a reduction in prices or through the taxation of profits.

Another objection to profit-sharing deserves a brief mention. It is that if it is to succeed the capital employed must be high in relation to the wages paid, otherwise the profits to be shared will be insignificant. Suppose, for example, that the capital employed in a business is 1,000,000*l.*, and the annual wages are 3,000,000*l.*, as might well happen in a shipyard; suppose, again, we assume the profit earned to be 10 per cent., which would be a very high average in the shipbuilding trade—before the War it did not, I believe, exceed 3 per cent. for the industry. If half the profits went to capital and the other half were shared between labour and capital—a very common form of profit-sharing—labour would receive 25,000*l.*, or an addition of only twopence in the £ on its wages. Bearing in mind the increases that have actually taken place in recent years, it will be recognised that such an addition would be regarded as insignificant. The fact is that in any country where labour is well organised wages absorb as much as can be allotted to labour consistently with a reasonable return to capital. And if a reasonable return is not offered to capital no capital will be forthcoming.

It is extremely doubtful if labour would tolerate a different remuneration between the various firms within an industry owing to the importance attached to the maintenance of a definite relation between the wages of different groups of men. But if they were prepared to accept a differentiation other forces would counteract it. A successful firm making large profits would be able to offer higher remuneration than an unsuccessful one, and thus attract the best men. Theoretically this may seem right and proper, but in practice the unsuccessful firm would find itself obliged to guarantee a bonus to its workers equivalent to the share of the profits accruing to the workers in the more prosperous ventures. Otherwise it would find that it could only attract the least efficient workers at a time when efficiency was most needed to save it.

The objection that under a profit-sharing system there might be glaring inequalities as between one business and another has been anticipated under the so-called profit-sharing scheme recently adopted in the coal-mining industry. It was realised that any scheme would fail if a collier working on a rich mine were to receive far more than his fellow-worker on a poor mine immediately adjacent, although the type of work done by each and the hours of labour might be exactly the same. Hence the country has been divided into districts, and within each district there is no variation in the scale of pay. A standard wage is fixed in each district, being in effect the July 1914 rates plus, in the case of piece-workers, the percentage additions which were made consequent upon the reduction of hours from eight to seven; and there is a guaranteed minimum for a certain period of the standard wages plus 20 per cent. A standard profit is fixed equivalent to 17 per cent. of the cost of the standard wages, and any surplus after paying standard wages, costs of production, and standard profits is to be shared between labour and capital, 83 per cent. being applied in each district to the payment of wages above the standard wages, and 17 per cent. being allocated to the owners as profit. This is not a profit-sharing scheme in the proper sense of the term because, while the district as a whole may make a profit, and therefore wages may increase in the proportions specified, certain individual firms may make a loss and yet be obliged to pay the increased wages. It is possible that if such a scheme is to prove workable, the mines in each district will be obliged to amalgamate, for under present arrangements the poor mines will in effect be penalised by the profits of the rich ones, whilst the latter will benefit owing to the reduction in average profits due to the losses on the former. Hence the distinction between the poor and rich mines will merely be accentuated. It is more probable, however, that there will be no average profits during the next few years, since substantial reductions in the price of coal are essential and inevitable, and that the scheme will prove to be still-born. Should it, however, turn out to be a vigorous infant and lead on, as I have suggested, to district amalgamations, a further consequence will be that the miners will make a strong demand for a voice in the control of the industry. This is a natural consequence of effective profit-sharing, and one which I believe to be unworkable. It would, however, carry me beyond the scope of this paper to discuss the question, and I will merely say that I believe the ultimate control in any business must always rest with those who bear the financial responsibility.

Whilst I do not believe that profit-sharing will ever solve the problem of the fair distribution of the proceeds of industry between labour and capital, it may prove of advantage in particular cases, and it is to be hoped that experiments will continue to be made in this direction. In fact, there is much to be gained by experiments in as many directions as possible. Co-partnership, the co-operative movement, which is a form of profit-sharing among consumers, building and other guilds, State and municipal management, individual enterprise—all have a part to play in satisfying the various demands of human nature, and there is room for all. For elasticity is an essential

feature of successful industrial development, and the individual liberty which this implies can only be found in countries where the law is respected and there is a strong Government. For, as has been truly said, ' Where order reigns her sister liberty cannot be far.' The outstanding feature in the history of our own Empire, as of every successful commercial community, and the lesson of Europe to-day, is that industrial prosperity depends upon stable political conditions combined with individual liberty. The absence of either always has resulted and always will result in industrial stagnation and disaster.

The conclusions, then, that I would put before you are these: There is no simple and straightforward system applicable to the division of the proceeds of industry between labour and capital. Both are essential to industry, and therefore to each other; hence the deeper interests of both lie in co-operation, and the task before the leaders of labour and of capital consists in promoting the interests of both, not in selfishly pursuing the advantage of the one at the expense of the other. Both must recognise the need of contenting the other, for if capital is not satisfied its springs will dry up and the industrial body will wither away, whilst if labour is discontented and the members of the industrial body war against each other the end is death. The real solution of the problem is a moral one, and can be achieved only if justice and virtue govern the lives of the members of the community, for all human organisations must reflect the character of those who work them. Arbitration offers no immediate solution of the difficulty, for to be effective it must be voluntarily accepted by the majority on both sides, and the principles by which arbitrators are to be guided must first be clearly expressed and accepted. But it is the goal at which civilisation must aim, and as a step in this direction public inquiries into all disputes between labour and capital should be encouraged after all attempts at mutual agreement have failed.

A clearer understanding of economic truths in the industrial world is essential if disputes are to be avoided. It must be recognised that the wealth available for wages depends on the total production of the country, and that whilst, if production increases, wages will go up, if it falls wages must come down. It must be recognised, too, that where foreign competition is concerned the wages paid in one industrial country must have an important bearing on those paid in others.

So long as the present industrial system continues—and no alternative system that is practicable at any rate within a measurable distance of time has ever been suggested—the wages system must prevail. Profit-sharing is no substitute for it, since, amongst other reasons which I have referred to, the amount necessary to provide a living wage will and should absorb practically the whole of the share available for labour, leaving only a reasonable return to capital sufficient to encourage its production.

The fundamental wage, or the wage of unskilled labour, should be a living wage—that is, a wage suitable to the development of the physical, moral, and intellectual attributes of the citizens of a free country. But it must be recognised that the degree to which this ideal can be attained must depend on the skill and endeavour of the people,

and due regard must be had to the progress, maintenance, and well-being of the industries of the country. It is idle to hope that the living wage can be based permanently on any given standard of civilisation; it is bound to fluctuate at different periods, and will depend largely on whether the industries of a country are progressive, stagnant, or retrogressive. Wages above the minimum or living wage are determined mainly by the law of supply and demand, but certain other factors enter into their determination, notably the principle that wages should be proportioned to the value of the service rendered, implying payment by results. There never was a time when it was more important that all should grasp, not merely what is possible, but what is reasonable as regards wages. For the artificial prosperity and trade activity that followed upon the War are at an end, and the reaction has begun. How long it will last no one can tell, but it is reasonably certain that we must expect a period of depression and falling wages and profits. It is essential that the wage-earners should recognise that reductions are inevitable, and not the fault of the capitalists; they should be satisfied that all reductions proposed are reasonable. The capitalist, on his side, must be prepared to accept his full share of sacrifice, and be ready, if need be, as a temporary measure, not merely to receive no profits, but to face a loss, in order that our difficulties may be tided over until our trade recovers and prosperity returns.

NOTES ON WATER-POWER DEVELOPMENT.

ADDRESS TO SECTION G (ENGINEERING) BY
PROFESSOR A. H. GIBSON, D.Sc.,
PRESIDENT OF THE SECTION.

The extent to which the water powers of the world have been investigated and developed during the past decade forms one of the striking engineering features of the period. Although falling or flowing water formed the earliest of the natural sources of energy to be utilised for industrial purposes, it is of interest to note that two-thirds of the water power at present in use has been developed within the last ten years.

The reasons for the revival of interest in this question are partly technical and partly economic.

The technical development of electric generation and transmission has made it economically possible to utilise powers remote from any industrial centre, while a rapid increase in the demand for energy for general industrial purposes and for the many electro-chemical, electro-physical, and electro-metallurgical processes which are now in general use, and whose field is rapidly growing, has provided a ready outlet for all such energy as could be cheaply developed.

The urgent demand for energy to supply the abnormal requirements of the war period, combined with the world shortage of fuel, was responsible for an unprecedented rate of development in most countries with available water-power resources, and especially in those countries normally dependent on imported fuel.

Thus in France some 850,000 water horse-power has been put into commission since 1915, and the country now has 1,600,000 horse-power under control as compared with 750,000 before the war. In Switzerland some 600,000 horse-power has been developed since 1914, or is in course of construction, as compared with 880,000 horse-power before the war. In Spain, where the pre-war output was 150,000 horse-power, the present output is 620,000 horse-power, and about 260,000 horse-power is now in course of development, while the Spanish Ministerio de Fomento is considering the development of some 2,000,000 horse-power to be delivered into a network of transmission lines covering the industrial parts of the country.

In Italy, schemes totalling about 300,000 horse-power are under way, and it is estimated that the total output will shortly amount to 2,000,000 horse-power. The Government Hydrographical Department is now engaged in gauging and surveying the profiles of the principal rivers, and statistics of available reservoir sites, of lakes suitable for storage, and of available horse-power are being compiled.

Japan, which only very recently began to investigate her water powers, has, since 1916, developed over 1,000,000 horse-power, or almost twenty per cent. of her available resources.

In Canada and the United States many large schemes have recently been brought into service, and some extremely large installations are now in course of construction or are projected. Thus the Queenston-Chippewa project on the Canadian side of the Niagara River is intended to develop some 500,000 horse-power, while a projected development of the St. Lawrence River will be capable of yielding 1,700,000 horse-power. In Canada the total development (2.3 million horse-power) in 1918 was almost three times as great as in 1910. In the United States of America the development has increased from something under two million horse-power in 1901, to 5.3 millions in 1908, and to nearly 10.0 millions in 1920.

Rapid as has been the development of water power in the United States in the past, it has been retarded by the fact that the privilege of using the national forests or other public lands for water-power development has only been granted by the issuing of permits which were not available for any definite period and which were revocable at the will of the Granting Authority. In the case of development on navigable streams, whether on public or private land, each scheme has required a special Act of Congress, and these Acts could be revoked by Congress at any time. Owing to the uncertainty of tenure there has naturally been some reluctance to invest capital in such undertakings.

By the recent Federal Water Power Act, signed in June 1920, licences for such developments may now be issued under the jurisdiction of a new body, known as the Federal Power Commission, for a period not exceeding fifty years, at the end of which the licence may be renewed, or the Government may take over the enterprise upon compensation of the licensee. In the issuing of licences, preference is to be given to State and municipal applications. The effect of this Act may be inferred from the fact that, within a month of its being signed, applications for licences to develop over 500,000 horse-power had been filed. The duty of collecting, recording, and publishing data regarding the utilisation of water resources, the water-power industry and its relation to other industries, and regarding the capacity, development costs, and relationship to possible markets, of power sites, has also been assigned to this Federal Power Commission.

World's Available Water Power.—During the past few years much attention has been paid to statistics of available and developed water powers. In the case of developed powers, these are usually stated in terms of the capacity of the installed machinery. This machinery is in general only used to its full capacity over a portion of each day, although in many such cases water is available for providing continuous power if desired.

Estimates of potential power are always to be accepted with considerable reserve. In order to make a reasonably accurate estimate, the run off from the catchment area, and the variation in this run off from month to month and from year to year, must be known, and it is only in comparatively rare cases that this information is as yet

available. Moreover, there is as yet no standard basis on which potential power is computed.

The power available from a given stream during the wet season is many times as great as during the dry season unless sufficient storage is available to equalise the flow throughout the year, and the cost of such storage would in general be prohibitive, even if it were physically possible to provide it.

The United States Geological Survey takes the maximum useful flow of a stream as being that which may be guaranteed during six months in each year. The minimum flow is taken as the average which can be guaranteed over the two driest consecutive seven-day periods in each year, along with the additional flow which may be obtained during this period by developing any available storage capacity in the upper waters of the stream. Estimates of potential power based on storage capacity are, however, subject to a wide margin of error owing to the limited data available, and in the following table the potential water power is estimated on the basis of the maximum flow as just defined, and in terms of continuous 24-hour power.

(Millions of Horse-Power.)

—	Available	Developed
Great Britain	0·9	0·2
Canada	23·0	3·28[1]
Remainder of British Empire including: Australia, Africa (East), „ (South), „ (West), British Guiana, India and Ceylon, New Zealand, Papua	30·0 to 50·0	0·7
Austria	6·5	0·57
Brazil	26·0	0·32
Dutch East Indies	5·5	—
France	5·6	1·6
Germany	1·5	0·75
Iceland	4·0	1·0[2]
Italy	4·0	1·25
Japan	8·0	1·5
Norway	7·5	1·25
Russia	20·0	1·0
Spain	5·0	0·88
Sweden	6·2	1·2
Switzerland	4·0	1·4
United States of America	28·0	9·8

Adopting these figures it appears that the available horse-power of the world is of the order of two hundred millions, of which approximately twenty-five millions is at present developed or is in course of development.

Power Available in Great Britain and in the British Empire.—With the noteworthy exceptions of Canada and New Zealand, practically

[1] Including projected extensions to plants now in operation.
[2] Projected but not yet constructed.

nothing had been done, prior to 1915, by any part of the British Empire, to develop or even systematically to investigate the possibilities of developing its water powers. It is true that a number of large installations had been constructed in India and Tasmania, but their aggregate output was relatively inconsiderable.

Since then, however, there has been a general tendency to initiate such investigations, and at the present time these are being carried out with varying degrees of thoroughness in India, Ceylon, Australia, South and East Africa, and British Guiana. While it is known that there is ample water power in Newfoundland, Nigeria, Rhodesia, Papua, and the Gold Coast, no very definite information is available, nor are any steps apparently being taken to obtain data in these countries.

The Water Power Committee of the Conjoint Board of Scientific Societies, which has been studying the state of investigation and development throughout the Empire since 1917, has, however, come to the conclusion that its total available water-power resources are at least equivalent to between 50 and 70 million horse-power.

Of the developed power in the Empire about 80 per cent. is in Canada. Throughout the remainder of its territories only about 700,000 horse-power is as yet developed, or only a little over 1 per cent. of the power available, a figure which compares with about 24 per cent. for the whole of Europe, and 21 per cent. for North America, including Canada and the U.S.A. These figures sufficiently indicate the relatively large scope for future development.

Power Available in Great Britain.—With a view of ascertaining the resources of our own islands, a Board of Trade Water Power Resources Committee was appointed in 1918. This Committee, which has just presented its final report, has carried out preliminary surveys of as many of the more promising sites as its limited funds allowed, and has obtained data from the Board of Agriculture for Scotland, the Ordnance Survey Department, the Ministry of Munitions, and from civil engineers in private practice, regarding a large number of other sites.

As might be anticipated, Scotland, with its comparatively high rainfall, mountainous area, and natural lochs, possesses relatively greater possibilities than the remainder of the United Kingdom, and investigation has shown that it offers a number of comparatively large schemes. Nine of the more immediately promising of those examined by the Committee have an average output ranging from 7,000 to 40,000 continuous 24-hour horse-power, and an aggregate capacity of 183,000 horse-power, while in every case the estimated cost of construction is such that power could be developed at a cost appreciably less than from a coal-fired station built and operated under present-day conditions. The aggregate output of the Scottish schemes brought before the notice of the Committee, some of which, however, are not commercially feasible at the moment, is roughly 270,000 continuous horse-power.

In addition to these there are a very large number of other small schemes which have not yet been investigated,[3] and it is probably well

[3] In a paper read before the Royal Society of Arts on January 25, 1918, Mr. A. Newlands, M.I.C.E., gave a list of 122 potential Scottish schemes, the capacity of which he estimated, on a very conservative basis, at 375,000 horse-power.

within the mark to say that there are water-power sites in the country capable of developing the equivalent of 400,000 continuous horse-power, or 1,500,000 horse-power over a normal working week, at least as cheaply as from a coal-fired installation.

A number of attractive schemes are also available in North Wales, though these are in general more expensive than those in Scotland.

Owing to the general flatness of the gradients, there are, except possibly around Dartmoor, no schemes of any large individual magnitude in England, but there are a large number of powers ranging from 100 to 1,000 horse-power which might be developed from river flow uncontrolled by storage.

Investigations on a few typical watersheds throughout England and Wales appear to show that the possible output averages approximately eight continuous horse-power per square mile of catchment area, which would be equivalent to an aggregate of about 450,000 horse-power. Although much of this potential output is not commercially feasible, it would give the equivalent of 500,000 horse-power over a normal working week if only 30 per cent. of it were fully utilised.

In the report recently issued by the Irish Sub-Committee of the Board of Trade Water Power Committee, it is estimated that approximately 500,000 continuous 24-hour horse-power is commercially available in Ireland, and that if utilised over a 48-hour working week, its capacity would be at least seven times as great as that of the engine power at present installed in the country for industrial purposes.

It appears then that, although the water-power possibilities of the United Kingdom are small in comparison with those of some more favoured countries, they are by no means so negligible as is commonly supposed, even in comparison with the present industrial steam-power resources of the country.

The capacity of the fuel-power plants installed for industrial and public-utility services in the United Kingdom in 1907 was approximately 9.8 million horse-power. Allowing for an increase of 15 per cent. since then, and an average load factor of 35 per cent., this is equivalent to 32,000 million horse-power hours per annum, or to a continuous 24-hour output of only 3.7 million horse-power.

According to Sir Dugald Clerk, the average consumption of coal per horse-power hour in this country is about 3.9 lb., which, on the above basis, would involve a total annual consumption of 55 million tons for industrial purposes, not including railways or steamships. This figure is in substantial agreement with the estimate of 60 million tons made for factory consumption in 1913 by the Coal Conservation Committee of the Ministry of Reconstruction, since this latter figure also includes coal used for heating and other manufacturing processes in factories.

Adopting this figure of 32,000 million horse-power hours as the annual demand for power for industrial purposes, it appears that the inland water-power resources of the United Kingdom are capable of supplying about 27 per cent. of this, a proportion which, in such an industrial country as our own, is somewhat surprisingly large.

Many of the small powers would be well adapted for linking up, as

automatic or semi-automatic stations, into a general network of electricity supply, or for augmenting the output of municipal supply works, as has been done so successfully, for example, at Chester, Worcester, and Salisbury.

The development of the many small schemes available in the Scottish Highlands would probably have a great effect on the social life of the community. It would go far towards reviving and extending those small local industries which should form an essential feature of the ideal rural township. Commercially such undertakings may appear to be of small importance, but as a factor in promoting the welfare of the State, economical and political, their influence can hardly be overestimated.

Some of the larger schemes in Scotland would lend themselves admirably to transmission to its industrial districts, while others, in close vicinity to the sea-board, would appear to be well adapted for supplying chemical, or electro-physical, or metallurgical processes.

There is a probability that at least two of these schemes will be developed in the near future. One of these—the Lochaber scheme—is capable of developing some 72,000 continuous horse-power, which is to be utilised largely in the manufacture of aluminium. It is interesting to note that when this scheme is completed the British Aluminium Corporation will have, with their station at Kinlochleven, an average continuous output of over 100,000 horse-power, and a maximum capacity of almost 140,000 horse-power.

The second—the scheme of the Grampian Power Company—is intended ultimately to develop upwards of 40,000 continuous horse-power, which it is proposed to use largely for general industrial purposes.

Should this latter scheme be carried to a successful conclusion it is likely to give an impetus to large-scale water-power development in Scotland. Its successful operation would certainly lead to the development of others of the same type, which would help to provide a much-needed home training-ground for British hydro-electric engineers.

While this is admittedly an inopportune moment to suggest anything in the nature of State co-operation in such developments, it may be pointed out that many of the Scottish powers in particular occur in sparsely populated districts, and that, although they would ultimately become remunerative, the difficulty of raising the capital necessary for their development is great. In view of their direct and indirect advantage to the community it would appear not unreasonable to advocate that financial assistance should be granted by the State in the earlier stages of such developments. If such assistance, say in the form of a loan maturing after a period of ten or fifteen years, could be granted, it would certainly give an immediate impetus to the development of water power in this country.

Conservation.—The importance of water-power development from the point of view of conservation of natural resources requires no emphasis. When the value of coal purely as a chemical asset, or as a factor in the manufacture of such materials as iron and steel, cement, &c., is considered, its use as a fuel for power purposes, when any other equally cheap source of energy is available, would appear, indeed, to be unjustifiable.

The consumption of coal in the best modern steam plant of large size, giving continuous output, would be about nine tons per horse-power year, and on this basis the world's available water power if utilised would be equivalent to some 1,800,000,000 tons of coal per annum. The world's output of coal in 1913 was approximately 1,200,000,000 tons, of which about 500,000,000 tons were used for industrial power purposes, so that on this basis 55,000,000 continuous water horse-power would be equivalent to the world's industrial energy at that date.

Not only does the use of water power lead to a direct conservation of fuel resources, but it also serves to a notable degree to conserve man power. To take an extreme example, each of the 40,000 horse-power units now being installed at Niagara Falls will require for operation two men per shift. It is estimated that to produce the same power from a series of small factory steam plants, over eight hundred men would be required to mine, hoist, screen, load, transport by rail, unload, and fire under boilers the coal required, while, if account be taken of the additional labour involved in horse transport, wear and tear of roads and of railroad tracks and rolling stock, the number would be considerably increased.

Uses of Hydro-Electric Energy.—While a large proportion of the energy developed from water power is utilised for industrial purposes and for lighting, power, and traction, an increasing proportion is being used for electro-chemical and electro-metallurgical processes. It is probable indeed that we are only on the threshold of developments in electro-chemistry, and that the future demand for energy for such processes will be extremely large.

In Norway the electro-chemical industry absorbed 770,000 horse-power in 1918, or approximately 75 per cent. of the total output, as compared with 1,500 horse-power in 1910. Of this some 400,000 horse-power was utilised in nitrogen fixation alone.

The production of electric steel in the U.S.A. increased from 13,700 tons in 1909 to 24,000 tons in 1914, and to 511,000 tons in 1918, this latter quantity absorbing 300 million kw. hours, equivalent to almost 400,000 continuous horse-power.

In Canada, in 1918, the pulp and paper industry absorbed 450,000 horse-power, or 20 per cent. of the total, while the output of central electric stations amounted to 70 per cent. of the total.

The electrification, on a large scale, of trunk line railways is also a probability in the not distant future. In the U.S.A. 650 miles of the main line of the Chicago, Milwaukee, and St. Paul Railway, comprising 850 miles of track, have been electrified, the power for operation being obtained from hydro-electric stations. In France much of the track of the Compagnie du Midi in the region of the Pyrenees has been electrified with the aid of water power; much of the Swiss railway system has been electrified; and the electrification of many other trunk lines on the European continent is at present under consideration.

Quite apart from the probable huge demand in the distant future for energy for the manufacture of artificial fertilisers by some system of nitrogen fixation, agriculture would appear to offer a promising field for the use of hydro-electric power.

Much energy is now being utilised in the U.S.A. for purely

agricultural purposes. In California, for example, there is in effect one vast system of electrical supply extending over a distance of 800 miles with 7,200 miles of high-tension transmission lines. This is fed from seventy-five hydro-electric stations, inter-connected with forty-seven steam plants, to give a total output of 785,000 horse-power. A further group of thirteen hydro-electric schemes now under construction will add another 520,000 horse-power. A large proportion of this power is used in agriculture, and a census in 1915 showed that electric motors equivalent to over 190,000 horse-power were installed on Californian farms. The Californian rice industry is almost wholly dependent on irrigation made possible by electric pumping, while most of the mechanical processes involved in farming are being performed by electric power.

There can be little doubt that the economic development of many of our tropical dependencies is bound up in the development of their water-power resources. Not only would this enable railroads to be operated, irrigation schemes to be developed, and mineral deposits to be mined and worked, but it would go far to solve the black labour problem, which promises to be one of some difficulty in the near future.

While those outlets for electrical energy which are now in sight promise to absorb all the energy which can be cheaply developed for many years to come, there are many other probable directions in which cheap energy would find a new and profitable outlet. Among these may be mentioned the purification of municipal water supplies; the sterilisation of sewage; the dehydration of food products; and the preservation of timber.

Scope for future Water-Power Development.—The figures already quoted indicate that the scope for inland water-power development throughout the world, and more particularly throughout the British Empire, is likely to be large for many years to come, and it is gratifying to know that British engineers are prepared to play a large part in such development work.

The utilisation of this water power is likely to give rise to some economic problems of interest and importance. When industrial conditions have again become stabilised, the competitive ability of the various nations will depend largely on economy in the application of energy to production and transportation, and the possession of cheap water power is likely largely to counterbalance the possession of such resources as coal and iron as a measure of the industrial capacity of a nation.

While it is probably true in industrial communities that the most attractive water-power schemes have already received attention, many of those available in countries which have hitherto been non-industrial are capable of extremely cheap development and will certainly be utilised as soon as a market for their output can be assured.

It is in such countries that the result of these developments is likely to be most marked, and will require most careful consideration. Thus the hydro-electric survey of India now being carried out by the Indian Government indicates that very large water-power resources are available in the country, and that, although a few large schemes have been or are being developed, the resources of the country are practically

untouched. There can be little doubt that in the course of time a large amount of cheap energy will be available in India for use in industrial processes, and as the country possesses a large and prolific population readily trained to mechanical and industrial processes, along with ample supplies of raw material for many such processes, all the conditions would appear to be favourable for its entry into the rank of manufacturing and industrial nations.

Modern Tendencies in Water-Power Development.—The large amount of attention which has been concentrated on the various aspects of water-power development during the past ten years has been responsible for great modifications and improvements in the design, arrangement, and construction of the plant.

Broadly speaking these have been in the direction of increasing the size, capacity, reliability, and efficiency of individual units; of improving the design of the turbine setting and of the head and tail works; of increasing the rotative speed of low head turbines; of detailed modifications in the reaction type of turbine to enable it to operate under higher heads than have hitherto been considered feasible; and of increasing the voltage utilised in transmission.

The capacity of individual units has been increased rapidly during recent years, and at the present time units having a maximum capacity of 55,000 horse-power under a head of 305 feet are being installed in the Queenston-Cheppewa project at Niagara, while units of 100,000 horse-power are projected for an extension of the same plant.

These modern high-power turbines are usually of the vertical shaft, single runner type, with the weight of the shaft, runner, and generator carried from a single thrust bearing of the Michell type. This type lends itself to a simple and efficient form of setting, while the friction losses in the turbine are extremely low. As a result of careful overall design it has been found possible to build units of this type having an efficiency of approximately 93 per cent.

One of the great drawbacks of the low head turbine in the past has been its relatively slow speed of rotation, which necessitated either a slow speed, and consequently costly generator, or expensive gearing. As a result of experiment it has, however, been possible so to modify the form of the runner as greatly to increase the speed of rotation under a given head without seriously reducing the efficiency.

Investigations in this direction are still in progress and promise to give rise to important results. At the present time, however, turbines are in existence which are capable of working efficiently at speeds at least five times as great as would have been thought feasible ten years ago.

The non-provision of a suitable pipe line has, until recent years, tended to retard the development of plants for very high heads. Under such heads the necessary wall thickness, even with a moderate pipe diameter, becomes too great to permit of the use of riveted joints. Recent developments in electric welding and oxy-acetylene welding have, however, rendered it possible to construct suitable welded pipes, and by their aid, and by the use of solid drawn steel pipes in extreme cases, it has been found possible to harness some very high falls. The

highest as yet utilised is at the Fully installation in Switzerland. Here the working head is 5,412 feet, corresponding to a working pressure of 2,360 lb. sq. in. The pipe line is 19.7 in. in diameter and $1\frac{3}{4}$ in. thick at its lower end, and each of the three Pelton wheels in the power house develops 3,000 horse-power, with an efficiency of 82 per cent.

Until comparatively recently the Pelton wheel was looked upon as the only practicable turbine for high heads, and the use of the Francis turbine was restricted to heads below about 400 feet. This was due partly to the fact that a reaction turbine of comparatively small dimensions gives a large output under a high head, and except in turbines of comparatively large power the water passages become very small, and the friction losses in consequence large.

A further and more important reason for the general choice of the Pelton wheel for high heads was the fact that in the earlier Francis turbines, when operating under heads involving high speeds of water flow, corrosion of the runner was very serious. This corrosion is now generally attributed to the liberation of air containing nascent oxygen, at points where eddy formation causes regions of low pressure. Careful design of the vanes has enabled this to be largely prevented in modern runners, and in consequence the field of useful application of the Francis turbine has been extended until at present turbines of this type are operating successfully under a head of 850 feet, and this limit will probably be exceeded in the near future.

The great increase in all constructional costs since 1914 has increased the cost of the average hydro-electric plant by something of the order of 150 per cent., and since the cost of energy produced by such a plant is mainly due to fixed charges on the capital expenditure, this cost has gone up in an even greater proportion owing to the higher interest charges now demanded.

It is true that the same increased cost applies within narrow limits to the output from every steam plant erected since the War, and the relative position of the two types of power plant with coal at about 25*s.* per ton is much the same as before the War.

The fact remains, however, that a newly constructed hydro-electric plant has often to compete in the market with a steam plant built in pre-war days whose standing charges are comparatively low, and in order to enable it to do so with success the constructional cost must often be reduced to a minimum compatible with safe and efficient operation. With this in view many modifications in design and construction have been introduced in recent plants, but there would still appear to be ample scope for investigation into the possibility of reducing the first cost by modifying many of the details of design and methods of construction now in common use.

Among recent modifications in this direction may be mentioned—

1. The elimination of the dam in storage schemes in which natural lochs or reservoirs are utilised, this water level being drawn down in times of drought instead of being raised in times of flood. This reduces the cost of construction appreciably in favourable circumstances, and eliminates the necessity for paying compensation for flooding of the land surrounding the reservoir.

2. The substitution, where feasible, of rockfill dams for those of masonry or monolithic concrete.
3. The introduction of outdoor installations with the minimum of power-house construction.
4. The simplification of the power plant.

Some progress has already been made in these directions, and it is probable that experience based on recent installations and experimental investigations will enable considerable further progress to be made.

Research in Hydro-Electric Problems.—There are few branches of engineering in which research is more urgently required and in which it might be more directly useful.

Among the many questions still requiring investigation on the civil and mechanical side may be mentioned—

1. *Turbines.*—Investigation of turbine corrosion as affected by the material and shape of the vanes.

Effect of erosion due to sand and silt.

Resistance to erosion offered by different materials and coatings.

Bucket design in low head high-speed turbines.

Draft tube design.

Investigation of the directions and velocities of flow in modern types of high-speed turbines.

Investigation of the degree of guidance as affected by the number of guide and runner vanes.

2. *Conduits and Pressure Tunnels.*—The design of large pipe lines under low heads with the view of reducing the weight of metal. The investigation of anti-corrosive coatings, so as to reduce the necessity for additional wall thickness to allow for corrosion.

Methods of strengthening large thin-walled pipes against bending and against external pressures.

Methods of lining open canals and of boring and lining pressure tunnels.

Effects of curvature in a canal or tunnel.

3. *Dams.*—Most efficient methods of construction and best form of section especially for rockfill and earthen dams. Best methods of producing water tightness.
4. *Run-off data.*—Since the possibility of designing an installation to develop the available power efficiently and economically depends in many cases essentially on the accuracy of the run-off data available, the possession of accurate data extending over a long series of years is of great value.

While such data may be obtained either from stream gaugings or from rainfall and evaporation records, the former method is by far the more reliable. For a reasonable degree of accuracy, however, records must be available extending over a long period of years, and at the present moment such data are available only in very few cases.

Where accurate rainfall and evaporation records are available, it is

possible to obtain what is often a sufficiently close approximation to the run off, but even rainfall records are not generally at hand where they are most required, and even in a district where such records are available, they are usually confined to easily accessible points, and are seldom extended to the higher levels of a catchment area where the rainfall is greatest. Even throughout the United Kingdom our knowledge of the rainfall at elevations exceeding 500 feet is not satisfactory, and little definite is known concerning that at elevations exceeding 1,000 feet.

In this country evaporation may account for between 20 and 50 per cent. of the annual rainfall, depending on the physical characteristics of the site, its exposure, mean temperature, and the type of surface covering. In some countries evaporation may account for anything up to 100 per cent. of the rainfall. As yet, however, few records are available as to the effect of the many variables involved. An investigation devoted to the question of evaporation from water surfaces, and from surfaces covered with bare soil and with various crops, under different conditions of wind, exposure, and mean temperature, would appear to be urgently needed. If this could be combined with an extension of Vermeulle's investigation into the relationship between rainfall, evaporation, and run-off on watersheds of a few characteristic types, it would do much towards enabling an accurate estimate of the water-power possibilities of any given site to be predetermined.

Even more useful results would follow the initiation of a systematic scheme of gauging applied to all streams affording potential power sites.

Among other questions which are ripe for investigation may be mentioned:—

1. The combined operation of steam and water power plants to give maximum all-round efficiency.
2. The relative advantages of high voltage D.C. and A.C. generation and transmission for short distances.
3. The operation of automatic and semi-automatic generating stations.

Tidal Power.—The question of tidal power has received much attention during the last few years. In this country it has been considered by the Water Power Resources Committee of the Board of Trade, who have issued a special tidal power report dealing more particularly with a suggested scheme on the Severn. The outline of a specific scheme on the same estuary was published by the Ministry of Transport towards the end of 1920.

In France a special commission has been appointed by the Ministry of Public Works to consider the development of tidal power, and it has been decided to erect a 3,000 kw. experimental plant on the coast of Brittany. With the view of encouraging research the Government proposes to grant concessions, where required, for the laying down of additional installations.

The tidal rise and fall around our coasts represents an enormous amount of energy, as may be exemplified by the fact that the power obtainable from the suggested Severn installation alone, for a period

of eight hours daily throughout the year, would be of the order of 450,000 horse-power.

Many suggestions for utilising the tides by the use of current motors, float-operated air compressors, and the like have been made, but the only practicable means of utilising tidal energy on any large scale would appear to involve the provision of one or more dams, impounding the water in tidal basins, and the use of the impounded water to drive turbines.

The energy thus rendered available is, however, intermittent; the average working head is low and varies daily within very wide limits, while the maximum daily output varies widely as between spring and neap tides.

If some electro-chemical or electro-physical process were available, capable of utilising an intermittent energy supply subject to variations of this kind, the value of tidal power would be greatly increased. At the moment, however, no such process is commercially available, and in order to utilise any isolated tidal scheme for normal industrial application it is necessary to provide means for converting the variable output into a continuous supply constant throughout the normal working period.

Various schemes have been suggested for obtaining a continuous output by the co-ordinated operation of two or more tidal basins separated from each other and from the sea by dams with appropriate sluice gates. This method, however, can only get over the difficulty of equalising the outputs of spring and neap tides if it be arranged that the maximum rate of output is that governed by the working head at the lowest neap tide, in which case only a small fraction of the available energy is utilised.

When a single tidal basin is used it is necessary to provide some storage system to absorb a portion of the energy during the daily and fortnightly periods of maximum output, and for this purpose the most promising method at the moment appears to involve the use of an auxiliary high-level reservoir into which water is pumped when excess energy is available, to be used to drive secondary turbines as required. It is, however, possible that better methods may be devised. Storage by the use of electrically heated boilers has been suggested, and the whole field of storage is one which would probably well repay investigation.

If a sufficiently extensive electrical network were available, linking up a number of large steam and inland water-power stations, a tidal-power scheme might readily be connected into such a network without any storage being necessary, and this would appear to be a possibility which should not be overlooked in the case of our own country.

Investigation necessary.—A tidal-power project on any large scale involves a number of special problems for the satisfactory solution of which our present data are inadequate.

Thus the effect of a barrage on the silting of a large estuary, and the exact effect on the level in the estuary and in the tidal basin at any given time, can only be determined by experiment, either on a small installation, or preferably on a model of the large scheme.

Many of the hydraulic, mechanical, and electrical problems involved

are comparatively new, and there is little practical experience to serve as a basis of their solution.

Among these may be mentioned:—

1. The most advantageous cycle of operations as regards working periods, mean head, and variations of head.
2. The methods of control and of sluice-gate operation.
3. Effect of changes of level due to wind or waves.
4. The best form of turbine and setting and the most economical turbine capacity.
5. The possibilities of undue corrosion of turbine parts in salt water.
6. The best method of operation; constant or variable speed.
7. Whether the generators shall be geared or direct driven.
8. Whether generation shall be by direct or alternating current.

The questions of interference with navigation and with fisheries; of utilising the dam for rail or road transport across the estuary; and, above all, economic questions connected with the cost of production, and the disposal of the output of such an installation, also require the most careful consideration before a scheme of any magnitude can be embarked upon with assurance of success.

In view of the magnitude of the interests involved, and of the fact that rough preliminary estimates indicate that to-day current even for an ordinary industrial load could be supplied from such an installation at a price lower than from a steam generating station giving the same output with coal at its present price, it would appear desirable that these problems should receive adequate investigation at an early date.

Facilities for Research in Hydraulic and Cognate Problems.—In view of the considerations already outlined, and especially in view of the large part which British engineering will probably play in future water-power developments, the provision on an adequate scale at some institution in this country of facilities for research on hydraulic and cognate problems connected with the development of water power is worthy of serious attention.

At present the subject is treated in the curriculum of the engineering schools of one or two of our universities, but in no case is the laboratory equipment really adequate for the purpose in question.

What is required is a research laboratory with facilities for experiments on the flow of water on a fairly large scale; for carrying out turbine tests on models of sufficient capacity to serve as a basis for design; and, if possible, working in conjunction with one or more of the hydro-electric stations already in existence, or to be installed in the country, at which certain large-scale work might be carried out.

The provision of such a laboratory is at the moment under consideration in the United States, and in view of the rapidity with which the designs of hydraulic prime movers and their accessories are being improved at the moment, it would appear most desirable that the British designer, in order that the deservedly high status of his products should be maintained and enhanced, should at least have access to equal facilities, and should, if necessary, be able to submit any outstanding problems to investigation by a specially trained staff.

The extent to which our various heat-engine laboratories have been able of recent years to assist in the development of the internal combustion engine, and to which our experimental tanks have assisted in the development of the shipbuilding industry, is well known to most of us, and the provision of similar facilities to assist in the development of our hydro-electric industry would probably have equally good results in this connection.

As a result of representations made by the Water Power Committee of the Board of Trade, I understand that it has now been decided to initiate a Chair of Hydro-Electric Engineering in some one university, and it is greatly to be hoped that funds may be available to enable the necessary laboratory to be designed and equipped on a scale commensurate with the importance of the work which it would be required to undertake.

[Note.—Owing to the resignation of Sir J. C. Frazer, F.R.S., from the presidency of Section H (Anthropology) shortly before the Meeting, no address was delivered in that Section.]

THE AIMS AND BOUNDARIES OF PHYSIOLOGY.

ADDRESS TO SECTION I (PHYSIOLOGY) BY

SIR WALTER M. FLETCHER, K.B.E., M.D., Sc.D., F.R.S.,

PRESIDENT OF THE SECTION.

Upon the occasion of our meeting in this metropolitan city of Edinburgh, the seat of an ancient university and a great centre of medical study and practice, it has occurred to me that it may be profitable for us to consider the part which physiology should rightly take in its relation to national life, to learning, and to medicine. Not only the place of our meeting, indeed, but some special circumstances of the present time seem to make it fitting that we should here review the progress, the proper scope, and the prospects of our chosen subject. We are now just half a century from the time when physiology first came to take its present position in this kingdom as one of the great branches of university learning and as a vital part of medical education. We have seen the close of a War which, though it diverted and distorted the progress of the science, yet brought it great opportunities of service in national life and taught us lessons, here as in so many other directions, of which we shall do well to take profit. The passing of the War, moreover, has brought a period of change and unrest during which impulses towards reform are being chequered by the results of fatigue or reaction. Both here and in America it may be said that, while physiology has come from the War with enlarged outlook and responsibilities, it is exposed to some new and perhaps dangerous influences in the present time of rapid resettlement. It may well be worth while, then, to look now both forward and back, to see the road by which we have hitherto been led and its relations to that which now lies before us.

I.

Physiology, as the passing generation has known it, took shape and established its boundaries in this country just fifty years ago, when, shaking off its long subordination to anatomy, it was brought to a new life of recognition and progress. The seventeenth century had seen England famous for her school of physiologists, leading the rest of the Continent in experimental results and in new ideas. Working upon the foundations laid by Harvey, that brilliant group at Oxford—Boyle, Lower, Mayow, Willis—had brought new light to the study of the living body. Nor was their service only recognised by fellow-workers abroad or by those that came after. Their names and fame were on fashionable lips; like that of their predecessor, Harvey himself, under Charles I., and of that other Cambridge philosopher, Glisson,

their immediate contemporary, their work was aided by the direct interest and favour of the sovereign. But, during the eighteenth century and the earlier part of the nineteenth, eclipse fell upon the light that had thus burned so brightly, though isolated gleams shone here and there. James Jurin, under George II., applied the Newtonian principles to calculating the work done by the heart and to other problems of the body, but his efforts to lay true and exact foundations for the study of disease were premature in the absence of experimental data. Stephen Hales, Chaplain to the future George III., made the first measurements of blood pressure in his garden at Teddington, and made many far-reaching observations of the first importance; but, as he wrote, there was indeed 'abundant room for many heads and hands to be employed in the work, for the wonderful and secret operations of Nature are so involved and intricate, so far out of the reach of our senses . . .'; and it was not then or till much later that many heads and hands were ready to be employed. Neither of these men had effective influence upon the thought or practical affairs of their day, either within the universities or outside them.

Physiology, as we know it now in this country, took its shape in a new revival which may be reckoned as beginning half a century ago. All our chief schools may be said to derive their lineage from that new home of active and unshackled inquiry—I mean University College, in Gower Street, London—and from the influence there of an Edinburgh graduate, William Sharpey, who at the age of thirty-four was taken from the Edinburgh school to be Professor of Anatomy and Physiology. Here, from 1836 until 1874, Sharpey was inspiring a group of younger minds with his eager outlook. Already in France the new experimental study of the living functions was being established by Claude Bernard—that true 'father in our common science,' as Foster later called him; already in Leipzig Ludwig, transmitting the impulse of Müller's earlier labours, had founded that school of physiology which moulded the development of the subject in Germany and other countries, and had very strong early influence upon several of those who were later to become leaders with us. England had lost the pre-eminence that Stuart kings at all events had valued and promoted. Learning had become identified in English society with the mimetic use of the dead languages, and progress at the two universities—even at the Cambridge of Newton, where mathematics kept independence of thought alive—was still impeded by the grip of ecclesiastical tradition and by sectarian privilege. But at University College learning had been unfettered. Here Sharpey and his colleagues were in touch with the best progress in France and Germany, and here the organised study of physiology as a true branch of university study may be said to have begun. Its formal separation from anatomy came later and irregularly; a separate Chair of Physiology was not created at University College until 1874, nor at Cambridge or at Oxford until 1883.

We ought in piety to recognise that this tardy reflection of Continental progress in our own subject, like parallel movements in other subjects, had in its early stages received invaluable aid from the Prince

Consort, who, familiar with the progress of other countries, had lent his influence and sympathy to many men of science in their struggle against the insularity and apathy of the wealthy and governing classes of the earlier Victorian days. The curious may take note that the first outward mark of recognition given by the official and influential world to the existence of physiology as such was given not, as in other and poorer countries much earlier, by the endowment of some chair or institute for research and teaching, but by an act of symbolic representation. For, when the expensive statuary of the Albert Memorial was completed in 1871, it was found that 'Physiology,' betokened by a female figure with a microscope, had been given its place among the primary divisions of learning and investigation acknowledged in that monument to the Prince.

From Sharpey himself and his personal influence we may trace directly onwards the development of all the chief British schools of physiology whose achievements have in the past half-century restored Britain to more than her old pride of place in this form of service to mankind. We here fittingly acknowledge first the close link with Sharpey which we find to-day in Sir Edward Sharpey Schäfer, who, after fruitful years in his old teacher's place at University College, brought that personal tradition back to this great school of Edinburgh from whence it originally came. At University College itself the line has been continued with undimmed lustre by Starling and Bayliss and their colleagues to the present day. From Sharpey's school again are derived the great branches which have sprung from it, both at Oxford and at Cambridge. Burdon Sanderson, Sharpey's immediate successor at University College, proceeded thence to Oxford and founded there, against many difficulties of prejudice and custom, the school of physiology which Gotch, Haldane, and Sherrington have nevertheless maintained so brilliantly in succeeding years. To Cambridge, Michael Foster, one of Sharpey's demonstrators, was invited in 1870 by Trinity College to be Praelector in Physiology and Fellow of the College. This enlightened and then almost unprecedented act, no less than the personal qualities of Foster that so aboundingly justified it, I would, as in private duty bound, hold here in special remembrance. Under Foster's influence there came into being at Cambridge a strong and rapidly growing school of physiologists, from Langley, Gaskell, Sherrington, Hopkins, to numerous successors. There sprang from him, too, a new impetus to other subjects, through his pupils Francis Balfour and Adam Sedgwick to embryology and zoology, through Vines and Francis Darwin to botany, through Roy to pathology. From Foster again through Newell Martin, who, coming with him from London, had caught not only inspiration from him but some of his power of inspiring others, and who left Cambridge for a Chair at Baltimore in 1876, we may derive a large part of the growth and direction of physiology since that time in the United States and in Canada. The rapid progress of all these biological sciences at Cambridge within a single generation and the volume of original work poured forth depended, of course, upon two necessary conditions. The first is one which has never failed in this country—the existence of men fitted by temperament to advance

knowledge by experiment. The second has been the supply of living necessities through the ancient endowments of the colleges, and these in the Cambridge of the last half-century have been freely and increasingly used in catholic spirit for the increase of any of the borders of knowledge.

If these have been the chief lines of descent along which our present heritage has come to us, as mind has influenced mind and the light has been passed from hand to hand, what has been the outcome as we look back over the half-century to those small beginnings?

Truly we can say that the workers in this country have in that short space of years laid the whole world under a heavy debt. In whatever direction we look we seem to see that in nearly all the great primary fields of physiological knowledge the root ideas from which further growth is now springing are in great part British in origin, and based upon the work of British experimenters. If we consider the blood circulation we find that our essential ideas of the nature of the heart-beat were established by Gaskell, and that other first principles of its dynamics and of its regulation have been laid down by successors to him still with us; that the intricate nervous regulation of the arterial system has had its chief analyses here, and that here have been made more recently the first demonstrations of the part played by the minute capillary vessels in the regulation of the distribution and composition of the blood. Of the central nervous system the modern conceptions of function in terms of the purposive integration of diverse impulses along determined paths have sprung direct from British work, while the elementary analysis of the structure and functions of the sympathetic nervous system has been almost wholly British in idea and in detail. As with the nervous regulation of the body, so with the chemical regulation of function by travelling substances—the so-called ‘hormones,’ or stimulants from organ to organ—this, too, is a British conception enriched by numerous examples drawn from experimental work in this country. In the study of nutrition, of the primary ‘foodstuffs,’ proteins, carbohydrates, fats, salts, and water, whose names in their supposedly secure sufficiency were written with his own hand by Foster upon the blackboard shown in his portrait by Mr. John Collier, to typify, as we may imagine, a basal physiological truth, we have come to learn that these alone are not sufficient for growth and life in the absence of minimal amounts of accessory unknown and unstable substances, the so-called ‘vitamins,’ which are derived from pre-existent living matter. This conception, undreamt of to the end of the nineteenth century, has fundamental value in medicine and in agriculture, and has already begun to bear a harvest of practical fruit of which the end cannot be seen or the beneficence measured. This discovery stands to our national credit, and large parts of its development and application have been due to recent British work. If we turn to the regulation of respiration and its close adaptation to body needs, that also, as it is now known to the world, is known as British labours have revealed it, just as the finer analyses of the exchanges of gas between the air and the blood and between the blood and the body substance have been made with us. The actual modes by which

oxygen is used by the tissues of the body, its special relations to muscular contraction, the chemical results of that contraction, the thermal laws which it obeys—all these fundamental problems of living matter have seen the most significant steps to their solution taken within the past generation in this country.

Work of this kind brings permanent enrichment to the intellectual life of mankind by giving new and fuller conceptions of the nature of the living organism. That we may think is its highest function; but it does more than this. Just as all gains in the knowledge of Nature bring increase of power, so these discoveries of the past fifty years have their place in the fixed foundations upon which alone the science and the arts of medicine now or in the future can be securely based. The special study of disease, its cure and prevention, has had notable triumphs here and elsewhere in the same half-century, and these as they come must make as a rule a more spectacular appeal to the onlooker. Yet it is the accumulating knowledge of the basal laws of life and of the living organism to which alone we can look for the sure establishment either of the study of disease or of the applied sciences of medicine. As we have seen, there are few indeed among the fields of inquiry in the whole range of physiology in which the British contributions to the common stock of ascertained knowledge or of fertile idea do not take a foremost place. It would be impiety not to honour, as it would be stupidity to ignore, these plain facts, which, indeed, are now perhaps more commonly admitted abroad than recognised at home. There is no occasion here for any spirit of national complacency—rather the reverse, indeed. British workers at no time earlier than the War have had the menial assistance or other resources which their colleagues in other countries have commonly commanded, and too often the secondary and relatively easy developments of pioneer work done in this country have fallen to well-equipped and well-served workers elsewhere. If in the past half-century better support had been available from public or private sources, or at the older universities from college endowments, it is impossible for any well-informed person to doubt that a more extended, if not a more diversified, harvest would have been won.

We stand too near to this remarkable epoch of progress to appraise it fairly. In the same span of years Nature has yielded many fresh secrets in the physical world under cross-examination by new devices which have themselves been lately won by patient waiting upon her. So great a revelation of physical truth has been lately made in this country, bringing conceptions of space and of matter so swiftly changing and extending, that our eyes are easily dimmed to the wonders of that other new world being unfolded to us in the exploration of the living organism. Only the lapse of time can resolve the true values of this or that direction of inquiry, if indeed there be any true calculus of 'value' here at all. We seem to see in the progress of physiology, not at few but at many points, that we stand upon new paths just opening before us, which must certainly—as it seems—lead quickly to new light, to fuller vision, and to other paths beyond. The advances of the next half-century to come must far exceed and outshine those

due to the efforts of the half-century just closing; that is probably the personal conviction of us all. Yet we may still believe that through all the history of mankind recognition will be given and honour be paid to the steps in knowledge which were made first and made securely in the period we now review. The men who have done this work will not take pride in it for themselves; they know that their strength has not been their own, but that of the beauty which attracted them, and of the discipline which they obeyed. They count themselves happy to have found their favoured path. Other and more acute minds might have usurped their places and found greater happiness for themselves if, under a social ordering of another kind, they had been turned to the increase of knowledge instead of to the ephemeral, barren, or insoluble problems of convention and competition. By how much the realised progress towards truth and the power brought by truth might have been increased under a changed social organisation we can never know, nor can we guess what acceleration the future may bring to it if more of the best minds are set free within the State for work of this highest kind, what riches may be added to intellectual life, or what fuller service may be given to the practical affairs of man and to the merciful work of medicine.

II.

To the story of progress which has just been sketched in outline the War brought inevitable interruption and change. To the more obvious disturbances and wastage of war I need not here refer, but I would point to some influences of that time which will be found, I think, to have left permanent effects, and on the whole good effects, upon the position and tendencies of physiology. Before 1914 physiology was being developed, as we have seen, in its still youthful status as one of the primary departments of knowledge; it had become a subject of independent university rank. Large and important parts of this development had proceeded at one or other of the ancient universities, out of touch with great centres of population, and out of touch, therefore, with immediate medical needs. In some degree this was not without advantage, and for two main reasons. Detachment from the pressure of need allowed the free pursuit of knowledge for its own sake and a full surrender to the hintings of Nature, wherever her clues might lead the inquirer. Experience amply showed, moreover, that when physiology was presented among other university subjects for study it gained, first as recruits and later as distinguished workers, many able young men who were attracted to it, often from other subjects, by the fascinations of its problems, and without regard to any of its potential applications to medical or any other practical ends. These were great gains which it would be easy, if it were not unnecessary, to illustrate by many convincing examples. Yet there were some heavy counterweights on the other side of the balance. The practical and urgent needs of humanity as found at the hospitals were not brought with full or due effect to the notice of physiologists. Those in charge of hospital patients were not selected to advance, or habitually

engaged in advancing, medical knowledge, and new physiological conceptions as they took shape in our laboratories only slowly and partially came to have effect in medical practice and medical study. The physiologist, to his own certain loss and to the no less certain loss of medicine, held aloof from the bedside, often when access was possible, and remained immersed in his laboratory interests. Little pressure, indeed, was ever brought to bear upon him by the physician to come to his aid. Connected with the evils of this separation was the divorce which the accidents of development had set up between physiology and pathology, as though the study of the damaged body could be separated from the science of the living organism and of its reactions to any disturbance from the normal. Yet, while the physician had come to tolerate the approach of the pathologist to the bedside, it occurred too rarely that he felt the need of the physiologist, or made himself familiar with new devices of physiological investigation.

If from a hospital in time of peace the most obvious call had seemed in the past to come from the side of infective disease or morbid process for the help of the pathologist, in war the stresses put upon the healthy human body made the physiologist and his methods indispensable. Bacteriological work and studies of immunity had their prominent place, of course, in the detection and prevention of infective disease, and wonderful were many of the achievements seen under this head. But in a sense the more complete the prevention of infective disease the more apparent became the physical stresses of war. The violences offered in modern warfare to the human body—whether through exertion and exposure, by terror or excitement, in physical damage by lead or steel or in chemical attacks by poison, and not least through the incredible stresses of flying high and fighting in the air—all these brought many new and urgent calls for precise physiological knowledge and for new studies by the physiologist. The results of pain and fear, of hæmorrhage, of ' shock ' by wound or operation—all these needed further analysis before sound treatment could be devised or improved. New studies were needed of changes in blood-pressure and blood-volume and in the qualities of the blood itself, new inquiries into the finer vessels of blood circulation and their relation to the nervous and other systems, and new analyses of the chemical mechanisms of the body and of the modes by which want of oxygen is met by adaptation or leads to final damage. But the well-nigh incredible demands made upon the machinery of man's body in and behind the battle-line, in all situations upon the land or under the earth, high in the upper air, in the sea or within its depths, by no means make up the tale. Our forces were engaged in every climate, from the Equator to the Arctic regions, and were faced by innumerable local or accidental variations of diet. Here again were required the applications of physiological studies of heat loss and of heat production to manifold practical problems of clothing and of diet. What would have seemed a fanciful fairy tale barely twenty years ago might in particular be told of the miracles wrought by the studied application of our new knowledge of ' vitamins ' in diet, in saving from painful disease or death many thousands of men in diverse climates and fields of war.

At home the bodies of the civilian population were exposed to many stresses, often hardly less than those of active service. Men and women alike were exposed to arduous toil, to dangerous occupation, to poisons of many kinds needed for munitions, and in all these dangers the guidance of the physiologist was needed for the avoidance of industrial fatigue and loss of output and for devising protection against industrial poisoning. The whole nation was threatened by the menace of starvation, and our escape from that, itself one of the governing conditions of our ultimate victory, was due to a system of rationing and of the management of food materials, animal and vegetable, which was based on accurate physiological knowledge, won by experimental methods.

I touch on these points here briefly and in outline only in order to draw attention to the special influence which, as I think, the War has exercised upon the position of physiology in this country. The physiologists gave no exceptional help to the nation during the War; the exponents of every branch of science were needed, were ready, and were used, in our national crisis. Hardly one division of science can be named the deficiency of which would not have made defeat inevitable. It is a truism and a commonplace to say that no bravery and no fortitude could have avoided defeat without the help of scientific men and of the fruits of experimental science, though that commonplace has not, I think, ever yet been enshrined in the addresses or thanks of Parliament or in the prayers and thanksgivings of our churches. But we may recognise, perhaps, that the nation as a whole, and those especially who have the government, public or private, of large groups of men in their hands, have learned that obedience to physiological law is a first necessity for the maintenance of the body machinery in health and for its effective and harmonious use. They have come to know, moreover, that the men who alone can guide them to this obedience are those who have learned in the school of investigation from Nature herself. The nation has seen a Minister fall whose control of the people's food was not based upon physiological law, and his successor, whose adoption of the teaching of physiological experiment was early and faithful, gain renown. Nor was this by any means an isolated object-lesson. There is no doubt, surely, that physiologists have a new vista before them of immense public usefulness, if they will hold themselves in readiness to give the same kind of service to the country in the stress of her industrial life during peace as they gave so freely and to such effect in time of war.

But if the War brought these lessons to the general public, what lessons have come from it to the physiologist himself? I would only recall briefly here the considerations which were brought home with sufficient clearness to us all, I think, during and after the closing stages of the struggle. The War, in the first place, displayed before us new and gigantic fields of physiological study. Viewing these so far as we can, even at this distance, dispassionately, we see how the stresses and accidents of warfare in all their variety offered to our study a series of experiments made upon the human body, and on a gigantic scale. Only by disciplined study of the results at all stages of these trials of war in all their varying

degrees of horror and distress could effective aid be given in palliation of suffering or its avoidance. It was inevitable that study of this kind and upon so great a scale should result—as, happily, it did result—in much permanent gain to physiological knowledge and to the beneficent power that all sound knowledge brings. New insight was given into the functional patterns of the nervous system and into the orderly hierarchies, so to speak, under which this or that function is brought into subordination to another of superior rank, and new knowledge was gained of the phenomena of separation and repair in the outlying nerve-trunks. Accurate information was collected of the nutritional needs, quantitative or qualitative, of human beings under varying conditions; and, in particular, many special conditions of warfare brought to the test, established the fundamental usefulness, and stimulated the growth of that newest chapter in physiology already mentioned—that dealing with the elusive but potent accessory factors in nutrition—the vitamins. These examples must suffice where scores of others familiar to all of you might be given.

In the second place, the experience of the War has had wholesome effect from its tendency to remove the barriers that here and there had grown up between physiologists and the practical needs of medicine. Physiologists had valued, and justly valued, their academic freedom of inquiry within the universities, and, indeed, we know that practical utility could not be better served in the long run than by the detached pursuit of knowledge for its own sake. But, partly for reasons of hospital and professional organisation already touched upon, and partly because, to its obvious and immense gain, physiology had attracted from other paths men who were not, and had never become, medical men, there were some capital parts of the subject of which the chief explorers had never used the medical field of work or brought to medicine the weapons they had, perhaps unwittingly, at command. We can recognise already that this partial divorce has been changed by the War into a union likely to be increasingly fertile. Of the professorial chairs of medicine or directorships of medical units established since the War, for the advance of medical knowledge within hospitals in accordance with the university standards and ideals acknowledged in other subjects of study, it is remarkable that to the greater number of these there have already been appointed men whose training has been in the methods of the physiological laboratory, and who applied that training to urgent medical problems of the War. There is hardly any one of our schools of physiology, moreover, to which some piece of living experience has not been brought in these last years to enforce the old lesson of the value to science itself of bringing natural knowledge to its fullest utilitarian applications. The practical fruits of scientific labour are found, if our hands are put out to gather them, to contain within themselves, like the natural fruits of the earth, the very seeds from which new knowledge and new fertility will spring. Many of our leaders in physiology brought to the problems of war the accumulated knowledge of their lives, as patriotism and humanity dissolved at a touch the hedges of custom and use. I know of not one such who did not find in the application of his vision and

training to the actual problems before him, first, a wholesome reminder of the limits of his knowledge and its clarity, and, second, new clues towards its advance, and that by no means only in a familiar or an expected direction. The stimulus of practical need here, as so often in experience, advanced the growth of knowledge beyond the point of immediate application to practice. Those who studied to find the best and most practical means of saving life threatened by severe hæmorrhage, or by the shock of wounds or operation, found in the course of meeting the immediate emergencies almost endless promptings to further inquiries, to be followed then or later—inquiries into the physical, chemical, or biological qualities of the blood, into its relations to the vessel walls, and into the functional changes of the capillary blood system and the factors affecting or controlling them. Those who fixed their attention upon the damage wrought in the respiratory organs by poison gases were led to many new studies of the fundamental physiology of the lungs. The lymphatic system of drainage of the lungs was re-examined, and wide new experimental studies of the modes of regulation of the breathing were undertaken which have thrown new and valuable light upon the normal mechanisms of respiration. An inquiry into the poisonous action of the high-explosive trinitrotoluene, and into the possibility that slightly abnormal forms of this substance, found as a small contamination of the normal form, might be specially toxic, led to a clear negative answer. But it led unexpectedly, it is both curious and useful to note, to the discovery that one of these abnormal forms was an effective reagent in the laboratory. By its means the chemical structure of a constituent of muscle substance known as carnosin was for the first time determined, and carnosin has now been synthesised artificially from simple materials.

In sum, then, we may gratefully recognise that the War in its horror and waste has not brought evil without any admixture at all of good. We may be encouraged at least to hope that the active co-operation which the War established and fostered in diverse ways between the physiologist and the medical or surgical clinician may remain to bring lasting good, on the one side to the cause of learning and its advance, and on the other to medical education and to medical progress.

III.

If we have thus looked backward to the development of physiology in the past half-century, and to the influence upon its course which the War has brought about, I would invite you to look forward to the future and to review the aims of physiology and the boundaries to which it should properly extend in its relations to other subjects of study.

Foster, early in his work at Cambridge, spoke of physiology as being the study of the differences between the living body and the dead body. The progress of this study, as it has been carried on during the past generation, may be considered from two directly opposite points of view. Viewed in one way, we may think of this progress as being a progress in analysis, as a disentanglement of the diverse though not separable functions of the body and of each of its parts. Viewed

again, we may see it as a steady progress towards synthesis, towards the unification of all the contributory functions of the parts into a single functional organism.

The analysis of the separate functions of each part of the body was an inevitable mental process as the anatomist revealed more and more accurately the visible machinery of the body. Bichat at the beginning of the nineteenth century had taught that the activities of the body must be the sum of the activities of the organs. The announcement of the universal cellular structure of the organs made by Schwann seemed but to carry this analysis one step further, and to show that in the sum of the activities of the constituent cells could be found the adequate expression of the functions of the whole body. The rapid improvement of the microscope in the latter half of last century, combined with the new resources of the aniline dyes by which transparent structures could be differentiated and made visible, greatly stimulated the analytic study of the body. As the various glandular structures were made visible and even, as it almost seemed, the inner life of the gland cell was revealed, as muscle fibres in their different kinds were made plain and the harder elements of the body resolved into the architectures due to different kinds of constructive cell, so it seemed to many that in a little time we should have the quest resolved in an appeal to a congeries of physico-chemical events within the individual cells. Even the mysteries of the central nervous system seemed to be dissolving as the new powers of histology, coupled with refined methods of experiment, showed the intricate pattern of communicating fibre and cell and gave provisional descriptive explanations of many isolated nervous phenomena. Meanwhile, the chemical structure, no less than the material form, of the body was being explored, and here, too, progress followed the path of analysis, ever more refined and complete. Just as old notions of ' humours ' of the body had been resolved into varieties of cell activity, so the vague chemical ideas conveyed in the words ' protoplasm ' or ' metabolism ' received precision by expression in terms of colloidal systems or of associated enzymes or catalysts in appropriate positions, effecting chemical changes of recognisable type among substances of relative simplicity.

Along these lines of analysis rapid progress has been made, but it is to be observed that it has been in great part along diverging lines. The tendency has been centrifugal, or, to use a biological simile, the growth of physiology has led to a fissiparous habit. Pursuit of knowledge by particular technical methods has led to specialism; men have reached points far distant along branches of inquiry that at first grew together from the stem. The very development of new technical methods may by itself lead unavoidably to separatism, for the microscope and test-tube may best be used in rooms widely different in equipment and often far separated in space. So have grown up new-named sciences within a science, and the histologist or cytologist, the neurologist, the pharmacologist, the biochemist—each carrying off, so to speak, his part of the subject—may be found to be incurring the dangers or even paying the penalties of schism.

Step by step, however, with this progress in analysis. a continual

advance towards synthesis has accompanied it as new truths have been unfolded to the investigator. Here, as in other fields, the conception that the whole is the same as the sum of its parts is either meaningless, or, if it have any meaning, is untrue. Fresh reinforcements have steadily come to the idea that the animal body is not to be rightly considered as a patchwork of the activities of its parts, but that the organism itself as a whole is the true physiological unit. In this conception the functions of the organs and of their own cellular subdivisions can only find due expression in relation to each other and to the functions of the whole. Just in proportion as analysis has proceeded with ever greater refinement to trace in terms of physics or chemistry the nature of given organic or cellular phenomena, the analysis itself is found to be pointing to new relationships between part and part of which the meaning is bound up in the unity of the organism.

Of this continued absorption of analytic data into synthetic conception, this interweaving of increasingly manifest diversities into an increasingly emergent unity, illustration can be found in many directions. The name 'hormone' has been given to chemical products of particular organs which pass by way of the blood to stimulate another organ or other organs of the body to changes in activity. This mode of chemical regulation by messenger, so to speak, is superadded to the more rapid method of regulation by nervous impulse through the nervous system: and already many beautiful examples of delicate interplay and co-ordination have been discovered between the two kinds of regulation. In its earlier phases the knowledge of these messengers gave us a picture of relatively simple, though wonderfully adjusted, acts of chemical regulation. As analysis of the hormonic exchanges of other glands and tissues of the body has proceeded, however, a system of interplay and reciprocal function of increasing complexity has been revealed by later studies. Our knowledge of this is still young and quite rudimentary, but at every fresh step in this advance it becomes more evident that the multiplying facts can only be resumed by a conception of the whole organism as a unit of which the parts exist to preserve the integrity and 'normality.'

In the study of the nervous system, again, new methods of observation and analysis have given us during the past half-century immense additions to our knowledge of the intricate fabrics of the brain and spinal cord and of the functions of the various systems of fibres and cells. The content of our knowledge of these must be tenfold that which was known fifty years ago. Here again, as investigation has gone forward, and as analysis has proceeded by methods so special and so refined that neurologists work, as it were, in a field of their own, it has proceeded only to reveal ever more and more clearly what Sherrington, one of the chief pioneers in this analysis, has himself called the 'integrative' action of the nervous system. The fabric of nerve cell and fibre, whether we trace its history from the lower to the higher animals, or whether we trace its complexity in the individual, is revealed to us as a series of superimposed controlling systems whose structural relations find intelligible expression only in terms of functions, and of functions of the animal as a whole.

Is the same return to synthetic conceptions to be found as a result of analyses of the biochemist? His work has brought much simplification to our notions of the chemistry of the body. We have learned that in the exchanges within the living cell we are not necessarily, or indeed probably, dealing with molecules of a complexity unknown outside the living body; we do not now think as formerly of substances being worked up through successive stages of elaboration into a living molecule—a molecule of 'protoplasm' of mystical complexity—or of other substances reappearing as the result of incessant degradation of parts of the living molecule. Analysis has shown already that many characteristic cell-changes turn upon relatively simple reactions of a kind familiar in chemistry between known and relatively simple substances. How much further will this analysis proceed? No doubt many of the typical functions of particular kinds of cell will become expressible in a set of chemical formulæ, and every simplification attained by the biochemist in terms of known chemical or physical law will be a notable gain. Yet even now we can feel assured that the analyses of the biochemist bring with them new emphasis upon the essential unity of the whole organism. Let me give but one illustration of this. In the studies of immunity from disease it had long been known that substances which to a chemist would appear to be identical could be sharply distinguished in the most decisive way by biological reactions. Tiny fragments of a small blood-clot can be made thus to declare whether they come from a man or from what other animal, when no chemist would have dreamed of finding a distinction. Dudley and Woodman have lately been able, however, to bring biochemistry within the range of this biological delicacy of discrimination, and have shown a subtle difference in the chemical architectures of the caseins derived respectively from the milk of a cow and of a sheep. More recently the two modes of analysis have been brought side by side. Similar cells in similar organs of the two not widely dissimilar birds, the hen and the duck, secrete layers of egg-albumin during the completion of that wonderful structure, the egg. From the 'white' of each egg can be prepared apparently identical albumins, and in a pure crystalline form. This albumin is built up in each case from simple materials—amino-acids—derived from the food, and we should naturally expect a close similarity between the two kinds of resulting albumin, that in the hen's egg and that in the duck's. The most refined methods of ordinary chemical examination show us, indeed, that the two are chemically identical and indistinguishable, containing on analysis the same amounts of the same varieties of amino-acids. But Dakin has lately succeeded in tracing a difference between the two albumins, exhibited only as partial differences in the order or pattern in which some of the constituent amino-acids are linked together in the structure of the albumin molecule. By using a physiological test, Dale, at the same time, has been able to show a decisive and even dramatic difference between the qualities of the two albumins so near to chemical identity. By using the 'anaphylactic' reaction of the organic tissue from an animal 'sensitised' against hen albumin, he has found that a suitable application of hen egg-albumin will produce a decisive response,

while an exactly similar dose of duck egg-albumin will produce no effect whatever; and so *vice versa*. Here, then, is some authentic stamp of unknown kind imposed uniformly upon the parts of the organism of a given species, even upon the molecules of the albuminous coating of its egg. We are brought sharply back from the relative simplicities of chemical analysis to consider this supra-chemical impress of specific pattern, a phenomenon which can have no meaning that is not drawn from a conception of the organism as a whole.

It would be impossible here, and quite unnecessary for the present purpose, to do more than refer finally to the beautiful researches of recent years upon the modes of regulation of breathing, upon the gas exchanges of the blood, and upon the associated activities of other organs, and especially of the kidney, which have brought such ample support and illustration to the doctrine first clearly taught by Claude Bernard, namely, that the different mechanisms of the body, various as they are, have their single object in 'preserving constant the conditions of life in the internal environment.' These regulative functions in particular have been fully discussed by Dr. Haldane in a recent notable essay, and he has shown how, as their chemical analysis has proceeded and observations have been collected by physiological methods, themselves of a delicacy often far exceding present physical and chemical methods, it has become more and more necessary to express the facts in terms of an organic unity. 'The physical and chemical picture is entirely obliterated by the picture of organism.' These considerations are full of interest, of course, in their relation to the rival mechanistic and vitalistic theories that have been advanced for the explanation of living processes. Here, however, I refer to this synthetic tendency of modern physiology because of its practical bearing upon the present development of the subject in the universities and the medical schools. As the preliminary analyses of the functions have been, as we have seen, centrifugal and fissiparous in their tendencies, so the accompanying and inevitable synthesis, resuming analytical data within the notion of organism, has been centripetal and conjugative. It is this bond of organic unity which must sooner or later serve to bring together the scattered workers in different fields of analysis. It is this conception of the organism, moreover, which must maintain physiology as a great primary branch of study—the study of the living organism.

If physiology remains as a free subject of university study, we need not have serious fear that the fissiparous, centrifugal tendencies already noticed will be dangerous or crippling. Ludwig organised his physiology teaching at Leipzig in 1846 under the three main divisions of histology, experimental work, and physiological chemistry. In the English revival that we have earlier sketched, this grouping, largely under the influence of Foster, was maintained not only at Cambridge, but at other centres here and in America. As years have passed, however, there has been an increasing tendency here to follow what is commonly done in other countries, and to place histology with anatomy. In my personal view, physiology cannot proceed without perpetual use of the microscope, and yet anatomy must be dead without histology. I should hope to see histology the well-worn bridge of union between the

two subjects, just as, I think, we should look to cytology and the study of cell development to offer active points of growing union between physiology and the sciences of animal and plant morphology. These and other questions of detailed organisation will, I hope, be explored fully in the discussion for which we are hoping to-day. With time also has come a great development of biochemistry, and this, if only from the structural necessities of its laboratory technique, is tending more and more to set up house for itself. This, too, is to be a matter for fuller discussion presently. We may perhaps hope to see in biochemistry as it grows not only a common meeting-ground and an unfailing source of new inspiration for physiologists and pathologists alike, but also a pathway by which organic chemistry may be led towards the study of living matter. Few organic chemists in this country, though more in America, have been led by that path till now, and yet we must believe that biochemistry has perhaps even more to give to organic chemistry, as we now know it, than it has to gain. A study of organic compounds in a spirit of detachment from the living processes which gave them birth must surely lead often to mere virtuosity in the laboratory transformations of chemical structure, and I venture very timidly to think that many signs point to the near approach of a time when organic chemistry will feel the need of fresh inspiration coming from the intricate laboratory of the living cell. In a university the separation of laboratories, which must be guided solely by convenience, as convenience is dictated by necessary differences in equipment and technique, may be easily transcended by the free communication of workers in different branches. Intellectual association and close co-operation, and especially within a university, seem inevitable, as we have seen, because of the converging approach of diverse workers in common reference to the conception of organic unity. There can be no boundaries to physiology narrower than the limits of the study of the whole organism and the balanced regulation of its living parts.

I would venture here, however, to point to some dangers by which the sound development of physiology seems to be threatened, that spring from its necessarily close association with medical education, dangers eminent in the present stage of rapid growth in medical studies both here and, even more obviously, in America. Historically, physiology may be said to have been born of medicine, but it has sanctions and strength quite independent of the great services it has rendered and has still to render to the material good of mankind through medicine, and, in a less, though in no insignificant degree, to agriculture. We may recall that chemistry, too, was almost equally born of medicine; medicine, at least, was the foster-mother and long the nurse of chemistry. Lyon Playfair, in his inaugural address of 1858, in this very place, said, nevertheless, that 'chemistry in her period of youth, full of bloom and promise, was forced into a premature and ill-assorted union with medicine.' We can now look back and see that chemistry, in becoming free of medicine, and in becoming a great independent branch of learning, has, by the fruits of that freedom, repaid to medicine a thousandfold her early debts of the nursery. So, too, the history of the last half-century, in which physiology has become an independent

subject of university study, shows how this freedom has multiplied the gifts which physiology has had it in her power to return to her ancient mother. There can be no dissolving of the ties between one and the other, but we must see to it that these ties are well adjusted and that there shall be no 'ill-assorted union' between the two.

In the rapid growth of medical schools throughout the English-speaking world there are present signs that the essential part which physiology plays in medical education and study may wrongly masquerade as the only service physiology has to give to man, and may appear to fill the measure of her rightful status. In more than one of the great American universities physiology is treated either in theory or in practice as a subject within the Medical Faculty to be housed within the Medical School, yet at the same time not as a subject in the Faculty of Arts or of Science, nor to be studied alone or with other sciences as part of a liberal and non-professional education. It is rare in the United States for physiology to be studied by any but professed medical students, and there is some reason to think that it is becoming rarer in Great Britain than it was a few years ago.

To my mind this tendency is to be deplored. It implies a reversal of that growth of physiology in freedom which began half a century ago and from which such good fruit has already been gathered. It has two chief evils among its inevitable results. Removed from its position among other university subjects by geographical separation that in some universities amounts to transportation and exile, it is deprived of the kinship and co-operation of the sciences touching its own boundaries—those of zoology, embryology, and botany, of agriculture, of psychology, of physics and chemistry. Assigned, if not limited, to a place in the medical curriculum, it is apt to be narrowed in its claims and outlook, and to lose not only its proper neighbours, but even parts of itself, whittled away in the organisation of a purely medical programme in the guise of pharmacology, neurology, toxicology, and the like, for which special funds may be available, separate places in the time-table reserved, and independent departments provided. But a second evil strikes more deeply. Any arrangements that give in effect a restriction of physiological studies to medical students alone must be doubly injurious. It is injurious to the general course of education, because it tends to cut away from the other university students the opportunity of possessing themselves, either as a primary or secondary study, of the knowledge and discipline of physiology which has educative value in the highest degree for the cultural or the practical sides of living. And here, secondarily, we may notice the loss to an applied study only less in importance to that of medicine; I mean the science and practice of agriculture. It is injurious, again, to physiology itself, because we know well from reiterated experience how many promising recruits for the future advancement of the subject have been brought to it, often, as it were, by chance, in the course of their university life, attracted to it whether from classical studies or mathematical, or from other branches of natural science. A notable number of the chief leaders in the science of the past and present generation have so been attracted, without any previous thought of medical studies as such

whether these have been added later or not; of these, not a few whose names are well known to us have never become, in the technical sense, students of medicine at all. They may have lost by this, but should we willingly have lost them?

I hope that what I have earlier said with regard to the great service that physiology has both to give to medicine and to receive from it will acquit me of any charge of desiring less, rather than much more, intimacy and intercourse between them. I believe that no better service can be done for the good of both than to increase their mutual offices and the ties between them. But we must see that, in uniting physiology to medicine, we do not uproot it from that soil in which alone it can abundantly flourish and bear fruit, the environment of a university with all that that connotes. If there be any serious doubt of the reality of the dangers I have indicated, I would point to the dearth of men fitted to promote and teach the subject among those coming from the schools in which physiology is regarded as a medical study and no more, and is not given its full university status. In the United States at present there is a grave and admitted dearth of suitable candidates for chairs of physiology, in spite of the remarkable work which has been done there in recent years and the fine material equipment in general available. I venture to offer my conviction that the prime cause of this shortage is the absence of the great recruiting possibilities of university life and the undue limitation of physiology to medical students. Men coming to physiology as a 'preliminary subject' and nothing more are not likely to think of it as their life-work, but will pass through it not to return.

Let me, in conclusion, point again to the highest of the tasks which physiology, like every other science, has to perform. Its highest and indeed its primary task is to enlarge the vision of man and to enrich his knowledge of truth. The secondary tasks of physiology, in finding power through truth, power to diminish pain and to restore health, and to guide to right and prosperous living, are happily so beneficent in kind, and already in some degree so fruitfully discharged, that it is not easy, or indeed common, to keep in mind that great and primary aim. Right thinking in this respect is the only constant guide to right action in all the practical questions which confront us now in our discussion of the position and the future of this science. 'Man does not live by bread alone': and we shall find—we have already abundantly found in experience—that it is only through the seeking of wisdom first that power to increase the comfort and convenience of life is most fully to be won. The practical services of inquiry have been easy for all to see. Men have come readily to think of physiology as the handmaid of medicine and as nothing more. Of late years we who follow the study of living things have not had interpreters to make plain to men at large the interest and beauty of the additions to revealed truth which have been coming from the work of the investigator. There are very few among the onlookers who have seen, or who can bring others to see, those clearer visions of the consummate beauty which are being revealed in the study of the body, visions as remote from the actual figments

daily painted for us by our sense organs as are the newest visions of the physical world, yet appealing as strongly to the intellectual and æsthetic emotions. Few hold the quest for natural knowledge in right relation to other activities of the mind; few see it not merely and not in chief as a useful pursuit of power, but in its essence as a pursuit of truth.

That knowledge of natural truth and of the changing pattern of our ideas of the natural world should be an unusual or quite subordinate part of a cultural equipment, in this and in recent generations, may be due to lack of interpreters, but it is due also to convention and educational habit, and these, perhaps, combine in special degree to shut out from the world of general culture the revelations of intricate beauty in the living body of man. Ancient and mistaken theological conceptions filtering through the Victorian age have tended to degrade the dignity and marvel of the body. Generations that have been nurtured upon narrowed classical studies have so far forgotten the spirit of Greece as to ignore the universal beauty of truth; it has been thought vulgar not to know the verbal details of an old mythology, but hardly respectable not to be ignorant of the elementary laws of life and of the unseen beauties of the body unfolded in modern study. So have many submitted to be enchained in ignorance and superstition as to vital matters of reality, victims of every passing charlatan. Out of this loss of instruction in the beauties and wonders of living substance, as they are becoming known, must come great loss of possible happiness, and indeed there comes, too, a loss of dignity, for we may fitly apply the rebuke of Robert Boyle, much more deserved now than in his darker century, who held it to be 'highly dishonourable for a Reasonable Soul to live in so Divinely built a Mansion as the Body she resides in, altogether unacquainted with the exquisite Structure of it.'

Meanwhile the workers will proceed in their quest for further truth, caring little if, for the time being, other eyes are blind to its beauty. They will still be lured by it as all eager minds have been lured before; some will confess the attraction of a call for help in human need and suffering, some will claim austerely that they follow only the bidding of a curiosity of mind, and some perhaps may work for fame. But, whether they know it or not, the effective lure that Nature holds out to those of her followers who have it within them to respond to it, and so to reach new knowledge, is a quickening hint of further beauty to be unfolded in further truth. Whether they know it or not, they might make the same Confession as that of St. Augustine: 'And I replied unto all those things which encompass the door of my flesh, "Ye have told me of my God, that ye are not He: tell me something of Him." And they cried, all with a great voice, "He made us." My questioning them was my mind's desire, and their Beauty was their answer.'

CONSCIOUSNESS AND THE UNCONSCIOUS.

ADDRESS TO SECTION J (PSYCHOLOGY) BY

C. LLOYD MORGAN, LL.D., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

Psychology has now been given full sectional status, taking effect at this meeting of the British Association. I trust that we shall justify the confidence reposed in us by our fellow-workers in other branches of science. I need hardly add that I deem it no mean honour to be chosen as your President on this occasion.

The subject of my address bristles with difficulties. I may at once state that my primary aim is to consider in what way mind and consciousness may be regarded as natural products of that all-embracing process which I propose to name 'emergent evolution,' and thus come within the purview of science as I understand its aim and methods.

Emergent Evolution.

What do I mean by emergent evolution? Shall we start from the platform of that which we call common-sense as tempered by the refinement of scientific thought? By general consent we live in a world in which there seems to be an orderly passage of events. That orderly passage of events, in so far as something new comes on to the scene of nature, is what I here mean by evolution. If nothing really new emerges—if there be only permutations of what was pre-existent (permutations predictable in advance by some Laplacian calculator)—then, so far, there is no evolution, though there may be progress through survival and spread on the one hand and elimination on the other. Under nature is to be included the plan, expressive of natural law, on which all events (including mental events) run their course.

From the point of view of a philosophy based on science our aim is to interpret the natural plan of evolution, and this is to be loyally accepted just as we find it. The most resolute modern attempt to interpret evolution from this point of view is that of Professor S. Alexander in his 'Space, Time, and Deity.' He starts from the world of common sense and science as it seems to be given for thought to interpret. In order to get at the very foundation of nature he bids us think out of it all that can possibly be excluded short of the utter annihilation of events. That gives us a world of ultimate or basal events in purely spatial and temporal relations. This he calls 'space-time,' inseparably hyphened throughout Nature. From this is evolved matter, with its primary and, at a later stage of development, its secondary qualities. Here new relations, other than those which are only spatio-temporal, supervene. Later in logical and historical sequence comes life, a new quality of certain systems of matter in

motion, involving or expressing new relations thus far not in being. Then within this organic matrix, already 'qualitied' (as he says) by life, there arises the quality of consciousness, the highest that we know. What may lie beyond this in Mr. Alexander's scheme may be learnt from his book.

This thumb-nail sketch can do slight justice to a theme worked out in elaborate detail on a large canvas. The treatment purports to formulate the whole natural plan of progressive evolution. From the bosom of space-time emerge the inorganic, the organic, the conscious, and, perchance, something beyond. And with this successive emergence of new qualities goes the progressive emergence of new orders and modes of relatedness. The plan of evolution shows successively higher and richer developments.

Such a doctrine, philosophical in range but scientific in spirit—to which, I may perhaps be allowed to say, I, too, have been led by a rather different route—I call emergent evolution.

The concept of emergence is dealt with by J. S. Mill, in his 'Logic,' under the consideration of 'heteropathic laws.' The word 'emergent,' as contrasted with 'resultant,' was suggested by G. H. Lewes in his 'Problems of Life and Mind.' When oxygen, having certain properties, combines with hydrogen having other properties, there is formed water, some of the properties of which are quite different. The weight of the compound is an additive *resultant*, and can be calculated before the event. Sundry other properties are constitutive *emergents*, which could not be predicted in advance of any existent example of combination. Of course, when we have learnt what happens in 'this' particular instance under 'these' circumstances, we can predict what will happen in 'that' like instance under similar circumstances. We have learnt something of the natural plan of evolution. We may also predict on the basis of analogy as we learn to grasp more adequately the natural order or plan of events. But could we predict what will happen prior to *any* given instance—i.e. prior to the development of this stage of the evolutionary plan? Could we predict life from the plane of the inorganic, or consciousness from the plane of life? In accordance with the principles of emergent evolution we could not do so. The Laplacian calculator is here out of court.

This is not the place to adduce the many facts at the inorganic stage of evolution, which, as I think, exemplify emergence (in this technical sense) with its hall-mark of something new, and its saltatory form of continuity—saltatory because there is often an apparent jump from one relatively stable product to another; continuous because there is no unfilled hiatus in the course of events. It is exemplified, as I think, in the modern story of the so-called chemical elements, in the very structure of the Mendeléeff table, in the systems of crystallography, and so on. In organic evolution it is recognised (though not under this name) by some biologists in the acceptance of mutations, in the outcome of much Mendelian research, and in the clue it affords to the origin of variations.

More to our present purpose, however, is its explicit recognition by Wundt in his advocacy of 'a principle of creative resultants' (Lewes

would have said ' emergents '), which ' attempts to state the fact that in all psychical combinations the product is not a mere sum of the separate elements that compose such combinations, but that it represents a new creation.' Clearly there is here emergence. But Wundt accepted the philosophy of what may be distinguished as ' creative evolution '—that which Professor Bergson in different form so brilliantly advocates. Wherein lies the difference? For M. Bergson the philosophical question is: What makes emergents emerge? Rightly or wrongly, I do not regard this question as one with which science, as such, is concerned; and in some passages at any rate this is the opinion of M. Bergson himself. Philosophy, he says, ought to follow and supplement science, ' in order to superpose on scientific truth a knowledge of another kind, which may be called metaphysical.' Be that as it may, his answer to the question: What makes emergents emerge? is Mind or Spirit as Vital Impulsion. (I use capital letters for concepts of this order.) Whereas, then, for Mr. Alexander mind as consciousness is an empirical quality emergent in nature at an assignable stage of evolution, for M. Bergson Mind, as Spirit, is the metempirical Source (I adopt Lewes's adjective) through the Agency of which emergent evolution has empirical being. For the one consciousness is a product of emergent evolution; for the other emergent evolution is the product of Spiritual Activity, which is sometimes spoken of as Consciousness. The methods of approach, the treatment, and the conclusions reached, are different. Although my present concern is with the former, this must not be taken to imply a denial of Spiritual Activity. Its discussion, however, belongs to a different universe of discourse.

In Mind.

To come to closer quarters with our sectional topic, what do we mean when we say that this or that is 'in mind'? In a well-known passage Berkeley distinguished that which is in mind ' by way of attribute ' from that which is in mind ' by way of idea.' Fully realising that this should be read in the light of Berkeley's adherence to the Creative concept, one may none the less claim for it validity on the empirical plane where mind is regarded as a product of emergent evolution. The former, therefore (i.e. what is present in mind by way of attribute), I shall speak of as *minding*, the latter as that which is mind*ed*. The former is a character constitutive of the mind—that in virtue of which it is a mind; the latter as objective *to* the mind or *for* the mind. That which is minded always implies minding; but it does not necessarily follow that minding implies something minded.

Let me name a few of the many cases in our own life where not only does the minded imply minding (which always holds good), but where minding implies something definitely minded (which often holds good). Perceiving implies something perceived; remembering, something remembered; imaging, something imaged; thinking, something thought of; believing, something believed; and so on through a long list. In each case what I may call, in general, the *-ing* has, as its correlative, a more or less definite *-ed*. Whether correlative to unconscious

minding, there is something more or less definite which is unconsciously minded, it is very difficult to say. But if I do not misinterpret current opinion it is commonly held that something minded is often present to, or for, the unconscious mind. I shall say somewhat more on this head later on.

The distinction based on that drawn by Berkeley may be expressed in another way. One may be said to be conscious *in* perceiving, remembering, and, at large, minding; that which is perceived, remembered, or minded is what one is conscious *of*. I am conscious *in* attending to the rhythm or the thought of a poem; I am conscious *of* that to which I so attend. I need not *then* be conscious *of* attending to the poem, though perhaps I may, in psychological mood, subsequently make the preceding process of attention an object of thought. I am well aware that Professor Strong has urged that, in its original use, the expression 'conscious of' was applied only with reference to mental process as such. One need not discuss this point. It must suffice to make clear the usage I accept.

Even in our own life there are cases in which one's consciousness *in* some experience—e.g. feeling fit or depressed—does not seem to have, correlative to it, anything definite of which one is conscious. It may, of course, be said that what one is here conscious of is some bodily condition, or some more abstract concept of welfare or the reverse. But, without denying that it may come to be so interpreted in reflective thought, it is questionable whether the dog or the little child knows enough about 'the body' or of 'welfare' to justify us in regarding these as objectively minded. There can be little question, however, that the dog or the child (and we, too, in naïve unreflective mood) may be conscious *in* such current episodes of daily life. Whether, therefore, there be something definitely minded or not, the emphasis is on minding (in a broad and comprehensive sense) as an inalienable attribute of that kind of being which we name 'mental.'

Mr. Alexander emphasises the distinction between what I have called the *-ing* and the *-ed* in the most drastic manner. He speaks of all that is in any way objective to minding as non-mental. I cannot follow his lead in this matter, because I need the word in what is for me (but not for him) a different sense. But what does he mean? It is pretty obvious that while seeing is a mental process in which I am conscious, the lamp that I see is not a mental process, but an object of which I am conscious. If, however, I picture the Corcovado beyond the waters of Rio Bay, is that mental? The picturing of a remembered scene is a mental process; but that which is thus pictured is not mental in the same sense. It is just as much re-presented for the remembering as the lamp is presented as an object for the seeing. And suppose I try to think of the four-dimensional space-time framework conceived by Minkowski; the thinking is unquestionably mental, but the framework thought of is not mental in the same sense. What is not mental in that sense Mr. Alexander calls 'non-mental.' I speak of that which is not mental in this sense as 'objective.'

A wider issue is here involved. Are we to include 'in mind' processes of minding only, or also that which is objectively given as

minded? Is the science of psychology concerned only with mental processes of the *-ing* order; or is it concerned also with all manner of objective *-eds*? One must choose. So long as we are careful to distinguish the *-ed* from the *-ing* it is better, I think, to include both.

Dependence and Correlation.

On these terms what is minded is no less mental than the process of minding. But I suggest that the word 'consciousness' should be reserved for that which Berkeley spoke of as 'in mind by way of attribute,' or, in Mr. Alexander's way of putting it, as 'a quality' of that organism which is conscious in minding. Anyhow, consciousness is here in the world. Creative Evolution says: Yes, here in the world, but not of the world. It acts (as *élan vital*) into or through the organism regarded as a physical system; but its Source is a disparate order of Being to which, in and for itself, and *an sich*, it properly belongs. It depends on the physical organism in act but not in Being. Now this, I urge, is a metempirical explanation of given facts, but not an empirical interpretation of them as (in my view) science tries to interpret. And its cause should be tried before a different court of appeal from that of science. Hence under emergent evolution one uses the word 'dependence' in another sense, and urges that the very being of consciousness, as a quality of the organism, depends upon (or implies the presence of) the quality of life as prior in the natural order of emergence. If we enumerate successive stages, then consciousness is a quality (4) of certain things (very complex and highly organised things) in this world. In these same things there is also present the quality of life (3), and a specially differentiated chemical constitution (2). Empirically we never find (4) without (3), nor (3) without (2); and we express this by saying that consciousness depends on (or implies the presence of) life; and that life depends on a specialised kind of chemical constitution. It is an irreversible order of dependence. But there are things, such as plants, in which we find (as is commonly held) life without consciousness; and other things, such as minerals, in which there is chemical constitution (not, of course 'the same' chemical constitution) without life. Furthermore, there seems to have been a time when consciousness had not yet been evolved; and an earlier time at which life had no existence. But this or that chemical constitution is itself an emergent quality (2) of certain things; and there was probably a yet earlier stage of evolution at which even this quality had not yet emerged—a purely physical stage (1) at which (let us say) electrons afforded the ultimate terms in relation within physical events, continuously changing under electromagnetic (and, of course, also under spatio-temporal) relations. That is as far as I, with my limited powers of speculative vision, can probe. Mr. Alexander, with perhaps more piercing insight, goes further. For him such entities as electrons are themselves emergent from the yet more fundamental matrix of space-time. For him the ultimate terms are point-instants (pure motions). I cannot here discuss his fascinating but rather elusive treatment. As at present advised I can find no satisfactory foothold without electrons, or something of the sort, as *points d'appui*.

Be that as it may, there is clearly nothing in the foregoing thesis which necessarily precludes the further consideration of the same events from the point of view of Creative Evolution. The questions: What makes emergents emerge? What directs the whole course of emergent evolution?—these questions and their like are *there* quite in place. Furthermore, as between emergent thesis and Creative antithesis, Kant's 'Solution of the Third Antinomy' may afford a guiding clue.

If one selects, as above, certain salient phases of evolutionary progress, and lays stress upon them, one must remember that within the span of each phase there are other emergent sub-phases, some of them, no doubt, worthy of selective emphasis. Nay more, it must be realised that one is only attempting to classify the myriad instances of emergence in an ascending hierarchy. In all phases, in all sub-phases, and in all the myriad instances, there is continuity of advance, in that (*a*) there is never any unfilled gap or hiatus in the course of events, and in that (*b*) any instance, sub-phase, or phase, arises out of, is founded on, and implies, that which lies just below it in the scale.

Here, however, an important question arises. The selected sequence of qualities is—

(4) Conscious.
(3) Vital.
(2) Chemical.
(1) Physical.

Are the four terms of this sequential order homogeneous? If so, the quality of consciousness in (4) is homogeneous with the purely physical quality under (1). But this is not in accordance with a cardinal tenet very widely accepted—namely, that the physical and the mental cannot be regarded as homogeneous. They are, it is urged, essentially heterogeneous. On the assumption (which I feel bound to accept) that this traditional view is right, how does emergent evolution deal with the problem? It further assumes (or accepts as an hypothesis to be tried out on its merits) that there obtains a correlation of diverse (and in that sense heterogeneous) aspects. The word 'correlation' is here used to designate a mode of natural 'gotogetherness' which is *sui generis*; and the word 'aspects' (for lack of a better) to designate the fundamental difference between the mental or psychical and the non-mental or physical—a difference that must be accepted as something given in nature. On this hypothesis, then, how do our emergent phases now run?

Let me recall that each of our four emergent stages gives emphasis to a salient phase of emergence, and that within each phase there are sub-phases also emergent through the supervenience of something really new. Within the vital quality, for example, there are ascending sub-qualities. It is for the physiologist to deal with these. There are, too, in any given organism different lines of advance closely inter-related within the life-system of that organism as a whole. We must select, then, that line of advance which serves to enable us to interpret psychical advance in terms of correlation. Here we may be content, so far as the physiological aspect is concerned, to label, say, three sub-phases (*a*), (*b*), and (*c*); where (*c*) represents such integration as is

established in the cortex of the brain in correlation with reflective conduct; (*b*) such intermediate level of integration as is acquired in the course of individual life; and (*a*) such integration as is prior to (*b*) and (*c*) and on which they depend. I seek only to give a provisional schema. Now, I assume that correlated with (*a*) there is an affective form of psychical existence which is not yet consciousness as I shall presently define it; that correlated with (*b*) is consciousness of the order of such perceptual cognition as we impute to many animals; and that correlated with (*c*) is reflective consciousness or judgment which implies conceptual thought, and is often spoken of as self-consciousness. We may label these (thus provisionally distinguished) (α), (β), and (γ). They stand, I believe, in an order of dependence. We never find (γ) without (β) and (α); nor ever (β) without (α). The presence of reflective consciousness implies perceptive cognition; and the presence of perceptive cognition implies that of affective enjoyment. We do, however, seemingly find organisms with (β) and (α) but without (γ); and (as I think) lowly forms to which one can impute (α) without (β). Our tabular statement may therefore take some such provisional form as this, which may at least serve to indicate my method of approach.

(4) Vital	(*c*) .	(γ) Reflective judgment	Consciousness	(iv)
	(*b*) .	(β) Perceptive cognition		
(3) Vital	(*a*) .	(α) Affective enjoyment	'The unconscious'	(iii)
(2) Chemical		?	?	(ii)
(1) Physical		?	?	(i)

On the left-hand side of the table we have 'outer aspect'; on the right 'inner aspect.' The 'inner aspect' (if such there be) under (ii) and (i) is left with a query. Panpsychic speculation is here and now beyond our horizon.

What I may call the *homogeneous* precursor of the quality of consciousness (iv) is 'unconscious enjoyment' (iii) which, notwithstanding its negative prefix, must be regarded as a positive character. The *heterogeneous* precursor of (iv, γ) is (4 b). Heterogeneous treatment involves passing over from one 'aspect' to the other—e.g. interpreting perceptive consciousness in terms of brain-physiology, or psychical habit in terms of synaptic resistance. I do not mean to suggest that heterogeneous treatment is without value. Far from it. I do wish to suggest that we shall do well to realise that it *is* heterogeneous.

The Quality of Consciousness.

Before proceeding further certain preliminary questions must be briefly considered. First, is there progressively emergent evolution in consciousness? It is one of cardinal importance. My contention is that such evolution obtains in both aspects, inner and outer, the one in correlation with the other. This means that interpretation under emergent evolution is applicable to mental no less than to non-mental events. In other words, there is just as much progressive emergence in the inner or psychical aspect of organic nature as there is in the outer

or physiological aspect. This is the keynote of mental evolution throughout its whole range.

I regret here to depart from the conclusion to which Mr. Alexander has been led. Take such episodes in our mental life as seeing a rainbow, hearing a musical chord, partaking of woodcock, dipping one's hands into cool water. In Mr. Alexander's interpretation (as I understand it) percipient consciousness, in each case, differs only in what he speaks of as 'direction.' That alone is enjoyed. All further difference in one's cognitive experience on these several occasions is due to the difference in that non-mental set of events with which one is then and there compresent. Even feeling, as affective, is not itself enjoyed. Feelings are objective experiences of the order of organic 'sensa.' They are not in mind by way of attribute. We are conscious of pleasure and pain but are not differentially conscious *in* receiving them. Consciousness is here just compresent with certain phases of life-process. Thus, for Mr. Alexander, consciousness, alike in sensory acquaintance, in perceptive cognition, and even in feeling pleasure or the reverse, is itself undifferentiated (save in 'direction'); all the differentiation is in the non-mental world (beyond us or within our bodies) which is experienced and which transmits its characters to a recipient in which the rather featureless quality of consciousness has emerged. No doubt for Mr. Alexander the recipient is not merely passive; for there is mental process—not Agency, though he so often uses the word 'act.' But this mental process just actively takes what is given; and all the difference still lies in that which is given and not in the enjoyment of how it is taken.

But it is only when Mr. Alexander is interpreting consciousness at the perceptive level that he advocates this doctrine. When he deals with values or 'tertiary qualities,' such as beauty, his treatment is quite different. Consciousness hitherto featureless gives to certain objects of judgment their characteristic features. How, then, does the interpretation here run? 'In our ordinary experience of colour,' he says, 'the colour is separate from the mind, and completely independent of it. In our experience of the colour's beauty there is indissoluble union with the mind.' The contention comes to this. Colour resides in the thing seen, with which the organism having the quality of consciousness may or may not be compresent. Whether it is so compresent or not makes no difference to the non-mental existence of the colour as such. On the other hand, beauty resides, not in the thing only and independently, but in 'the whole situation,' which we may bracket thus [coloured thing *in relation to* compresent organism with quality of consciousness]. 'In that relation the object has a character which it would not have except for that relation.' The doctrine of 'internal relations' is accepted where beauty is concerned, and rejected in respect of colour. In other words, if the beautiful thing be one term and the conscious organism the other term, each gets its character (*qua* beautiful but not *qua* coloured) from its relation to the other. I should say that this holds good for the colour of the object, no less than for its beauty. My chief concern, however, is not with what Mr. Alexander rejects but with that which he accepts.

He holds (1) that the beauty of an object 'is a character superadded to it from its relation to the mind in virtue of which it satisfies, or pleases after a certain fashion, or æsthetically.' Now this being pleased or satisfied is referable (within the situation) to the organism which has the quality of consciousness, i.e. in brief to the mind. So far at least it seems to be a differentiated feature in consciousness no longer merely recipient. Mr. Alexander tells us (2) that, within the relational situation, 'the beauty is attributed to the object.' He says that 'it is the paradox of beauty that its expressiveness belongs to [I should say is referred to] the beautiful thing itself, and yet would not be there except for the mind.' He accepts (3) 'value' as that which satisfies a need; and he would (I think) not reject the view that it is primarily a felt need for behaving or acting (socially he would add) in some manner in regard to, or with reference to, the object to which value is attributed. He accepts also (4), as precursors of true values, what he calls 'instinctive values,' which I should speak of as the utilities of organic behaviour (e.g. under Darwinian treatment). We thus have (i) a specific mode of being conscious; (ii) reference of this differentiated feature in consciousness to the object; and (iii) a recognition of the pragmatic value of tertiary characters as determined by social conduct. I urge that, *mutatis mutandis*, the same treatment applies to the secondary characters; and that such treatment does away with Mr. Alexander's rather drastic difference of interpretation on the perceptive and on the reflective plane. In the case of secondary characters, no less than in that of values, we have (i) specific modes of being conscious, (ii) reference of this differentiating feature in consciousness to the object, (iii) as founded on the utility of behaviour thereto. Finally, we have Mr. Alexander's general conclusion. 'Thus value,' he says, 'in the form of the tertiary qualities emerges not with consciousness or mind as such, which the animals also possess, but with reflective consciousness or judgment.'

This conclusion seems to indicate that just as the quality of consciousness marks a phase of emergent evolution, with something genuinely new supervenient to the quality of life, so too within this phase there are ascending sub-phases of emergence. In reflective consciousness (the iv, γ of my table) there is, in 'value,' something genuinely new, supervenient on the perceptive consciousness (iv, β) which affords its evolutionary precursor. In other words, just as consciousness has its status in the hierarchy of salient qualities, so too within consciousness there are reflective and perceptive sub-qualities.

It is, I think, clear that the question I have here raised is of importance sufficient to justify the space I have devoted to it. It comes to this: Are there differentiating features ('*qualia*' they may be called) in consciousness as such? Do they, under conscious reference to objects, make these mental objects other than they would be if relation to consciousness were absent? If so, is this outcome of conscious reference restricted to the 'tertiary characters,' or is it also applicable to the 'secondary characters'? My belief is that it has to be reckoned with throughout the whole range of mental evolution.

The Place of Consciousness.

A second preliminary question is this: Where does consciousness dwell and have its being? From the point of view of emergent evolution I take it that the answer is: Consciousness is correlated with certain physiological and physical events which have place in the organism—and there only.

One has to deal with the relations which obtain in nature at their appropriate levels of emergence; and I hold that the proper level of spatial (and temporal) relations is that of physical events. But since all higher emergent strata depend on this stratum, and would not be present in its absence, space-time relations are implied throughout the whole series. In further illustration of what I mean, the proper level of energy-relations is sub-vital. Vital events, in a system which is not only physico-chemical but has also the quality of life, no doubt *depend on* energy-changes at the physico-chemical level. But there is no specific 'vital energy' (still less 'psychic energy') *in the same sense of the word 'energy.'* The word is then used with a different connotation.

Our present concern, however, is with 'the place of consciousness' under some meaning to be attached to this expression. There is so much ambiguity in the question 'Where is it?' and in the answer 'It is there,' that a little must be said thereon.

Suppose that one is dealing with things in one's room. Each thing is interpretable as a group of events with physical substratum. May we say, then, that any such given event (spatially related, of course, to other such events) has place in the group or system of events which is the thing? I take it that in one valid sense 'it is there.' In this sense a multitude of events—chemical, vital, unconscious, and conscious—have place which is dependent on that of the physical events in the organism. In this sense they are included in that system of events which we call the organism. But the organism is included in a larger spatial system. Shall we, for our present purpose, which is rather biological and psychological than physical, call this larger whole the situation? Now, suppose that my dog leaves the mat, on which he has been lying, to bask in a patch of sunshine near the window. He alters his place in the room as situation; but the physical, chemical, and other events retain their place in him. Whither he goes, thither also go all these events. In one sense they are still there—wherever he may be. In another sense their place has changed. They were, a few minutes ago, on the mat; now, they are near the window.

And if we ask: Where does the dog behave? I take it that the natural answer is: In the environing situation. But the question may be taken to mean: Where do his muscles function? Then the natural answer would be: In his body wherever it may go under their functional action. Let us next ask: Where does the dog perceive? In one sense he perceives in the situation, which includes the thing seen and the dog that sees it. But in another sense the perception is in him. That is where the process of perceiving (or more strictly the physical events correlated therewith) may be said to occur and to have place.

We should distinguish, then, a 'there of place,' within the inclusive situation, and a 'there of place' within the thing included in that situation. But when the dog rose from the mat and walked to the window, the influence of the sunlit patch which took effect on his eyes was the basis (founded on prior behaviour) of reference to place near the window. That is where he saw it; it is towards that spot that his procedure was directed. Now, on this occasion, reference to place within the situation, and behaviour in reaching that place, were happily consonant. There was nothing of the nature of illusion. But when one of his predecessors in my household barked at his mirror-image, the place of reference for behaviour was not consonant with what sophisticated human folk call the 'real place' of that thing towards which he behaved. The conditions were abnormal; and 'place of reference' did not coincide with 'real place.' So, too, if you see a pike below the surface in a still pool, and try to shoot him with a saloon rifle, you will probably miss him (unless you have learnt the trick) because the place of reference for behaviour in aiming is not the place where he happens to be. Coincidence of place of reference with 'real,' or acknowledged, place can only be established through the outcome of behaviour, crude at first, intellectually guided at last. If this behaviour *works*, well and good in the realm of practice; but it may work admirably, and yet not stand the test of validity in the realm of interpretation which includes the problem of cognition. In any case we have to distinguish (*c*) the 'there of reference' from the 'there of place' (*a*) in the situation and (*b*) in the thing. The 'where' to which the colour of the ruby, and to which its beauty also, *is referred*, is unquestionably 'there' in the gem; the conditions for colour-perception are undoubtedly 'there' in the cognitive situation as a whole; but the chemical changes due to electro-magnetic influence are 'there' in the retino-cerebral system of the organism. And it is in correlation with these changes which run their course within the organism that the quality of consciousness is emergent.

Does it follow from what has been said that 'the place of consciousness' is in some differentiated part of the organism—say in the cortex of the brain? Again the question is ambiguous. In what sense has it place there? Certain focal events in what one may call the intra-organic situation have place there; and in that sense conscious enjoyment is the correlate of physiological changes that may there be localised. It is dependent on these changes, and in their absence would not be present or in being. But it is no less dependent on all that takes place in the intra-organic situation. Hence in the wider sense nothing less than the whole organism as a going concern is the seat of the enjoyment which is correlated with its total working as a vitally integrated system.

Consciousness as Objective.

It will now, I hope, be sufficiently clear that when I say that consciousness dwells and has its being in the organism, and there only, my meaning is that any given instance of the class of events we call conscious (in a comprehensive sense of the word) is correlated with certain vital and physical events which have place in that organism.

There is, however, a very different but still quite empirical view. It may be said that consciousness as such embraces all the objects to which there is conscious reference. In other words, on this view it pervades the whole situation. That is 'where it is.' Let us consider what is here meant. It rests on the interpretation of consciousness as a mode of connection under which objects in the whole situation are the terms related *inter se*. Hence Professor Woodbridge urges that objects are 'in consciousness' in the same sense as things are 'in space' or events are 'in time.' Just as the expression 'in space' or 'in time' conveniently condenses the longer and more cumbrous expression 'in that kind of inter-relatedness of things which is called spatial or temporal'; so too does 'in consciousness' condense the fuller expression 'in that kind of inter-relatedness of objects which we call conscious.' And just as there is a spatio-temporal continuum within which things have place; so too, according to Mr. Woodbridge, 'consciousness may be defined as a kind of continuum of objects.' We should, therefore, he says, deal with the relations of the objects in consciousness to one another 'in the same way as that in which we deal with the relations of things in space to one another.' It is, I think, clear that consciousness, on this view, is coextensive with what is sometimes called 'the field of consciousness'—that of which one is conscious in reference thereto. In other words, consciousness is nowise limited to the organism, but is a special kind of relatedness which pervades the whole conscious situation. In my phraseology following Berkeley, the field of consciousness is 'in mind by way of idea' but not 'in consciousness by way of attribute.' But we are here considering a different usage under a different terminology.

Professor Holt, whose avenue of approach, like that of Mr. Woodbridge, is primarily logical, and new-realistic, develops an interesting doctrine of 'neutral entities.' I cannot here parenthetically discuss this doctrine with its stress on the objective reality of universals independently of consciousness. We may, I think, for our present purpose, take his view to be that what we commonly call the environment of an organism is *au fond* constituted by those universalised neutral entities we name objects, and that it is these neutral objects which call forth in the organism its specific responses or its more highly organised behaviour. But not all the environment calls forth such response or behaviour. That part which does so on any given occasion is what Mr. Holt calls a 'cross-section.' It is, so to speak, the business part of the total environment—that part which counts for behaviour—and it is through behaviour that it is selected from the rest of the environment. Now this neutral cross-section, defined by responsive behaviour, 'coincides exactly with the list of objects of which we say that we are conscious.' Mr. Holt therefore, on the basis of this coincidence, feels himself free to call the environmental cross-section the 'psychic cross-section,' or 'consciousness' or 'mind,' within which the individual members are 'sensations,' 'perceptions,' 'ideas,' &c. It is clear, therefore, that for Mr. Holt, as for Mr. Woodbridge, consciousness is, or includes, all that part of the environment to which there is conscious reference; that it 'is extended both in space and time . . . being

actually such parts of the object as are perceived'; and that the cross-section we are said to perceive is coincident with that towards which we behave.

Obviously we have in Mr. Holt's doctrine one form of a behaviouristic interpretation of consciousness. This opens up an issue which cannot here be discussed. One can, however, briefly indicate what seems to be the essential question at issue. Let us provisionally grant that organic behaviour towards what we call an object is 'coincident' with conscious reference to that object; nay more, let us grant that in the absence of prior behaviour to it there would never be evolved such conscious reference to it. Even granting this, does it follow that this conscious reference is not only 'coincident with' behaviour but is nothing more than that behaviour? In accordance with the principles of emergent evolution it does not follow, and it is not so; the one is a function of the organism's life, dependent on, but emergently more than, its physico-chemical constitution, while the other is a function of that organism's consciousness, dependent on, but emergently more than, that organism's life. And just as life is a quality of the organism that behaves and is centred therein, so too is consciousness a quality (higher in order of emergence) of the organism which is conscious in perceiving and in behaving.

Modes of being Conscious.

We may revert, then, to the view that the quality of consciousness has place in the organism (or more strictly is correlated with physical and physiological events which have place there), but that conscious reference, like organic behaviour on the plane of life, is effluent from that organism which receives physical influence.

My position under genetic treatment is this: (*a*) Physical processes of many and varied kinds are, on occasion, influent on the organism which has receptors attuned to them; (*b*) very complex changes, physico-chemical and physiological, are called forth in that organism; and (*c*) it responds in organic behaviour, coming thus into new fields of physical influence. Thus, under light-radiation, influence from that which, in the language of our highly developed adult reference, we call a ladybird, affects the retinal receptors of the chick a few hours old; organised changes in his tissues result therefrom; he pecks at the ladybird, and tango- or chemo-receptors are thus physically influenced. So far the interpretation is biological and behaviouristic only. But, rightly or wrongly, I impute to the chick affective enjoyment on which some measure of conscious cognition is founded. *That* is neither physico-chemical nor physiological, though it is correlated with both. It is mental. It is the psychical or inner aspect of processes which have also their outer aspect with which the bio-chemist and the physiologist deal. But it just as truly belongs to that organism as does its life, or its chemical constitution.

We want now to come to closer quarters with this inner or psychical aspect correlated with the whole range of life-processes (*a*), (*b*), and (*c*). What is it? Perhaps all that one can say in reply to this question is that what it feels like that it is. But one can enumerate different ways

in which we enjoy this inner aspect—in which we are conscious in the most comprehensive sense of the word. Thus when one is seeing, hearing, tasting; when one is running, climbing, swimming; when one is imaging in reverie or in dream; when one is irritable, worried. or anxious; joyous or sad, in discomfort or at ease, fit and well, sick or sorry; when one is thinking or trying to recollect, following an argument, or solving a problem; accepting some statement in an attitude of belief, rejecting it, or poised in a state of doubt; when one chuckles over a joke, or winces under a bad pun; when one vibrates to music or shudders at the braying of a street band; nay even when, thereafter, 'silence like a poultice comes to heal the blows of sound'; in all these cases, and in a thousand others, we have instances of what it is to be conscious in the most comprehensive sense.

It will of course quite rightly and pertinently be asked: Who or what is thus conscious, now in this way and now in another? The empirical reply to this question (that to be given under emergent evolution) is: The integrated system of all the fluent conscious events that *are* thus integrated within that system. That is just what the mind is—an integrated system of consciously inter-related terms intrinsic to the organism and correlated with its life. No doubt a further question lies behind: What is it that gives to such a system the integration that it has? It is here that Creative Evolution offers an explanation in terms of Agency. In accepting the 'given' as that which we find in nature—and in leaving the question: What gives? to be discussed in the philosophical class-room—emergent evolution does but follow, as I think, the traditional procedure of science.

Consciousness and Enjoyment.

In speaking of a mind as an integrated system of conscious events the word 'conscious' is used in the broad and comprehensive sense that was almost universally accepted a generation ago. But in accordance with current usage we must now distinguish consciousness from the unconscious. I happen to regard the word 'unconscious' as peculiarly unfortunate—chosen as it is on the *lucus a non lucendo* principle. But let that pass. There it is and we must make the best of it—seeking to penetrate its dark wood. Under the older and more comprehensive use, consciousness may be indefinable. As in the case of spatial or of temporal relatedness we have got down to something that we find, rather than to something that can be strictly defined. Hence one has to proceed by indicating instances that fall within the inclusive class which we so name. The position is that, in the comprehensive class which we used to comprise under the heading of consciousness, it is now thought desirable to make two sub-classes—(*a*) the unconscious and (*b*) the conscious. There is call, therefore, for the indication of some criteria which shall serve to distinguish the one from the other. Here definition is required. And since the unconscious is 'served with the negative prefix,' it is clear that the criteria we seek must distinguish by their presence the conscious from the unconscious in which these criteria are absent. Under what heading, then, are we now to place the comprehensive class including both (*a*) and (*b*)? I suppose we

may call it the class of psychical events—as distinguished from physical and physiological events. But we still want some convenient noun which we may qualify by the adjectives 'conscious' and 'unconscious.' I borrow from Mr. Alexander, and adapt for my present purpose, the name 'enjoyment.' Perhaps the chief objection to the choice of this word is that it must be understood as including what is unpleasant no less than that which is pleasureable. But as I cannot find a better, and am loth to coin a worse, I ask leave to use this word 'enjoyment' to include all that has the psychical character or aspect. I regard the emphasis on affective tone which it suggests as a point in its favour.

On these terms there fall within the comprehensive class of enjoyment two sub-classes: (*a*) unconscious enjoyment and (*b*) conscious enjoyment—the latter marked by certain differentiating criteria. It may, however, be said (with some impatience): This division and subdivision into classes and sub-classes may be all very well in its way; but we ought to deal with concrete systems, not with abstract classes. So be it. Then, in this or that psychical system or mind, with concrete individuality, there is enjoyment which is (in some sense) unconscious, and there may also be enjoyment which is conscious (under some definition); and we want to distinguish in some way the one kind of enjoyment from the other. That puts the matter in more concrete form.

The question now arises: Is the distinction between the conscious and the unconscious just the same as that which is often drawn between 'above the threshold' and 'below the threshold' (supraliminal and subliminal)? Or, if they are not just the same, is there such close and intimate alliance that we may still say that all that is supraliminal is conscious and all that is subliminal is unconscious?

Let us first ask what we are to understand by supraliminal and by subliminal. I find this question exceedingly difficult to answer, save in rather vague and general terms. It involves a boundary line—the threshold—very hard to draw if one keeps within the sphere of what I have called homogeneous treatment. Is it a matter of intensity of psychical process, or of complexity, or of some combination of both? If so, can we, on purely psychical grounds, get a scale of one or other or both, so as to determine that zero-level at which what we call the threshold stands? It is difficult to do so.

May we say that the supraliminal is what we actually feel or experience, and that the subliminal is that the presence of which we infer? Then on what grounds is this inference based? Is it that we find, on occasion, that we have done something without any felt experience in doing it? If so, what evidence is there with regard to the nature of the psychical or inner aspect which *on that occasion* accompanied the doing? Or is it that the supraliminal experience is such as to lead us to infer that the subliminal modifies its felt nature? But if the difference is felt, as such, the subliminal so far enters into the supraliminal field so as to be felt indirectly if not directly. In that case the boundary seems hard to draw.

Shall we then resort to heterogeneous treatment? Shall we regard the psychically supraliminal as correlated with some assignable

character of physiological process, say in the cortex of the brain? It is sometimes said that 'when the brain-paths are worn smooth' the correlated psychical process becomes (or tends to become) subliminal. Without denying the partial validity of some such interpretation in correlating the physiology and psychology of habit, can one accept the general principle that at some stage of lessened synaptic resistance enjoyment is subliminal and at some stage of heightened synaptic resistance it is supraliminal? Is there not rich enjoyment (apparently well above the threshold) in the performance of well-established habit? And is there good evidence that (let us say) clear and vivid perception or swift and effective thought (which seems to thrill with supraliminal enjoyment) is proportional to physiological friction or synaptic resistance?

If the emphasis fall, not on the synaptic resistance overcome but on the establishment of a constellation of neuronic connections, and if it be urged that it is integration in progress which is correlated with what is psychically supraliminal, there may be much that is in favour of some such view. But does it follow that it is only when integration is in progress that enjoyment is supraliminal? There is surely much which is the outcome of well-established process that seems to be distinctly above the threshold. I am not satisfied that in our present state of knowledge heterogeneous treatment helps us very much to draw a definite line.

Reverting, then, to homogeneous treatment, it is often said that the subliminal (commonly regarded, I think, as a synonym for the unconscious) may best be defined as that which lies beyond the reach of introspection. But the introspection in terms of which the distinction is made stands in need of careful re-examination. Apart from behaviourist criticism, which has to be reckoned with, Mr. Alexander has raised the pertinent question: What is it that is reached by introspection? Is it the process of minding (e.g. attending or being interested), or is it that which is minded (what one is attending to, or interested in)? If the latter, he denies that it should be called introspection; it is a form of 'extrospection' in relation to what, for him, are non-mental (and, for me, objective) images or concepts—in mind by way of idea. And if the former, he denies that there *is* such introspection; for minding, though it is enjoyed, cannot, he says, be an object of contemplation or at the same time then and there minded. Now I have taken the word 'consciousness' as connoting mental process—i.e. that which is in mind by way of attribute. And I am disposed to agree with Mr. Alexander that mental process as such (and therefore consciousness as such) is not directly within the reach of introspection. I cannot follow this up. Indeed, my aim is only to show that if we are to define the supraliminal in terms of introspection we need a careful and up-to-date discussion of that in terms of which we so define it. To say that everyone knows what introspection is does not suffice.

Furthermore, those who have carefully considered the matter will probably regard introspection as possible only at the level of reflective thought. Presumably the cow has not reached that level. But if the

supraliminal is to be defined as that which is within the reach of introspection, can the cow have any supraliminal enjoyment if she have no introspection by means of which to reach it? Does comparative psychology endorse this current method of dealing with that very elusive limit, the threshold?

It must not be inferred from what I have said that the concept of threshold must be abandoned. It may be a difficult line to draw and yet be there as a boundary. We may still speak rather vaguely of supraliminal and subliminal. What I wish to suggest is that the line between them need not be coincident with that between conscious and unconscious. There are, I believe, modes of enjoyment both conscious and unconscious in the supraliminal field. But this reopens the main question: What are the differentiating criteria of the conscious?

Criteria of Consciousness.

Ask the plain man what he means when he speaks of acting consciously and he will probably reply: 'I mean doing this or that with some measure of intention and with some measure of attention to what is done or to its outcome. The emphasis may vary; but one, or other, or both, of these characterise action that I call conscious. If I offend a man unconsciously there is no intention to give offence. When a cyclist guides his machine unconsciously he no longer pays attention to the business of steering, avoiding stones in the road, and so forth.' Now if this correctly represents the plain man's view, it is clear that a full consideration of his attitude would involve careful discussion of intention and of attention. This is beyond my present scope. I want to dig farther down so as to get at what, as I think, underlies his meaning, and thus to put what I have to submit in a much more general form.

I want, if possible, to get down to what there is in the most primitive instances of consciousness—i.e. right down to that which characterises them as such. I believe that there is always in addition to that which is immediately given (say under direct stimulation in sense-awareness) some measure of revival with expectancy, begotten of previous behaviour in a substantially similar situation. Consciousness is always a matter of the subsequent occasion, and always presupposes a precedent occasion. In other words it is the outcome of repetition; and yet, paradoxically, when it comes it is something genuinely new. But this is the very hall-mark of emergence. That is why Mr. Alexander and I speak of consciousness as an emergent quality.

Let us analyse some simple first occasion—that on which a chick behaves to a ladybird will serve. The eye is stimulated from a distance with accompanying enjoyment (*a*). The chick responds by approaching and pecking with enjoyment in behaving (*b*). There follows contact stimulation with its enjoyment (*c*); and, thereon, behaviour of rejection with (*d*). We have thus (as I interpret) a biologically determined but orderly sequence affording successive modes of enjoyment *a*, *b*, *c*, *d*. So far the precedent occasion. On a subsequent occasion there is (*a*) as before in presentative form; this is immediately given in sensory acquaintance. But (*b*, *c*, *d*) are also 'in mind'—

mediately or in re-presentative guise, under revival, as what Professor Stout calls 'meaning.' We have therefore (under an analogy) on the precedent occasion the notes a, b, c, d, struck in sequence. We have on the subsequent occasion (b, c, d) rung up by (a) through a 'mechanism' (a bad word since the mechanical is superseded) provided psychically and neurally in the instrument. And when the notes (a, b, c, d) thus vibrate together they have the emergent quality of what one may speak of as the *chord* of consciousness.

What is there, however, about this emergent chord which differentiates it from the precedent sequence of notes a, b, c, d? It must be something psychical in its nature. I suggest that the revival carries with it a specific mode of new enjoyment which may be called 'againness'; that which affords the basis of felt recognition. There is also something equally new in expectancy. That this is (so far as our own experience testifies) a factor in the chord of consciousness is, I should suppose, scarcely open to question. I believe that it arises somewhat thus. On the precedent occasion the order of sequence was (c), *after* (a). On the subsequent occasion the *quale* in consciousness takes the form of what one may call the 'comingness' of (c) precedent to the 'comeness' which normally follows. But I cannot here follow up this clue.

Now whereas on the precedent occasion it is behaviour unconsciously directed towards that from which stimulation arrives that determines the order b, c, d as sequent on a; on the subsequent occasion it is the 'meaning' (b, c, d) which then consciously determines the direction of behaviour. This centering of 'meaning' on that to which behaviour was on the precedent occasion unconsciously directed is the basis of conscious reference to an object.

The characteristics, then, of a chord of consciousness are revival with expectancy and with conscious reference which anticipates, and, through anticipation (thus forestalling the event), may endorse, or inhibit, the further course of behaviour. And its emergent character, as chord, makes consciousness, not merely an additive blend of constituent tones of enjoyment, but (in Browning's forcible emphasis on a wholly new quality) 'a star.' (Cf. *Abt Vogler.*)

I have thus far dealt with the criteria of consciousness on the lines of what I conceive to be its evolutionary genesis. I must now ask whether these criteria—revival with expectancy and reference—do not characterise what we commonly regard as conscious enjoyment in our own adult life. My own experience is consonant with the outcome of genetic treatment. And I would ask others if there is not in our current consciousness always some measure of felt 'againness' carried over from the past in revival, and always some measure of 'comingness' in expectancy. I would ask whether there is not, as essential to consciousness, some leaning back on previous experience, some leaning forward to that which the future has in store. Is not this what M. Bergson means (I do not say all that he means) when he speaks of consciousness as 'a hyphen' linking past and future?

It need only be added that the conscious enjoyment in minding lies in the felt againness and comingness and referring—i.e. in the *-ing*

aspect. But there are in consciousness as above differentiated always correlative -*ed*-aspects in that which is revived, that which is expected, and that to which there is objective reference.

Stress on Integration.

I suppose that there may be pretty general agreement that, in dealing with mind, emphasis—perhaps the chief emphasis—should fall on integration. I use the word ' integration ' for that kind of systematic relatedness which obtains in an organism, and in a mind, where the functioning of sub-systems, as parts of the whole, depends on that of the system as a whole.

Let us here very briefly advert to that organic integration which characterises a system which has the emergent quality of life. We find a number of sub-systems—respiratory, circulatory, reproductive, and so on—within the comprehensive life-system of the organism. We find these functional activities interrelated in many very subtle and delicate ways in the life that is common to them all. We consider, for example, the integrative action of the nervous system, and of that which may now be called the ' hormonic ' system of internal secretions distributed by the blood-stream. The working of any one sub-system may facilitate or enhance the working of another; or it may partially arrest or even inhibit it. Abnormal functional activity of one sub-system may throw another sub-system out of gear; and so the trouble may spread. But the sub-systems are not historically prior to the life-system as a whole within which they play their parts; nor is the whole (*that* whole) prior to its sub-systematic constituents; whole and parts have been progressively evolved together with such closely related interplay as characterises the quality of life.

Now I assume, or accept as a provisional hypothesis, that unconscious enjoyment is correlated with life-process throughout its whole range. I know not where to draw the line between presence and absence. And how else can we interpret in homogeneous treatment, under emergence, psychical continuity in the race? In other words, wherever there is life there too, even in the germ cells, there is also, I assume, an accompaniment of enjoyment, psychical in its nature, at a level correlative with that of the current physiological process.

If this hypothesis be provisionally accepted in the spirit in which it is provisionally offered, what holds good for the life-system holds good also in principle and *mutatis mutandis* for the psychical system. But within that system there emerges the higher quality of consciousness (iv, β) characterised, let us say, by cognitive reference to the objective environment (to emphasise this criterion). Hence in the light of developing consciousness there is a progressive re-grouping in reference to the objects of which we are conscious, or objects in terms of which much of our unconscious enjoyment is re-interpreted. We say that dispositions, or interests, or innate tendencies, or emotional systems, or instincts, or impulses, are awakened to activity from a state of more or less unconscious slumber. (We are sure to use some rather metaphorical expressions.) These are then regarded as the sub-systems of the mind. Each has some measure of autonomous integration; all

are in some measure inter-related; and in a well-balanced mind, the net results of a bewildering number of psychical processes, many of them previously subliminal and unconscious, are caught up in subservience to conscious integration. But taken in detail there is much interplay between the psychical sub-systems as such, with facilitation, partial arrest, more or less inhibition, and perhaps derangement of function. There may be failure of normal integration within one systematic whole, or even such dislocation as we speak of as complete dissociation. And any of the psychical sub-systems—the so-called sexual complex for example—may be active in the subliminal region of the unconscious, or may rise into the supraliminal field and may modify the course of conscious events.

There is thus integration within the sub-systems severally, and integration of these sub-systems collectively so as to constitute a whole with (let us hope) due balance and poise. The unity of the whole is not that of simplicity but that of integrated complexity. In the degree in which the total integration fails to conduce to what we speak of as mental health and sanity we regard the poise as abnormal, and seek, by appropriate means (under the guidance of sympathy), (1) to ascertain to what sub-systematic conditions the lack of balance is due, and (2) to re-establish, if possible, the normal poise. It is here that psycho-therapy has done such valuable work in the practical application of psychological principles no longer restricted to the sphere of reflective consciousness only.

Levels of Psychical Integration.

In our normal life much integration proceeds on the reflective level—that of rational thought and volitional conduct. The older philosophers, with some variation of terminology, urged that the difference between this reflective level and the perceptive level below it (e.g. in Descartes' animal automatism) is one not only of degree but of kind. The difference, they said in effect, is radical and absolute, demanding metempirical explanation. Thus the word ‘kind’ carried a definitely metaphysical implication the influence of which is still with us to-day. But apart from this, as a matter of frankly empirical description of what is found, it was their way of expressing what I seek to express by saying that reflective consciousness has a new emergent quality—that which characterises reason as distinguished from perceptual intelligence. We have, however, the one word ‘consciousness’ for both these levels. But within the more comprehensive subclass, comprising all instances of consciousness, we may distinguish two sub-classes subordinate therein, (i) that of instances of reflective consciousness (iv, γ), and (ii) that of instances of non-reflective consciousness (iv, β). Both sets of instances have the criteria of consciousness. But in (i) there is a further *differentia* in that ‘value’ (in the technical sense) is referred to the object of such reflective thought. There is then, on this view, reflective integration, and there is also non-reflective or perceptive integration, each on its appropriate level, and each in its distinctive way conscious.

It is to the reflective level that all interpretation and explanation

properly belong. And it is here that there emerge the significant relations of conduct to value (truth, beauty, goodness) in conscious reference to objects of reflection. That, in us, much integration is established at this level of our conscious life cannot be questioned. But to say that all psychical integration is established at this level is itself an interpretation subject to truth-value; and one is pretty safe in roundly asserting that it is erroneous. Now, regarded from the point of view of emergent evolution, just as the quality of consciousness is dependent on, and supervenient to, the quality of life, so too is reflective consciousness dependent on, and supervenient to, the prior development of unreflective consciousness—in human folk in large measure begotten, through perceptive imitation, of the customs of our 'herd.' This unreflective process, as such, imitatively follows a lead which is itself the outcome of traditional habit no less unreflective. But reflective interpretation in due course supervenes when values come within the mental horizon; and it may be (alas often is) erroneous. And a leading type of false interpretation to which men are prone is seen in the tendency to trump up reflective motives in terms of value for actions determined by integration that is unreflective in character. As Huxley long ago put it, 'What we call rational grounds for our beliefs are often extremely irrational attempts to justify our instincts.'

How then do we stand? There is perceptive integration (consciously but not reflectively established) such as is the salient feature in the mental life of many animals. This passes up from its proper level to that of reflective consciousness, and is there re-integrated in the new significant field of value. Then, as reflective habitudes of valuing get firmly rooted, such re-integration spreads downwards to give value to more and more of that which has been established under the lower and earlier integration of the perceptive order. Behaviour is reorganised as conduct in terms of value.

This double process is noteworthy. When the emergent level of reflective consciousness is reached, the outcome of prior unreflective integration passes up from its lower level. But as re-integration at the upper level proceeds, more and more of the unreflective substratum undergoes reflective regrouping around the values which are the new centres of that higher re-integration. Unreflective integration ascends from below; reflective re-integration descends from above. But they are different; the new 'form' of integration is other than the old. There is always some 'conflict' which has been a fruitful theme in drama from the time of the Greeks onwards. And in our so-called normal life (to say nothing of that which is abnormal) this conflict of systems, with different centres of grouping and fields of influence, is daily and hourly in evidence.

Now carry the matter a stage lower. Unreflective integration of the perceptually intelligent order is consciously established in the course of individual life. The animal unreflectively learns to act in nice accordance with varying circumstances just as man learns also to act reflectively in relation to value. But is there not a yet deeper integration the products of which come up from below the perceptive level? Unquestionably there is. A generation ago it was regarded as purely

physiological; but that involves heterogeneous interpretation. If we accept the correlation of enjoyment with life we can regard it as unconscious integration. Its characteristic feature is that it is not consciously established in the course of individual or personal life. Its integrated 'form' is inherited and not acquired, though it may be swiftly re-integrated at the perceptive or (later) at the reflective level. As such and in its primary 'form,' as initially given, it is an ancestral bequest transmitted as a psychical legacy through the parents. Of it the individual is the unconscious heir.

Primarily dependent on the great life-functions, closely correlated with current physiological process in the organic sub-systems, finding expression in the co-ordinated, albeit unlearnt, behaviour adapted to racially recurrent life-situations, unconscious enjoyment, as psychical aspect, is, as M. Bergson says (but under a different interpretation), moulded to the very form of life—nay more, to every changing phase of the physiological balance and poise of the organism as such. Of this unconscious enjoyment much is, and may remain, the subliminal basis for a supraliminal superstructure at the levels of conscious integration. But *qua* unconscious it does not necessarily remain subliminal. In any of its 'forms' it may, on occasion, surge up into the supraliminal field with strongly affective tone, and thus afford new factors to be woven into the tapestry of the higher conscious integration. It still, however, even there, bears the mark of the unconscious in that it is new and unexpected with no feeling of againness just because, as fresh and new, there is no againness in the individual life to feel. And this insurgent factor, welling up from the unconscious, may, and often does, come into conflict with the outcome of perceptive or unreflective, and still more markedly with the outcome of our reflective re-integration. This more radical conflict is *au fond* that between what is racially established for the furtherance of life, as such, and what is socially established (far later in the evolutionary order) at the reflective level of that which we call (with emphasis on one of the values) our morality.

Having no space for further elaboration in detail, I must rest content with drawing attention to the following salient points. Although it originates in the unconscious and is there shaped to its integrated 'form,' the uprush from the deeper psychical strata founded on life-inheritance may glow and thrill with the affective tone which is the hall-mark of enjoyment and may take a very high place in the supraliminal field. Secondly, in that field it can only be distinguished (and that mainly inferentially) under analysis. It cannot ever be separated from the conscious factors which emergently combine with it in perceptive or in reflective re-integration. Thirdly, the distinguishing positive character of the innate factor which comes up from the unconscious (if we can catch it prior to further combination) is that it is new to individual experience. As new, there is no revival, no feeling of againness, no expectancy of what will next come based on the experience of what has come on like occasions; for there have been no like occasions in the course of individual life. And it gets all its reference to objects through its alliance with the conscious.

It seems to me therefore imperative to distinguish in that which is present in the supraliminal field according to the mode of origin of the integration that obtains. We must ask: How far is the 'form' which it assumes (iii) the outcome of reflective integration; (ii) the outcome of unreflective or perceptive integration; and (i) the outcome of the integration in the subliminal unconscious to which as living beings we are heirs? If I am right in regarding (ii) and (iii) as successively emergent qualities of consciousness there is somewhat of a leap (though no breach of continuity) from (i) to (ii), and from (ii) to (iii). There is always something more (involving new terms in new relations) in the higher-level conclusion than is contained in the lower-level premisses. This is the cardinal principle of all emergent evolution. Without this there would be nothing really new—merely a reshuffling of the old.

Revert now to ascending and descending integration. Under what may be spoken of as degradation—going down a step with habitude and habit—well-established reflective integration may assume the status of unreflective integration, and well-established unreflective integration that of the unconscious. The illustrative facts are familiar enough. It appears that the physiological correlates of this descent or degradation from higher to lower levels may be interpreted in terms of neural loop-lines and lower-level short-cuts due to lessened synaptic resistance in subordinate centres. If this be so, it is strictly accordant with the dependence of consciousness on life that psychical degradation should accompany physiological automatisation. The one is the correlated inner aspect of processes with which the physiologist has to deal.

Are there Unconscious Images and Ideas?

In the interpretation to which I have been led unconscious enjoyment (not necessarily involving unconscious images and ideas) is no less integrated than is the system of physiological events which gives to life its emergent quality. If the analogy be permitted, just as in the physiological symphony of life there are chords and phrases and motifs, each with an emergent character of its own (e.g. the part played by the instruments of the reproductive sub-system), so too, in the psychical symphony of unconscious enjoyment there are correlated chords, phrases, and motifs. And all goes well so long as due balance and harmony is maintained in the orchestral performance, no matter what instruments play a dominant part at the time being. But unconscious enjoyment is primarily inherited psychical music correlated with the outcome of life-inheritance. I entertain little doubt that the life of animals, could we only feel its inner aspect as they themselves do, is brim-full of a rich music of unconscious enjoyment. As I write the swifts are wheeling and shrilling in the summer air. Am I wholly wrong in imputing to them an integrated form of enjoyment which is theirs on a basis of inheritance? Perhaps even sympathetic naturalists fail adequately to realise to what extent in animals the business of life as such, with further life as its wage, has also its psychical reward in enjoying so fully the performance of life's job. And this reward in the enjoyment of doing is inherited with the ability to do. A

behaviourist interpretation of how it all comes about is, I believe, perfectly sound in its way. Not in what it emphasises, but in what (among extremists) it ignores—a psychical factor—does it seem to me to be deficient. In us at any rate the presence of enjoyment is undeniable. And though it is so readily caught up into consciousness it still carries, I think, the marks of its unconscious origin. What does the poet or the artist tell us? Does he not claim that what springs up within him—if it be in truth (he may add) in any valid sense *his*—is quite inexplicable on what he regards as psychological principles? And if psychological principles deal only with conscious integration he is right. His poetry, or his art, is not in its essential nature the outcome of perceptive or reflective integration. Its well-springs lie deeper than that in the unconscious. He rightly affirms that the real thing in all true art is beyond his conscious control, though the means by which it is expressed must be learnt and may be bettered by taking thought. This is enshrined in the proverb: *Poeta nascitur, non fit.* And even of those who can only appreciate his work, may it not be said, with a touch of paradox, that enjoyment in art becomes reflectively conscious in criticism. This need not mean that the critic enjoys poetry any the less for the combination in higher integration of unconscious and conscious enjoyment. What it does mean is that the glad newness and glory of surprise lies in the poetry and not in the criticism. Once again it must be said that it is the fresh unexpectedness that is still the hall-mark of the unconscious.

And here a question arises which I find it difficult to put in readily intelligible form. Is the rich enjoyment which gets human expression in the poet—but gets expression also in the Black-cap, consummate master of song—is this enjoyment dependent on that expression, or is the expression dependent on unconsciously integrated enjoyment? Which is prior to the other in order of dependence? What, you may ask, am I driving at in propounding so subtle a conundrum? Well, I take it that the Black-cap sings, under the conspiring influence of the situation and environing conditions, because it is part of his inborn nature so to sing under these circumstances. His song is primarily the outcome of the unconscious poise of a psychical system, correlated no doubt with a physiological poise. In that sense surely the expression in song depends on unconscious enjoyment—or, if it be preferred, the behaviour in song depends on the integrated life-process with which unconscious enjoyment is correlated. Whether we say that the behaviour-expression (with *its* accompanying enjoyment) is dependent on impulse, or disposition, or instinct, or emotional state, what we mean is that if the latter be absent the former will not come into being. If I may so put it, unconscious enjoyment, affectively integrated, becomes clothed in the expression, with *its* enjoyment, and is consciously integrated therewith on the higher perceptive level. And what of the poet? I think that he too may tell us that unconscious integration of the emotional order precedes the imagery in which it is expressed—that, as he may put it, 'the poetic inspiration strives to find expression'—that the clothing in imagery depends on the prior affective integration, as yet unconscious.

This leads on to the broader question. Does that which we call the unconscious depend on the presence of images and ideas; or are images and ideas the cognitive raiment which the unconscious puts on at the emergent levels of perceptive and reflective consciousness? The question in brief really comes to this: Are there what we may comprehensively speak of as memories in the unconscious? In much present-day resuscitation of Herbartian notions (which some of us thought were little better than picturesque mythology long ago discarded as obsolete) the unconscious is peopled with such memories—with images, ideas, wishes, and thoughts, living together, as Professor James Ward puts it, 'like shades on the banks of the Styx.' Is this so? It is against this sort of thing that the behaviourist rises in vigorous protest; and, swinging his pendulum too far (in some cases), drops psychology overboard and proceeds on his course in the biological ship. For those who cannot go to this extreme the alternative view is that memories have being only in supraliminal consciousness and that the unconscious, as such, is no wise imaginal. It is not yet cognitive. Only through cognition at the higher level of unreflective or perceptive consciousness does it begin to put on the raiment of images, ideas, and the rest, and thus find expression in the supraliminal field.

On this alternative view not only are there no inherited memories in any form or guise, but there are no memory-images in existence save as correlative to an existent process of conscious remembering. One opens up here the whole problem of retention. What is retained—the blossoms of imagery, or the conditions under which they will in due season appear? The plant does not retain flowers; but its abiding nature is such that flowers are put forth under the influence of external conditions at a recurring stage of constitutional life-balance. This analogy may be rejected. If so the grounds of rejection should be clearly set forth. Is it on the ground that lilac-blossoms are *not* stored but that my memory-image of those I saw last spring *is* stored? One may then ask whether there is any better scientific evidence for the latter than for the former. M. Bergson is unwearied in his reiteration of the absurdity of supposing that images are stored in the brain. But M. Bergson contends that memory-images *are* stored in the 'obscure depths' of a realm of being quite disparate from that of the brain. All that one has ever experienced is thus retained. 'I believe,' says M. Bergson, that 'our past life is there, preserved even in the minutest detail; nothing is forgotten; all that we have perceived, thought, willed, from the first awakening of our consciousness, persists indefinitely. But the memories which are preserved in these obscure depths are for us in the state of invisible phantoms.' If this is to be accepted as 'scientific truth' the man of science may reasonably ask for such evidence as he is accustomed to demand in other branches of scientific inquiry. And if it is part of the metaphysics which we are 'to superpose upon scientific truth' this should be more clearly stated than some at least of M. Bergson's disciples are wont to state it. At all events the status of images, ideas, wishes, and thoughts in the unconscious—nay deeper than that whether *as such* they are there

existent at all—is perhaps the most important of the fundamental questions which psychology has just now to answer.

And the answer must be sought, not only by those psychologists who have wide training and all-round experience, but in the full light of science as a whole. Part of my aim has been to lay stress on the solidarity of scientific inquiry. The psychologist must not work independently of the physiologist and the biologist, nor they independently of the chemist and the physicist. No member of the brotherhood of science may ignore or contravene what has been established in other fields of research. Though there is more at any higher level of emergent evolution than there is in the lower, the more is never divorced from the less on which it is founded. At the higher stage new modes of relation may obtain; but they are nowise discrepant with those which still obtain in the lower. And we must never interpret the lower in terms which belong to a higher emergent stage. That is false method in science. It is perhaps the cardinal principle of explanation in metaphysics; but in science it must be unreservedly condemned.

We here touch the quick of the world-problem under the interpretation of science and explanation by metaphysics. Emergent evolution works upwards from materiality through life to consciousness which attains in man its highest reflective level. It accepts the 'more' at each stage as that which is given, and accepts it to the full and ungrudgingly. It urges that the 'more' of any given stage is dependent on, or implies, the 'less' of the stages which are prior to it both logically and historically. It does not interpret the higher in terms of the lower; for that would imply denial of the emergence of those new modes of natural relatedness which characterise the higher and make it what it is. Nor does it explain the lower in terms of the higher. It leaves that kind of explanation to metaphysics. If physical changes are explained in terms of life; if physiological changes are explained in terms of unreflective or perceptive cognition, or this is explained in terms of the reflective consciousness which is emergent in philosophical thought; if all that we know is explained as the expression of yet higher and more completely integrated Mind or Knowledge—*that* is, I believe, the distinguishing mark of metaphysical as contrasted with scientific method. I do not deny its validity within its proper sphere. I do question its validity and its utility in science. But to distinguish is not to separate. It may well be that the methods are not antagonistic but complementary. None the less I seek to bring out as clearly as I can the position as I see it. Interpretation of the higher as founded on the lower (but fuller and richer in the advance of nature) is, I conceive, in accordance with the method of science; explanation of the lower in terms of that which is given only at a higher (and eventually the highest) stage—valid as it may be in metaphysics—must unreservedly be condemned in science.

In dealing with a very difficult problem, in trying to dig down to foundations, in seeking to link up psychology with other branches of science under one consistent scheme of natural development, I have doubtless said many things which call for disagreement and

protest. Many perhaps will not accept the distinction I draw between what I regard as empirical and what I regard as metempirical treatment. I have, however, only dwelt upon it so far as seemed to be necessary to indicate my concept of what science is and what it should seek to do. And though, on this occasion when men of science are gathered together, I hold a brief for the science in whose name we meet, it has been no part of my aim to disparage metaphysical explanation within its proper sphere. I may perhaps be allowed to say that, on a different platform, I should be prepared to defend, to the best of my ability, the Creative concept as nowise antagonistic to that of emergent evolution. I should then ask with Kant: 'May it not be that while every phenomenal effect must be connected with its cause in accordance with empirical causation, this empirical causation, without the least rupture of its connection with natural causes, is itself an effect of a Causality that is not empirical but [as Kant puts it] intelligible?'

THE PRESENT POSITION OF THE THEORY OF DESCENT, IN RELATION TO THE EARLY HISTORY OF PLANTS.

ADDRESS TO SECTION K (BOTANY) BY

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PRESIDENT OF THE SECTION.

It has long been evident that all those ideas of evolution in which the older generation of naturalists grew up have been disturbed, or, indeed, transformed, since the re-discovery of Mendel's work and the consequent development of the new science of Genetics. Not only is the 'omnipotence of Natural Selection' gravely impugned, but variation itself, the foundation on which the Darwinian theory seemed to rest so securely, is now in question.

The small variations, on which the Natural Selectionist relied so much, have proved, for the most part, to be merely fluctuations, oscillating about a mean, and therefore incapable of giving rise to permanent new types. The well-established varieties of the Darwinian, such as the countless forms of *Erophila verna*, are now interpreted as elementary species, no less stable than Linnean species, and of equally unknown origin. The mutations of De Vries, though still accepted at their face value by some biologists, are suspected by others of being nothing more than Mendelian segregates, the product of previous crossings; opinion on this subject is in a state of flux. In fact, it is clear that we know astonishingly little about variation.

My friend Dr. Lotsy, indeed, proposes to dispense with variation altogether, and to find the true origin of species in Mendelian segregation; inheritable variability, he believes, does not exist; new species, on his bold hypothesis, arise by crossing, and so, as he points out, we may have an evolution, though species remain constant. Thus everything apparently new depends on a re-combination of factors already present in the parents. 'The cause of evolution lies in the interaction of two gametes of different constitution.'

I am aware that very surprising results have been obtained by crossing. Nothing could well have been more striking than the series of *Antirrhinum* segregates which Dr. Lotsy showed us some years ago at a meeting of the Linnean Society. And now we hear of an apetalous *Lychnis* produced by the crossing of normally petaloid races. We do not know yet to what extent that sort of thing goes on in Nature, or what chance such segregates have of surviving. Still, if one may judge by Dr. Lotsy's experimental results, ample material for Natural Selection to work on might be provided in this way.[1]

[1] See Dr. Lotsy's book, *Evolution by Means of Hybridisation*, The Hague, 1916.

Dr. Lotsy's theory that new species originate by Mendelian segregation, if true, would have the advantage that it would make quite plain the meaning of sexual reproduction. Hitherto there has been a good deal of doubt; some authorities have held that sexual reproduction stimulated, others that it checked variation. But, if we eliminate variation, and rely solely on the products of crossing, we get a clear view—'species, as well as indivduals, have two parents'; sexual reproduction can alone provide adequate material for new forms, and can provide it in unbounded variety.

Again, though Dr. Lotsy himself is far from sanguine on this point, the crossing theory might be helpful to the evolutionary morphologist, for breeding is open to unlimited experiment, and we might hope to learn what kinds of change in organisms are to be expected. For example, the *Lychnis* experiment shows how easily a petaloid race may become apetalous. Such results might ultimately be a great help in unravelling the course of evolution in the past. We should gain an idea of the transformations which might actually have taken place, excluding those which were out of the question. At present all speculation on the nature of past changes is in the air, for variation itself is only an hypothesis, and we have to decide, quite arbitrarily, what kind of variations we think may probably have occurred in the course of descent. One need only recall the various theories of the origin of the seed from the megasporangium to realise how arbitrary such speculations are.

But, while recognising certain advantages in the theory of the origin of species by crossing, it is not for me to pronounce any opinion as to its truth. It is only the present position of the question that concerns us to-day. We shall hope to hear a statement of Dr. Lotsy's views from his own lips.

Some modern geneticists believe that there is evidence for mutation by the loss of factors, apart from the effects of crossing. Dr. Lotsy considers that such changes, if proved, can afford no explanation of progressive evolution. 'Evolution by a process of repeated losses is inconceivable.' It has, however, been pointed out by Dr. Agnes Arber, in her recent admirable book on Water-plants, that, on any theory of evolution, 'what organisms have gained in specialisation they have lost in plasticity.' She avails herself of a human analogy and says: 'The man, though superior to the baby in actual achievement, is inferior to it in the qualities which may be summed up in the word "promise," just as the Angiosperm, though its degree of differentiation so greatly exceeds that of the primordial protoplasmic speck, is inferior to it when judged by its power to produce descendants of widely varying types' (p. 335).

This is true, but it is not clear that this admitted loss of potentialities is the same thing as the loss of factors, in the sense of genetics. For example, if a glabrous variety of Violet really arose as a mutation by loss of the factor for hairiness, assuming that such a loss was permanent, the effect would seem to be a diminution of specialisation, though, no doubt, it might also be interpreted as a loss of potentiality.

Turning for a moment to Darwin's own theory of the origin of

species by means of Natural Selection, the efficacy of the latter, in weeding out the unfit, is, of course, still acknowledged, and some geneticists allow it a considerable rôle. But there is a strong tendency in these days to admit Natural Selection only as a 'merely negative force,' and as such it has even been dismissed as a 'truism.' Now Darwin's great book was most certainly not written to enunciate a truism. He regarded Natural Selection as 'the most important, but not the exclusive, means of modification' ('Origin of Species,' p. 4). It was the continual selection of the more fit, the 'preservation of favoured races,' on which he relied, and not the mere obvious elimination of the unfit, and this great idea (so imperfectly understood by many of his contemporaries and successors) he worked out with astonishing power, in the light of the changes which man has produced, with the help of his own artificial selection.

It may be that the theory of Natural Selection, as Darwin and Wallace understood it, may some day come into its own again; certainly it illuminated, as no other theory has yet done, the great subject of adaptation, which to some of us is, and remains, the chief interest of Biology. But in our present total ignorance of variation and doubt as to other means of change, we can form no clear idea of the material on which Selection has had to work, and we must let the question rest.

For the moment, at all events, the Darwinian period is past; we can no longer enjoy the comfortable assurance, which once satisfied so many of us, that the main problem had been solved—all is again in the melting-pot. By now, in fact, a new generation has grown up that knows not Darwin.

Yet Evolution remains—we cannot get away from it, even if we only hold it as an act of faith, for there is no alternative, and, after all, the evidence of Palæontology is unshaken. I have thought it fair to lay stress on the present state of uncertainty in all that concerns the origin of species. On another occasion I even ventured to speak of the return of 'pre-Darwinian chaos.' But out of this chaos doubtless light will come.

Last year we had a joint discussion on Genetics and Palæontology; among many good speeches, I specially remember a remark by Miss Saunders, our then President, that Mendelism is a theory of heredity, not of evolution—a caution not unneeded, though, as the crossing hypothesis shows, the connection between the two conceptions may prove to be a very close one.

Genetics is rendering the greatest service to Biology generally in ensuring that organisms shall be thought of as races, not as isolated individuals, mere chemical and physical complexes, at the mercy of the environment. The whole tendency of modern work is to show that in living things Heredity is supreme. An organism is what it is by virtue of the constitution of the germ-plasm derived from its parents. As Dr. Church has said in one of his recent Botanical Memoirs: 'The individual is no longer to be regarded as an isolated unit, or a casual creation, but is the present representative of a "race." That is to say, the individual is not, as short-sighted chemical physiologists tend to believe, a mere physical mechanism, the creature of the external

environment to which it passively responds; but it is the living presentation of a continuous line of organism, successful since living, or a "race" leading back as the expression of continued response to very similar, but not necessarily identical, environment, in unbroken plasmatic continuity, over a period of time which, in terms of ultimate cytological history, may represent a continuous reaction and record for anything up to such an inconceivable period as two thousand million years.' This expresses the case vigorously, whether we accept the time estimate or not. Dr. Church goes on to say that 'during this period the more fundamental reactions, as expressed in morphological units of construction, have been established as constants beyond any hope of change.'[2] This last statement is an important one for the palæontologist, for all our attempts to trace descent rest on the assumption that, in a general sense and as regards certain well-established characters, 'Like breeds like.'

History, then, broadly speaking, is everything. But there is more than one kind of history in Biology. First, we have the exact records of the Mendelian from generation to generation, F^1, F^2, and so on; this alone is adequate, but we usually have to be content with something much less. At the other end of the scale there is the fossil history, full of gaps and uncertainties of every kind, but always imposing from its vast duration. Then there are intermediate kinds of biological history, such as the imperfect records of the breeding of cultivated plants or domestic animals. These can sometimes now be interpreted in the light of the more exact genetic histories, as Dr. Lotsy will show us in the case of some neglected and misinterpreted observations of Darwin. 'Domestication,' as he says, 'spells segregation, followed by selection and isolation of the desirable segregates.' Darwin himself, though necessarily groping in the dark where genetics were involved, yet thought the study of cultivated and domestic races the best clue to the origin of species. If this holds good still, it makes a strong point in favour of the crossing theory of evolution, for the history of cultivated races seems to be largely the history of deliberate or unconscious Mendelian crossings. We may reasonably expect to find a relation between the process of origination of new cultural races and that of new species in Nature.

This suggests the question, what we mean by a 'species'—far too difficult a matter to discuss now. Whatever we may think of Darwin's theory, his 'Origin of Species' is at any rate a classic, and I believe we cannot do better than continue to use the word in the same sense as Darwin used it—*i.e.* essentially in the sense of a Linnean species.

Perhaps the best answer to the question 'What is a species?' is in the form '*Ranunculus repens*,' avoiding all attempts at definition. I know Dr. Lotsy thinks differently, but pure races, whatever else and however important they may be, 'are but rarely or never met with in Nature' (Lotsy), and are certainly not *species* in the classical sense in which Darwin used the word; to my mind it seems a pity to go out of our way to change completely the meaning of a familiar term. We

[2] *Form Factors in Coniferæ*, Oxford, 1920, p. 22.

can continue to call 'pure races' by that name or any more modern equivalent, and 'elementary species' may still be called so, or I have no objection to calling them 'Jordanons.' In the interests of practical taxonomy they necessarily have to be kept subordinated to Linnean species. There are difficulties enough either way, but they are, as it seems, less if we adopt the conservative course. That many Linnean species are real units of a definite order is generally admitted. Dr. Lotsy himself dwells on their distinctness, which depends on their usually not inter-crossing, and appears to be shown by the fact that among animals members of the same species recognise each other as such and habitually breed together. Such habitual breeding together under natural conditions is perhaps the best test of a species in the Linnean sense. 'The units within each Linneon (=species) form an inter-crossing community.' (Lotsy.) He adds: 'Consequently it is Nature itself which groups the individuals to Linneons.' These 'pairing communities' have recently been re-christened by Dr. Lotsy 'syngameons,'[3] a good name to express this aspect of the old 'species.'

I do not propose in these brief remarks to venture on that well-worn subject the inheritance of acquired characters—*i.e.* of such characters as are gained during the lifetime of the individual by reaction to the environment. There has always been a strong cross-current of opinion in favour of this belief, especially, in our own time, in the form of 'unconscious memory,' so ably advocated by Samuel Butler and supported by Sir Francis Darwin in his Presidential Address to the British Association at Dublin. Professor Henslow, as we all know, is a veteran champion of the origin of plant structures by self-adaptation to the environment. On the other hand, some geneticists roundly deny that any inheritance of somatically acquired characters can take place. In any case, the evidence, as it seems, is still too doubtful and inadequate to warrant any conclusion, so, however fascinating such speculations may be, I pass on.

To bring these introductory remarks to a close, we see that while the theory of Descent or Evolution is undisputed, we really know nothing certain as to the way in which new forms have arisen from old. During the reign of Darwinism we commonly assumed that this had happened by the continual selection of small variations, and we are no longer in a position to make any such assumption.

We have been told on high authority that 'as long as we do not know how *Primula obconica* produced its abundant new forms it is no time to discuss the origin of the Mollusca or of Dicotyledons.' (Bateson.) Yet this is just the kind of speculation in which a palæontologist is apt to indulge, and if kept off it he would feel that his occupation was gone! However, so long as we may believe, as already said, that, on the whole, like breeds like, that grapes do not spring from thorns or figs from thistles, there is perhaps still sufficient basis for some attempt to interpret the past history of plants in terms of descent. But certainly we have learnt greater caution, and we must be careful

[3] Lotsy, *La Quintessence de la Théorie du Croisement.* Archives Néerlandaises des Sciences, Sér. III. B., t. iii., 1917.

not to go far beyond our facts, and, in particular, to avoid elaborate derivations of one type of structure from another where the supposed transitional forms have but a purely subjective existence; we have realised the difficulty of tracing homologies. We may still be allowed to seek affinities, even where we cannot trace descent. And though we may sometimes go a little beyond our tether and give rein to bolder speculations, there is no harm done so long as we know what we are doing, and there may be even some good in such flights if our scientific use of the imagination serves to give life to the dry bones of bare description. On this subject I am somewhat more optimistic than Dr. Lotsy, who, abandoning his 'Stammesgeschichte' point of view, has dismissed all attempts at phylogenetic reconstruction as 'fantastic.'

There are some questions of the highest interest that at present can scarcely be approached in any other but a speculative way. Within the last year or two new points of view have thus been opened out. For example, Dr. Church's able essay on 'Thalassiophyta and the sub-aerial transmigration' has brought vividly before us the great change from marine to terrestrial life.

The origin of a Land Flora had, of course, been discussed with much ability before, but rather as incidental to a morphological theory. Dr. Church puts the actual conquest of the land in the foreground. We watch the land slowly rising toward the surface of the primeval ocean, the rooted sea-weeds succeeding the free-swimming plankton, and then the continents slowly emerging and the drama of the transmigration as the plants of the rock-pools and shallows fit themselves step by step for sub-aerial life when the dry land appears. It is a striking picture that is thus displayed to our view—whether in all respects a faithful one is another question; we must not expect impossibilities. The doubts which have been raised relate first to the assumed world-wide ocean, which seems not to be generally accepted by geologists. If continental ridges existed from the first (*i.e.* from the original condensation of watery vapour to form seas), the colonisation of the land may have followed other lines and have happened repeatedly. Perhaps, after all, that would not greatly affect the botanical aspects of the transmigration.

The other difficulty is, however, a botanical one. Dr. Church looks at the whole problem from the sea-weed point of view, and it is well he does, for sea-weeds have been badly neglected, especially by some of the great continental morphologists, who used to lead our speculative flights. Dr. Church is much impressed by the high organisation of many sea-weeds, especially, in the living marine flora, by that of the Brown Algæ. Here we find well-differentiated leaves, special reproductive shoots, extremely efficient holdfast roots, and, sometimes, a definite alternation of generations, while, on the anatomical side, we meet with true parenchymatous tissues, a well-developed phloëm and secondary growth in thickness. There is, in fact, in many respects, an anticipation of, or an analogy with important features which characterise the higher plants of the land.

Dr. Church believes that the chief morphological characters of the Land Flora were first outlined in the sea; that such characters were not

newly assumed after transmigration, but that they merely represent an adaptation to sub-aerial conditions of a differentiation already attained at the phase of marine phytobenthon (rooted sea-weeds). At the same time it is not suggested that any existing class of sea-weeds can be taken as representing the ancestry of the Land Flora; the transmigrant races are, as Algæ, extinct—they may have been Green Algæ of a high grade of organisation, on a level now perhaps most nearly represented by the highest of the Brown Seaweeds.

Thus the transmigrants, which were destined to become the parents of the Land Flora, are pictured as already highly organised and well-differentiated plants, which only needed to provide themselves with absorptive instead of merely anchoring roots, and with a water-conducting system (xylem and stomata) in order to fit themselves for sub-aerial life, while, on the reproductive side, the great change remaining to be accomplished was the adaptation of the spores to transport by air instead of by water.

It is clearly impossible to criticise the theory in detail, for the assumed transmigrants are *ex hypothesi* unknown; we can only form a distant conception of what they were from the analogy of the highest sea-weeds of the present day, which admittedly belong to quite different lines of descent. Dr. Church puts the transmigration so far back (pre-Cambrian) that not much help can be expected from fossils, but to this subject we shall return.

Some botanists find a difficulty in accepting the suggestion that plants already elaborately fitted out for a marine life could have survived the transition, however gradual, to a totally different environment. Such thinkers prefer to believe that lower forms may have been more adaptable, and that morphological differentiation had, in a great degree, to start afresh when the land was first invaded. My own sympathies, I may say, are here with Dr. Church, for I have long inclined to the belief that the vascular plants were, in all probability, derived from the higher Thallophytes. The view of the late Professor Lignier, now so widely accepted, that the leaf, at least in the megaphyllous or Fern-like Vascular Plants, was derived from specialised branch-systems of a thallus, assumes, at any rate, that the immediate ancestors possessed a well-developed thallus, such as is now known only among the higher Algæ. The Hepaticæ, as we now know them, clearly do not come into question, and the Pro-hepatics, which Lignier postulated as early ancestors, have only a theoretical existence, and if they were ever present in the flesh may well have been transmigrant Algæ.

The question now arises, how far have we any evidence from the rocks, which may bear on the transmigration and on the nature of the early Land Flora? A very few years ago no such evidence was available—such data as we then possessed seemed too obscure to discuss. Quite recent discoveries, especially those from the famous Rhynie Chert-bed, have shown that in Early Devonian times certain remarkably simple land-plants existed, which in general configuration were no more advanced than some very ordinary sea-weeds of the present day. At the same time these plants were obviously fitted for terrestrial life,

as shown by the presence of a water-conducting tissue and stomata, and by the manifestly air-borne spores. These simplest land-plants are the Rhyniaceæ (*Rhynia* and *Hornea*), while the third genus, *Asteroxylon*, was more advanced and further removed from any possible transmigrant type.

My friend Dr. Arber was so impressed by the primitive character of *Rhynia* (the only one of these genera then known) that he boldly called it a Thallophyte, while recognising, in respect of anatomical structure, an intermediate position on the way to Pteridophyta. This is not really very different from the view taken by the investigators themselves, though they call the plants Pteridophytes, which they certainly are, if we go by internal structure rather than external morphology. But if, as Kidston and Lang suggest, the Rhyniaceæ 'find their place near the beginning of a current of change from an Alga-like type of plant to the type of the simpler vascular Cryptogams,'[4] they must have been very primitive indeed and might even be regarded as fairly representing the true transmigrants which had not long taken to the land.

It is true that the Middle Devonian is much too late a period for the original transmigration (I believe there is some evidence for land-animals in the Lower Silurian), but one may argue that some of the transmigrant forms may have survived as late as the Devonian, just as the *Selaginella* type seems to have gone on with little change from the Carboniferous to the present time. There must have been many such survivals of earlier forms in the Devonian period, if Arber was right in regarding all the characteristic plants of the *Psilophyton* Flora as 'much more probably Thallophyta than Pteridophyta.'[5] Certainly some of them, apart from the Rhyniaceæ, have an alga-like appearance (e.g. *Pseudosporochnus*) and there is some evidence that such plants also were already vascular. There is, in fact, no doubt that the earlier Devonian Flora is turning out to have been on the whole more peculiar and more unlike the higher plants than we realised a few years ago. The Early Devonian plants cannot usually be referred to any of the recognised groups of Pteridophytes, and this is not owing to our imperfect knowledge, for it is just in those cases where the plants are most thoroughly known that their unique systematic position is most manifest. Arber called all the plants in question 'Procormophyta'—an appropriate name. As Kidston and Lang point out in their later work, the three groups—Pteridophyta, Bryophyta, and Algæ—are brought nearer together by the Rhynie fossils.

And yet there is evidence that about the same period stems with the highly organised structure of Gymnospermous trees already existed. I refer to remains of which *Palæopitys Milleri*, from the Middle Old Red Sandstone of Cromarty, is the type. We need much further investigation of these higher forms of Early Devonian vegetation, but we know enough to impose caution on our speculations.

[4] This view is further developed and expanded in the authors' fourth memoir, which I have had the privilege of reading in MS.

[5] *Devonian Floras, a Study of the Origin of Cormophyta.* Cambridge, 1921, p. 47.

The Rhyniaceæ, at all events, were leafless and rootless plants. In one species of *Rhynia* and in *Hornea* the aerial stems are entirely without any appendages, while in the other *Rhynia* there are hemispherical swellings, which have been identified by Arber with certain states of the spines in *Psilophyton.* The emergences of *R. Gwynne-Vaughani* have been interpreted as nascent leaves, but more recent observations, showing their late histological origin, have rendered this hypothesis very doubtful.

In *Asteroxylon*, a higher plant altogether, the stem is clothed with quite distinct leaves, though they are somewhat rudimentary as regards their vascular supply. Have we, in these plants, and others of contemporary date, the first origin of the leaf from a mere non-vascular emergence, or had reduction already begun, so that in Rhyniaceæ, for example, the leaves were in the act of disappearance? In the former case we should be assisting at the birth of Lignier's phylloids, the microphylls of the Lycopod series, though, as just mentioned, the outgrowths in *Rhynia Gwynne-Vaughani* may have had nothing to do with leaves.

But the opposite view may also be tenable. We have already seen that these plants have been referred both to the Pteridophytes and the Thallophytes; they also show signs of Bryophytic affinities, and I understand that it has even been proposed to include them in the Bryophyta, in which case every possible view will be represented. The *Sphagnum*-like structure of the columellate sporangium or sporogonium of *Hornea* and *Sporogonites* may justify the Bryophytic attribution, and it is then, of course, easy to extend it to *Rhynia.* If we were to adopt this opinion, we should probably have to regard these simple Devonian plants as representing stages in the reduction of the sporophyte to a sporogonium, the leaves being already nearly or quite lost, while the branched thallus was still much in excess of the simple seta of the modern Moss or Hepatic. Naturally we know nothing of the gametophyte, so that the material for comparison is limited. Kidston and Lang, however, have recently pointed out that the presence of spore-tetrads clearly indicates the existence of a gametophyte.

I make no attempt to decide between these views. There can be no reasonable doubt that the Psilophytales generally represent an earlier phase of Cormophytic life than any of the groups previously recognised. But we must not assume that *all* their characters were primitive. It has been pointed out that the Rhyniaceæ were peat plants, and that the peat-flora is apt to be peculiar. Under such conditions it is not improbable that a certain amount of reduction may have already been undergone, though this is not the view taken by the investigators.

There is one more point in connection with the Rhynie plants which may be mentioned, as it is of purely morphological interest, and may be more in place here than at a later stage of the discussion.

In *Hornea*, as Kidston and Lang have shown, the terminal 'sporangia evidently arose by the transformation of the tips of certain branches of the plant.'

They are, in fact, very little modified as compared with vegetative

parts of the stem. The epidermis and subjacent layers of the sporangial wall differ but slightly from the corresponding tissues of the branch, while the columella is continuous with the phloëm, and resembles it in structure. The sporangium has no special stalk, and in some cases is forked, like the stem, having evidently been formed when the branch was in the act of dichotomy.

In *Rhynia* the sporangia are better differentiated, but here also cases occur where the spore-bearing region differs little in structure from the branch which it terminates. In both genera the spore-containing organ is thus nothing but the more or less altered end of a branch, quite comparable to the stichidium, which is differentiated in some Red Seaweeds as the receptacle of the tetraspores, while in other Algæ of this group the tetraspores are produced in unaltered portions of the thallus. In *Hornea* the fertile branch-ending is less differentiated than in *Rhynia*, and we must be prepared to meet with related forms in which the spore-bearing region was not differentiated at all, except for the presence of the spores.

Goebel taught that the sporangium was an organ *sui generis*, a special reproductive structure, which had never arisen from any vegetative part of the plant.[6] His view has been generally accepted, but, in the light of the conditions in Rhyniaceæ, appears to be no longer tenable. While the spores may still be described as organs *sui generis*, for there have always been reproductive cells since plants became multicellular, the sporangium proves to be really a portion of the vegetative stem or thallus, which has gradually become specialised as a receptacle for the spores. The sporangium thus turns out to be strictly homologous with a definite part of the vegetative body of the plant. In these remarks I am glad to find myself entirely in accord with the views of Kidston and Lang, as stated in their fourth memoir on the Rhynie plants.

The recent work on the Early Devonian Flora has wide bearings. It has long been noticed that among the fossils of that period no typical Fern-fronds are found. Those remains which are most suggestive of Fern-like habit consist merely of a naked-branched rachis. It used to be assumed that the absence of a lamina might be explained by bad preservation. But, as Professor Halle points out, the chief reason for condemning the preservation as bad was the fact that a lamina was absent!

The evidence really seems to indicate that the so-called fronds of that age did not possess a leaf-blade. As Professor Halle says: 'In the Lower Devonian, finally, we find frond-like structures bearing sporangia, but no fronds with developed laminæ. One can hardly escape the conclusion that the "modified" fertile fronds may represent the primitive state in this case and that the flattened pinnules are a later development, as suggested by Professor Lignier.'[7] These naked

[6] 'Vergleichende Entwickelungsgeschichte der Pflanzenorgane.' Schenk's *Handbuch der Botanik*, Bd. III., Part I., p. 130, 1884.

[7] T. G. Halle: *Lower Devonian Plants, from Röragen, in Norway.* Stockholm, 1916, p. 38.

fronds may, in fact, be regarded as the little-differentiated branches of a thallus. It is often impossible to say whether we have to do with the ramification of a stem or with a frond. Halle even suggests that one of his species of *Psilophyton*, *P. Goldschmidtii*, may furnish us with an intermediate stage between the two, as required by Lignier's hypothesis. Plants of the *Rhynia* type may represent a still earlier phase, in which there was no differentiation whatever, but merely a branched thallus. It is a curious point that 'the circinate vernation of the Fern-fronds is paralleled in the branches of *Psilophyton princeps*.'

The evidence, as at present understood, seems to suggest that, in the earlier Devonian Flora, Ferns, properly so called, may not yet have been in existence. The predecessors of the Ferns (Lignier's 'Primofilicinées,' *not* Arber's 'Primofilices') were there no doubt, but not, so far as we know, the Ferns themselves. Yet it seems that highly organised stems of a Gymnospermous type were already present at about the same period. Thus the evidence from the older Devonian Flora, so far as it goes, materially supports the opinion that the Seed Plants cannot have arisen from Ferns, for the line of the Spermophyta seems to have been already distinct at a time when true Ferns had not yet appeared.

The idea that the Gymnosperms were derived, through the Pteridosperms, from the Ferns, which I once advocated, must, I think, be given up, on grounds which were stated two years ago at the Bournemouth meeting of the Association. It is safer to regard the Pteridosperms, and therefore the Seed Plants generally, as a distinct stock, probably as ancient as any of the recognised phyla of Vascular Cryptogams, and derived from some unknown and older source. At the same time the striking parallelism between the Pteridosperms and the true Ferns must be recognised. These views are essentially in agreement with those previously expressed by my friend Dr. Kidston.

I may be permitted to quote in this connection an interesting remark made by Professor Paul Bertrand in a letter received last year. He was speaking of a strange group of plants of Lower Carboniferous or possibly Upper Devonian age, the Cladoxyleæ. These plants have a complex polystelic structure in both stem and petiole, but seem to be quite distinct from the later and better-known polystelic family, Medulloseæ. Professor Bertrand, the chief living authority on the Cladoxyleæ, speaks of them as very primitive types, in which the distinction between stem and petiole was still but little marked. Yet he considers them as most probably Phanerogams. These views, if confirmed, imply that the Phanerogams or Seed Plants started as a distinct phylum, quite low down, at a phase when the differentiation between stem and leaf was still incomplete.

Without laying too much stress on an expression of opinion such as Professor Bertrand's, I believe the present evidence is in harmony with the view he suggests. The Spermophytes, as it seems, have been an independent class of plants from very early times; they are not to be derived from the Vascular Cryptogams, as we have hitherto conceived them, but are of the same standing with them, having sprung

from some long-extinct stock, comparable, perhaps, to Kidston's and Lang's Psilophytales, though not necessarily on the same line.

The significance of the Pteridosperms has perhaps been somewhat misunderstood. It now seems that they do not, as some of us once imagined, indicate the descent of the Seed Plants from Ferns, but rather show that the Seed Plants passed through a Fern-like phase; they ran a parallel course with the true cryptogamic Ferns, and, like them, sprang from some quite early race of land plants, such as Rhynie has revealed to us. But the phylum was never any more Fern-like than the Pteridosperms themselves. This, at least, is the view which now suggests itself, but our knowledge is still very meagre. We especially want to know more about the Devonian Spermophyta, for at present we have scarcely any evidence even of the existence of seeds in any Devonian Flora. Such data as we possess are all anatomical, and a disciple of Williamson must be on his guard against the risk of repeating the old mistake of the Brongniartian school.

Having ventured so far into speculative regions, it may be well to return for a moment to the facts, and ask to what extent our knowledge of the Fern-like Seed Plants has advanced since the original discoveries of 1903-1906. I fear that there is not very much to record. We now have one or two additional species of *Neuropteris* bearing seeds, and also the probable seed of *Heterangium*. Further, we have various indications of the characters of the pollen-bearing organs in some Pteridosperm genera, though the documents, being mostly in the form of impressions, are deficient in detail. Such new information as has come to hand confirms in a satisfactory manner our former conclusions, but does little to extend them.

On the anatomical side there has been more liveliness. We now know quite a number of Palæozoic plants, of varied structure, which have something in common with the better-established Pteridosperm families, Lyginopterideæ and Medulloseæ, while they certainly have nothing to do with Lycopods, Horsetails, or Sphenophylls. We therefore call them Cycadofilices or Pteridosperms. I prefer to use one name for them all and incline to the latter, for, while the plants are generally more or less Fern-like in structure, many of them show no special resemblance to Cycads.

At present we know of no fewer than eight families, based mainly on anatomical characters, which we provisionally include under Pteridosperms:

1. The familiar Lyginopterideæ (Lower and Upper Carboniferous).
2. The *Rhetinangium* family, founded on Dr. Gordon's new genus (Lower Carboniferous).
3. The Megaloxyleæ, discovered by Prof. Seward (Upper Carboniferous).
4. The Calamopityeæ, recently enriched by Dr. Kidston with a new genus, besides new species (Lower Carboniferous).
5. The *Stenomyelon* family, another of Dr. Kidston's discoveries, described by him in conjunction with Gwynne-Vaughan (Lower Carboniferous).

6. The *Protopitys* type, a singularly isolated one, elucidated by Solms-Laubach (Lower Carboniferous). The above are all monostelic. Next come the two essentially polystelic groups:

7. Cladoxyleæ, already mentioned, a somewhat mysterious race, of Lower Carboniferous or possibly even Upper Devonian age.

8. The well-known Medulloseæ (Upper Carboniferous).

It is noticeable that five of these families are Lower Carboniferous (or possibly, in certain instances, older); one (Lyginopterideæ) includes both Lower and Upper Carboniferous members, while two (Megaloxyleæ and Medulloseæ) are at present known only from the Upper Carboniferous.

Of the eight families in question there are only two (Lyginopterideæ and Medulloseæ), in which we have any evidence as to the fructification. The other six are known only by their vegetative and mostly by their anatomical features. Of these the Protopityeæ and the Cladoxyleæ are the most isolated, differing, for example, in the structure of their tracheides from the other families. There seems to be no reasonable doubt that the families represented by *Lyginopteris*, *Rhetinangium*, *Megaloxylon*, *Calamopitys*, *Stenomyelon*, and *Medullosa* are related, and belong to one and the same main phylum. Considering that members of two widely separated families in this series are known to have borne highly organised seeds, there is a strong presumption that the whole set were reproduced by seeds of some sort. In the case of the two families Protopityeæ and Cladoxyleæ the marks of affinity are less obvious, but even here there is more in common with the type-families Lyginopterideæ and Medulloseæ than with any other group.

I think then that we are justified, in the present very imperfect state of our knowledge, in provisionally keeping all these families together, as probably, in some wide sense, Pteridosperms. On this view, they formed a distinct, extensive, and varied class of plants, already very well developed in Lower Carboniferous times, and no doubt going back to the Upper Devonian, though here the available evidence is scanty.

The question may be asked: Did all the Seed-plants pass through the Pteridosperm phase, or were there other parallel lines of descent? Some recent work, no doubt, tends to link up the Cordaitales with the Pteridosperms. *Mesoxylon*, for example, is merely a *Cordaites* with centripetal wood in the stem, a character which strongly suggests an affinity with the *Lyginopteris* or *Calamopitys* type. In fact, some members of the Calamopityeæ (Zalessky's *Eristophyton*) show a certain approach to Cordaitales.

A more striking point is that no marked distinction has been found between the seeds of Pteridosperms and those of Cordaitales. The general community of seed-structure is strong evidence of close affinity and of a common stock.

There seems to be no proof that the family Cordaiteæ existed as such in Devonian times; we do not know much about them even in the Lower Carboniferous; the family is typically Upper Carboniferous and Permian. On the other hand, the *Pitys* family, which we include in the wider group Cordaitales, is as old as any known Pteridosperm;

Zalessky's genus *Callixylon*, an evident ally of *Pitys*, is of Upper Devonian age. The affinities of the still more ancient *Palæopitys Milleri* have not yet been determined.

The position of the Pityeæ hangs in the balance, at least until Dr. Gordon's new results are fully placed before us. From his discovery of the peculiar foliage and leaf-traces as well as from the stem-structure it appears that the Pityeæ form a very distinct group, farther from the other Cordaitales than we once supposed, and not much like any of the Pteridosperms either. At any rate, we may suppose that the Pityeæ branched off from the common stock low down, while the Poroxyleæ and Cordaiteæ may have been of later origin. For the present, however, one may be content to regard the early Spermophytes as constituting a single main phylum. Since these words were written, however, Dr. Margaret Benson has maintained a contrary view, arguing that the Cordaitales, Ginkgoales, and Conifers represent a wholly distinct stock, more allied to the Sphenopsida than to the Fern-like [8] races. The independence of this line has also been maintained by Prof. Chamberlain [9] and discussed by Prof. Sahni.[10]

On our hypothesis, the Upper Palæozoic phyla, with which we have to reckon, are the Pteridosperms (representing the early phase of the Seed-plants), the Ferns, the Sphenophylls, the Equisetales, and the Lycopods. These five lines were probably all well differentiated in the Upper Devonian Flora; the only doubt concerns the Equisetales, which seem not to be known with certainty before the Lower Carboniferous, but they were so well developed then that they must have existed earlier.

When we get back to the Middle and Lower Devonian the case is completely altered. Not one of the five phyla is here clearly represented, unless it be the Spermophyta; for these we have the evidence of apparently Gymnosperm-like stems. Thus the field is left absolutely open to speculation. We may imagine, either that the various phyla converged in some early vascular stock (illustrated by the Psilophytales), or that they ran back in parallel lines to independent origins among the transmigrant Algæ and, perhaps further still, to separate races of purely marine plants. Both views are represented in the publications of recent authors.

Dr. Arber, in his 'Devonian Floras,' maintained the early existence of three distinct lines of descent: the Sphenopsida, Pteropsida, and Lycopsida. In agreement with the present writer, he included the Equisetales in the Sphenopsida. Each of the three lines is described as descended from Thallophytic Algæ of a distinct type. Thus Arber's view was decidedly polyphyletic. It must, however, be borne in mind that the supposed ancestral 'Algæ' were plants in which he expected to find 'some form of primitive vascular system, at least as far advanced as in *Psilophyton*' (*l.c.*, p. 74).

[8] 'The Grouping of Vascular Plants,' *New Phytologist*, June 30, 1921.

[9] 'The Living Cycads and the Phylogeny of Seed Plants.' *American Journal of Botany*, vol. 7, 1920.

[10] B. Sahni, 'On the Structure and Affinities of *Acmopyle Pancheri*.' *Phil. Trans. R. Soc.*, Ser. B., vol. 210, 1920.

Arber derived the Sphenopsida from Algæ-bearing whorled branches of limited growth, converted into leaves, which were originally and always microphyllous. The Pteropsida, with which he associated his Palæophyllales (*Psygmophyllum*, with foliage like the Maiden-hair tree), were descended from Algæ in which the branches were large, numerous, scattered, and not whorled, eventually metamorphosed to megaphyllous leaves. The Lycopsida, on the other hand, were derived from Algæ in which the usually dichotomous axis bore emergences, metamorphosed to microphyllous leaves.

Thus, as regards the origin of the leaf, Arber was in general agreement with Lignier, while he differed from the French author in the important point that he did not derive the Sphenopsida from the Fern-stock, but kept them as an independent line.

A remarkable feature in Arber's hypothesis is his treatment of the Psilotales. He made this problematic family 'a quite independent race, also of Algal origin, which appeared on the scene long after the other races . . . possibly in Mesozoic times or even later' (p. 87). Thus he rejected both the connection with Psilophytales, suggested by Kidston and Lang, and the affinity with Sphenopsida, once maintained by the present writer.

We thus see that, on Arber's view, there were altogether four distinct lines of descent, running back independently to 'Thallophytic Algæ.'

Dr. Church, from quite a different point of view, arrives at somewhat similar conclusions, but he goes further. He says: 'Speaking generally, it appears safer to regard a "race" or "phylum" as the expression of a group of organisms which derived their special attributes from the equipment of a preceding epoch, if not in one still further back. Thus all the main lines of what is now Land Flora must have been differentiated in the Benthic Epoch of the sea (*i.e.* as algal lines), as all algal lines were differentiated in the Plankton phase. The possibility is not invalidated that existing groups of Land Flora may trace back their special line of progression to the flagellated life of the sea, wholly independently of one another (Pteridophyta).' ('Thalassiophyta,' p. 41.)

Taking the Lycopods and Ferns as an example, and arguing from their different types of flagellated spermatozoids, Dr. Church states: 'It appears impossible to avoid the conclusion that the Lycopod phyla only merge with those of the Filicineæ in a distant Plankton phase, even beyond an independent origin as benthic sea-weeds' (*l.c.*, p. 82). Thus the idea of independent parallel lines of descent is carried to its extreme limit. 'Each phylum goes back the whole way, without any connection with anything else.' Of course, this thorough-going polyphyletic conception is involved in the doctrine already mentioned—that morphological differentiation was attained in the sea before the transmigration.

I have cited Dr. Arber and Dr. Church as independent representatives, approaching the question from quite different sides, of the polyphyletic or parallel-phyla hypothesis. The opposite view, of convergent monophyletic races, is also well supported. Some reference has already

been made to Professor Halle's position. After speaking of the possible relation of the *Psilophyton* type to Lycopods on the one hand and Ferns on the other, he adds: 'From this point of view the whole pteridophytic stock would be monophyletic, the Lycopsida and the Pteropsida being derived from a common form already vascular. It would not thus be necessary to assume a parallel evolution of a similar vascular system along two different lines.' (Halle, *l.c.*, p. 39.)

He does not refer to the Articulatæ, of which, it is true, there are only the most doubtful indications in the Lower Devonian rocks. Halle, too, accepts Lignier's view of the twofold origin of the leaf, from emergences in the Lycopsida, from thallus-branches in the Pteropsida.

Kidston and Lang, in the light of their Rhynie discoveries, regard Halle's survey as 'a fair statement of the present bearing of the imperfectly known facts.' They lay great stress on the synthetic nature of their genus *Asteroxylon*, which they say 'appears to agree with *Psilophyton* in possessing in a generalised and archaic form characters that are definitely specialised in the Psilotales, Lycopodiales, and Filicales.' They add: 'The Geological age and succession of the Early Devonian plants are, on the whole, consistent with the origin of the various groups of Vascular Cryptogams from a common source.[11] We have already referred to the Bryophytic features, which have been recognised in the Rhyniaceæ. Kidston and Lang make use of these to extend their tentative conclusions to the Bryophyta. In concluding their third memoir they say: 'In *Rhynia* and *Hornea* we have revealed to us a much simpler type of Vascular Cryptogam than any with which we were previously acquainted. This type suggests the convergence of Pteridophyta and Bryophyta backwards to an Algal stock. The knowledge of *Asteroxylon* confirms and enriches our conception of a more complex but archaic type of the Vascular Cryptogams, which supports the idea of the divergence of the great classes of Pteridophyta from a common type, and links this on to the simpler Rhyniaceæ' (*l.c.*, p. 675.) The monophyletic view, though stated with appropriate caution, could not be more clearly expressed. It is fully maintained in these authors' later statements.

It is evidently impossible to decide between the two theories in the present state of our knowledge; we are now only beginning to acquire some conception of the vegetation of Early Devonian times. The discovery, however, of the existence at that period of an unexpectedly simple race of vascular plants to some extent favours a monophyletic interpretation, even though we accept with some reserve the wonderful synthesis of characters which *Asteroxylon* appears to exhibit. To some minds, too, the important points in which all existing Pteridophyta, however diverse, agree will still suggest a common origin not too remote. Among such common characters may be mentioned the alternation of generations with the sporophyte predominant; the development both of the spores and the sexual organs; and the histology, especially of the

[11] *On Old Red Sandstone Plants, showing structure, from the Rhynie Chert Bed*, Part III., p. 673. In Part IV. this conclusion is further emphasised, and it is suggested that the Rhyniaceæ are really too simple morphologically to suit the views of either Lignier or Church.

vascular system and the stomata. The community of reproductive phenomena is explained by Dr. Church on the principle that reproductive phases are inevitable and are therefore the same in all phyla. A like explanation may to a certain extent be applicable to somatic features, some of which may be the necessary consequences of the sub-aerial transmigration. Thus a polyphyletic hypothesis may no doubt be justified, but it urgently needs to be supported by further evidence of the actual existence of separate stocks among the earliest available records of a Land Flora.

The study of Fossil Botany has led to results of the utmost importance, in widening our view of the Vegetable Kingdom and helping to complete the natural system, to use Solms-Laubach's old phrase once more. One need only mention the Mesozoic Cycadophytes, the Cordaitales, the Pteridosperms, the Palæozoic Lycopods and Equisetales, the Sphenophylls, and now, most striking of all, the Psilophytales, to recall how much has been gained. We have indeed a wealth of accumulated facts, but from the point of view of the Theory of Descent they raise more questions than they solve. In this address I have briefly touched on some of the most general and most speculative problems in the hope of giving an opening for discussion. It might have been more profitable to deal in detail with definite facts of observation, but recent discoveries have brought us face to face with the great questions of descent among plants. However imperfect our data may be, both as regards the method and the course of evolution, the problems suggested, nevertheless, make urgent claims on our attention.

THE PLACE OF MUSIC IN A LIBERAL EDUCATION.

ADDRESS TO SECTION L (EDUCATIONAL SCIENCE) BY

SIR HENRY HADOW, C.B.E., D.Mus.,

PRESIDENT OF THE SECTION.

SOME years ago we were sitting round the fire in an Oxford Common Room. The Dean, who had the evening paper, let his eye fall upon a paragraph of musical criticism, and read it aloud in that tone of polished irony which we all knew to be his accustomed mark of disapproval. It was a harmless paragraph and contained somewhere an innocent technicality—I think 'sub-mediant.' When he had finished, he looked across to the eminent scholar by the fireside and said, 'Of course, you know what a "sub-mediant" is?' To which came the answer, slow, meditating and pious, 'God forbid!'

That is fairly typical of the attitude adopted in those days by scholarhip and literary culture toward the sister art. There were, no doubt, at Oxford and elsewhere, some notable exceptions, but in general the erudite world of England regarded music as something outside the scholar's province: something to be enjoyed as a recreation or a pastime, something even to be encouraged with generous rewards and good-humoured praise, as the Squire might dismiss the mummers on Christmas Eve; but as far as any sympathy or insight was concerned there had been very little progress since the time when, as Byron says,—

> 'John Bull, with ready hand,
> Applauds the strain he cannot understand.'

Applause, no doubt, as much as you will—artists live on applause—but as for understanding or even supposing that there was anything to be understood—'God forbid!'

Two other remarkable pieces of evidence may be adduced from more recent years. The Home University Library, issued by an enterprising publisher and controlled by a body of very distinguished editors, set out to supply a series of monographs on all subjects in which an intelligent reader could take an interest; science, history, poetry, politics, foreign travel—all were to be included, nothing human was to be alien from it. When, at the completion of the hundredth volume, it was pointed out that there had been no book on Music or on any subject in which Music could enter, the reply was that this omission was intentional for fear there should be no readers. Music was not regarded as one among the hundred subjects most likely to engage a reader's attention. There is a similar omission from the Cambridge History of Literature, that monumental work—*ære perennius*—which has become indispensable to every scholar of our

language or our letters. In it we have criticisms of books of almost every conceivable variety of topic, there is even sympathetic mention of books on pugilism, but there is no account of any books on Music. To emphasise the omission, Burney and Hawkins are both noticed, one as the father of Madame d'Arblay, the other as a rather eccentric member of Johnson's circle, but there is nothing to indicate that they wrote two great historical works which are still read with pleasure and consulted with profit. Everyone who has looked into the matter will have observed this same neglect in bibliographies and dictionaries, and other works of reference. Information about Music and Musical Literature must be sought as a rule in specialised volumes intended for musicians alone. It shares, no doubt, the all-embracing hospitality of the Encyclopædia Britannica, but it has not yet won citizenship in the daily life and civilisation of our people.

This is clearly an error, the perpetration of which is a serious loss to the country at large. Music is not only a source of noble pleasure —everyone admits that, at any rate in theory—it is a form of intellectual and spiritual training with which we really cannot afford to dispense. It is not merely a matter of pleasing the ear with successions of beautiful sound or stirring the emotions with vibrating tone and poignant rhythm. It is just as truly a language as French or Latin. It is just as truly a form of mental discipline as any subject in Science or Mathematics. That it can be studied with much more personal enjoyment than some of its compeers may perhaps be maintained; though on this score there is very little difference between it and literature; but even if that be granted, it is a very peevish asceticism which would, for this reason, depreciate its value in our educational system. The notes in a perfect melody follow each other by as sure logical necessity as do the words in a line of Shakespeare. They are not only beautiful; they not only appeal to the discerning ear by a thousand tones and associations; they have also an inherent significance, which in music, as in poetry, is a sure criterion of the difference between good art and bad.

No doubt there is here one salient difference between the two arts. In language a part at any rate of the significance depends upon the relation between the thing said and an external reality which it expresses or depicts; in music the whole significance is intrinsic, determined by the laws of its own form and the impulse of its own spirit. But though the kinds of significance are different, the fact of significance is equally present in both arts, and here I would venture to call in question two opinions, both of which seem to me entirely and fatally erroneous. One, which I saw a few days ago, in a volume of essays (and which, indeed, a reader of literary criticism may see almost once a week), is that poetry appeals to the intelligence and music to the emotions. The answer to this is that if poetry is to be summed up as an appeal to the intelligence, then Euclid was a very great poet; and if music has no further function than to appeal to the emotions, then it is nothing better than melodious nonsense. The other of the two is that any succession of notes constitutes a melody and that of such melodies intelligible music can be made up. The answer to

this is that such a sequence of notes can no more make a melody than a sequence of words makes a sentence. Everything depends on whether the words do or do not carry a meaning. Suppose, for instance, I wrote a sonnet of which the last line should run

'And purple decks the fragrant empyrean,'

I should have produced a sequence of quite admirable words, but it would not be a line of poetry. In just the same way the difference between a melody of Beethoven and the types of melody which once fell under the censure of Sir Hugh Allen is very largely that the melody of Beethoven has a noble meaning, and that the bad tunes of the streets have either an ignoble meaning or none at all. It may frankly be admitted that a vast proportion of what is printed and sold as music is far below this criterion; it is meaningless and therefore worthless. But if the advocates of literature or of the representative arts feel any inclination to despise music on this score they may be recommended, before pronouncing judgment, to look at home. The present generation of English readers has bought 130,000 copies of 'The Young Visiters,' the last generation made the fortune of Mrs. Henry Wood, its predecessor of Martin Tupper, and so the tradition stretches back through T. H. Bayly, Robert Montgomery, and a whole series of false idols surfeited with undiscriminating incense. The state of pictorial art in this country may be attested by some of our print shops, many of our private collections, and most of our municipal galleries. Indeed, it is not from the poet or the artist that one usually hears this argument. They know too well of what slender glass their houses are built. In all arts alike the work which endures is the work which appeals to the whole nature of man, spiritual, intellectual, emotional, and of this there is plenty in music to give full justification to its claim.

Here an objection may be lodged—it may be said that this is merely special pleading, that music would not have been neglected unless it had deserved neglect; and in this there is a great measure of truth. The case for music has been badly presented; a great many hearers who really understand it are no more conscious of the fact than M. Jourdain knew that he was talking prose, and the vast majority who accept it without understanding do so because they vastly overrate its difficulties and are repelled by some unnecessary formalities in its method.

A good many treatises on music correspond not to the writings of literary critics, but to elementary school books on grammar; they are concerned with alphabets and case endings and rules of syntax. The reader who takes them in hand is likely to fling them aside with the same impatience with which Montaigne dismissed the 'trash-names of grammar' which learned men had assigned to 'the tittle-tattle of his chambermaid.' It is not that the technical terms in music are any worse than those in other arts and sciences; they are less aggressive than the botanical description of a rose, and shorter by several syllables than the usual designation of a chemical compound, but they somehow seem to have occupied more of the field. They have forced themselves needlessly upon our attention; they have correspondingly led people to

believe that all musical criticism springs from their tangled roots. And to this may be added a real difficulty which music specially has to confront. Our ordinary language has been so framed with reference to external nature and the life of man that the critic of literature or painting has a far easier task than the critic of musical style or musical structure. Music is equally philosophical in basis, but its philosophy is, in the nature of the case, if not more penetrating than that of the poet, a little more abstract in form. Compare, for instance, a play of Shakespeare with a symphony of Beethoven. The comparison is really extraordinarily close; there is the same kind of architectonic power in the construction; there are the same points of interest and adventure; there is the same high and noble emotion; there is the same humour; there is even, allowing for the difference of medium, the same characterisation. But when Mr. Bradley analyses for us a Shakespeare play, there are a thousand points on which he can illustrate his meaning in words and phrases which directly relate his experience to human life. When Sir George Grove analyses a symphony of Beethoven, he is hard put to it to find any verbal analogues at all; when they do come they hardly seem more convincing than metaphors, and almost every point in his admirable account has to be illustrated and enforced by musical examples which the majority of people persistently declare themselves unable to read. The result is that the musical critic has often to substitute emphasis for persuasion, and has tended to dogmatise—not because he is unsure about his convictions (though this is a common basis of dogmatism), but because the difficulty of expressing them drives him to an unusual trenchancy.

Another reason for the prevalent error is that musical history has been far too sharply separated from the general history of civilisation. This, again, is a matter of proportion; the English Histories of my boyhood were mainly occupied with battles and treaties, and paid very little attention to letters or science or discovery, but at worst they have nothing to show parallel to Lord Macaulay's great History of England, which in an exhaustive account of the reign of James II. finds no room for the mention of Purcell. The result is again the loss of human interest, which tends to relegate music into a remote and abstract world which is far away from men's business and bosoms. Professor Dowden once wrote a very remarkable essay about the influence of the French Revolution on English literature; an essay of equal interest and importance might be written about its influence on Viennese music. And indeed we are coming more and more to see that the whole artistic expression of the people is an index of its national character and a symptom of its national health.

It is not, therefore, because music is unworthy of a place in our intellectual life that we have hitherto left it so much on one side. We have been frequently reminded of late—and we cannot be reminded too often—that the one supreme period of English music, the period in which our composers stood in the forefront of the whole world, is the period which produced the great Elizabethan seamen and the great Elizabethan dramatists; the period in which Drake circumnavigated the world and Shakespeare the soul of man; and, what is more, that

our madrigal writers and Church writers, and writers for the Virginals and the Lute, were not isolated phenomena, brought by some unexpected Providence into a country unfit to receive them; they were the natural outgrowth of a civilisation which accepted music as an essential part of a man's upbringing and nurture. In the days of Elizabeth the whole of England was full of music, as Shakespeare's plays are full of it, and we are not so much better than our Elizabethan ancestors that we can afford to disregard what they claimed as one of the most valuable parts of their education.

It has been said that complaints against an abuse have usually been most urgent at the time when the abuse is in the natural course of being redressed, and this is certainly true of the strictures which have been made in the earlier part of this paper. During the last twenty years an extraordinary change has taken place in the part assigned to music in our civilised life. The reform is only just at its beginning, but it has as a matter of fact begun, and though we may be like Caesar and 'think nought done while aught remains to do,' we can, at any rate, see round us enough signs of progress to go forward with considerable encouragement. For one thing, the study of music no longer means, as it did a generation ago, a reluctant drill in the elementary practice of a musical instrument. We are learning the wisdom of confining our executants to those who show some taste or aptitude for performance, and have come to see that confining the study of music to them is just as irrational as it would be if we confined the study of literature to students who aimed at being poets or actors. By all means develop and encourage our specialised schools of music. They have a great tradition; they have done and are still doing magnificent work, and one of the results which they have already produced is that we are no longer obliged to look to our Continental neighbours for executive and creative artists, that our own players and singers and our own composers can hold their own against any rival in the world. But still more important, and at any rate more germane to this present paper, is the recognition of music as an essential part of that liberal education which we are endeavouring to bestow upon all citizens throughout the country, and it is in this that the most remarkable advance is now being made. Our public schools, which half a century ago treated music as an unpopular alternative to cricket, have now begun to find a place for it, if not always in the curriculum, at any rate in the corporate life. The old days of the visiting music master, shy, embarrassed, probably a foreigner, ill at ease in Common Room, hardly counting as a member of the staff, have now been replaced by a more genial and hospitable system, by which the school music is placed in the hands of a well-educated and genial colleague who can mix with his fellows on equal terms and is as sure of a welcome as any among them. The school concerts are more numerous and of far higher quality than they were in the old days, and in many schools they are prefaced by explanatory lectures on the more elaborate or recondite works performed. Most of all, perhaps, is the change noticeable at Oxford and Cambridge, which have become radiating centres of musical activity and are sending out every year

trained men to carry on their tradition through every corner of the land. Yet hardly less in importance is the action which has recently been taken by some county and municipal authorities, who have appointed special Directors of Music to organise the work for all the schools in their area. The improvement already effected by this means is very remarkable, and will be the more conspicuous still as the movement spreads and advances.

What, then, it may be asked, further remains for us to do? The answer may be suggested on the following lines: First, that music should be recognised in our formal education of school and college; that it should be given a place in the curriculum and full recognition in the examination system. It is likely that this proposal will at once arouse an outcry, on the ground that it is adding a new subject to an already overloaded scheme. But, in the first place, I have never known any teacher complain of overloading in regard to his particular subject; and, in the second place, I would suggest that music for the whole school should consist of little more than class singing and an occasional concert or lecture, and that those who have the taste and aptitude for pursuing its serious study should do so in substitution for some other subject. The study of a great composer might be made of as much educational value as that of a great poet. On the other side, the qualities of abstract thinking and of mental construction implied in the study of musical form are closely analogous to those of our natural sciences, and might well be made of the same educational value. It should be quite possible to draw up a syllabus for music which would fit into the existing schemes of school and college work, and which would neither encourage faddists, nor excuse idlers, nor produce that lamentable class of people, not yet quite extinct, who talk emotionally about music without any understanding. Secondly, there should be a great improvement in the place of music in our libraries. Every public library in the country and, if possible, every school and University library should contain a musical department which includes not only the standard classical compositions, but the first-rate books on musical æsthetics and criticism. There are a great many more of such books than is commonly supposed. Almost every civilised nation has contributed to them, and they range from entertaining volumes of light essays to such profound philosophical treatises as Schopenhauer's book on 'The Platonic Idea.' At present an allusion to music in average society would tend to cut the conversation down to the roots; half the company would feel nervous and uncomfortable, half apprehensive of a dull or pontifical lecture. It ought to be just as possible for people to be well read in music and interested in communicating their ideas about it as they are at present in ordinary civilised society over questions of literature or the representative arts. And this leads to a third point—that the ordinary educated man ought to be trained to read music. The script, though it is not always very rational, is not unduly difficult, and its mastery unlocks the door of a new literature. A very great many of us have only rare and infrequent opportunities of hearing the best music. We have no means of refreshing our memories between recurrent performances, and we therefore lose

a great deal of the effect which they produce. If we learn to read (by which I do not mean to sing or play at sight, but to read silently as one reads a play or a novel) we have added another valuable resource to our intellectual life. Lastly, and as corollary to all these, we all of us need to simplify our attitude towards music. One result which follows from the uncertainty of its position is that it has not yet found its proper bearings. People who have any musical gifts are a little inclined unduly to stress them, because they have a misgiving that their neighbours do not rate music sufficiently high. The outside world, which would be very glad to understand more about music, but regards it as a kind of hieroglyphic or sacerdotal secret, which the profane may not penetrate, is equally reticent because it is afraid to put forward an opinion in the presence of the expert. We want really to pool our knowledge, to concentrate our interests, to develop on this side, as we have on so many others, a sense of comradeship and co-operation, and this can only be done if we are all made free of the company; if our musical education is such that we can meet each other as frankly and openly in this field as educated men are accustomed to do in the discussion of science or poetry. And this we can only do if music is enfranchised in our educational system, if it takes its assured place in the community and is invested with the full rights of intellectual citizenship.

THE STUDY OF AGRICULTURAL ECONOMICS.

ADDRESS TO SECTION M (AGRICULTURE) BY

C. S. ORWIN, M.A.,

PRESIDENT OF THE SECTION.

For the third year in succession the University of Oxford has been honoured by the selection of one of its resident members for the office of President of the Agricultural Section of the British Association, and on the occasion of the Edinburgh Meeting it may be of interest to recall that historically, at all events, the study of Agriculture and Rural Economy at Oxford takes second place to no university with the single exception of Edinburgh. I am not a scientist in the commonly accepted sense of the word, and nothing but my deep conviction of the need for wider recognition of the importance of the study of economics in connection with agricultural research work could have overcome my reluctance to assume an office in which I have been preceded by such a long line of distinguished men.

It is now about five-and-twenty years since research and educational work in agriculture began to be developed seriously in this country. Since that date a very great deal of effort has been expended in investigating the forces by which plant and animal life are controlled, and to bring natural science to bear in every way upon the problems of food production. Work along these lines has been productive of most valuable results to the farmer; but at the same time the fact has been overlooked that, when all is said, farming is a business, and if it is to succeed as such it must be carried on with a clear regard for the economic forces which control the industry. So, whilst desiring nothing but the fullest recognition of work in the fields of natural science applied to the investigation of farming problems, I must express without any qualification the view that the equal importance of the study of these economic forces has never been adequately recognised. Educational and research work in agriculture which takes no account of the dominant importance of economics must always be ill-balanced and incomplete, for farming business requires for its proper control a consideration of human relationships, of markets, of transport, and of many other matters which should come within the purview of the economist, as well as, or even more than, a consideration of questions regarding the control of plant and animal growth with which the scientist, in the limited sense of the name, is concerned. No one could wish to deny the need for the close and continual study of the soil and the means by which it can be made to produce more abundantly;

no one could deny the need for research work in problems of animal and plant life. But the main concern of the farmer is to know not so much that which he can *grow* and how best to grow it as that which he can *sell* and how to sell it at a profit. Given the necessary capital and labour, conditions may be contrived under which any soil may be made to produce any crop; but the wisdom or otherwise of embarking upon any particular form of production can be determined only by a study of economic forces. In Bedfordshire, for example, considerable areas of very moderate land are met with given up to a most intensive form of agriculture; but land equally suitable for a similar form of farming may be met with in many other parts of the country which is producing not a tenth part of the value in food products nor employing a tenth part of the capital and labour, whilst at the same time the systems under which it is farmed are fully justified by the results. The reason of the difference, as doubtless everyone realises, is that the land in the former case is so situated that it has access, in the first place, to supplies of organic manures on an abundant scale and at a cheap price, and, in the second place, to markets crying out for its produce, whilst one or both of these facilities are denied to the other areas. In the Chilterns district of Oxfordshire farming a generation ago was mainly directed to the production of corn and meat, and nothing that has arisen out of the work of the investigators along lines of natural science would have called for any radical changes in agricultural policy on these soils. But economic forces, inexorable in their effect, have brought about a revolution, and arable land previously under corn and sheep is now laid down to grass or occupied with fodder crops for the maintenance of the dairy herds which have replaced sheep throughout the area. Again, in the hill districts of England and Wales there occur combes and valleys admirably adapted by soil and climate to the production of potatoes, and the highlands of Devonshire and Somerset may be cited in illustration. In these places, however, in the majority of cases, even though good markets may exist—Somerset, for example, imports potatoes—the lack of transport facilities makes it impossible for the farmers to produce anything which does not go to market on four legs. Coming last to the question of human relationships, we find that it is possible to organise much more intensive forms of agriculture than any of our own, which would be an enormous advantage to a consuming nation like Britain; examples of such are to be met with in varying degrees of intensity in many countries. The Chinese, one reads, have increased production per unit area to an almost incredible extent, and in a lesser degree a similar state of affairs exists in parts of France and in Belgium (so often held up to us in this country as a model of productive capacity which we should strive to emulate). But in all these places the results are only achieved by a prodigal use of labour. The nation gains, no doubt, in the volume of produce available for its consumption, but the individual producer, deprived under this system of the opportunity to apply his manual effort in conjunction with an adequate amount of capital and land, is sacrificed to the consumer's advantage, and is driven to spend himself, year in and year out, for a reward for his toil which the

British worker, with so many alternative openings in more profitable directions available for him under our industrial system, would never for one moment submit to. From what I have read, I imagine that the fact which drove so many Scottish crofters across the seas was much less the selfishness of deer-stalking landlords than the opportunity for exchanging a few acres of rocks and heather in the Highlands for 160 acres of the virgin soil of Canada. People only submit to poor conditions of life when they have no alternative, and one of the most important studies awaiting the investigator of agricultural economics is that of the lines on which to develop the industry so as to give the worker the biggest reward for his toil.

These few illustrations may serve to indicate the over-riding importance of the economic factor in farming just as in any other business. It is a common experience in industry that many scientific and technical processes are possible which are not profitable, and it is in the light of the profit that they leave that all of them must be judged.

Economic conditions are subject to continual change, and the variations may be both sudden and extreme. This makes it the more needful to be continually recording experience and to examine it for the facts that emerge from which to obtain guidance for future policy. Much information is required both for national and individual guidance. Of late years, for example, there has been much advocacy of more intensive cultivation of the soil; it is said that by closer settlement and more intensive methods the production from the land could be much increased. On the other hand, there are those who advocate a development of extensive farming as being the only means by which to attract capital to the land and to pay the highest wage to the worker. Both sides to this controversy can and do produce evidence in support of their views, and some figures derived from a survey made by my colleague, Mr. J. Pryse Howell, will serve to illustrate both. The total area surveyed was 9,390 acres, divided into fifty-two farms of various sizes, and the region was selected by reason of the uniformity of the general conditions. All available data for each holding were collected, and after grouping the farms according to acreage the figures were thrown together and averaged for each group, with the following result:—

Production per Unit of Land and per Unit of Labour from Holdings of Various Sizes.

Group	No. of Farms in each Group	Average Size of Farms	Average Arable Land per cent.	Altitude	Average Rent per Acre	Average Men per 100 Acres	Sales per Acre	Sales per Man
Acres		Acres		Feet	s. d.		£ s. d.	£ s. d.
I. 0–50	5	39	17	341–369	32 10	7·1	11 19 11	168 19 0
II. 50–100	10	78	22	319–384	33 0	6·4	9 19 2	156 2 0
III. 100–150	14	138	21	370–453	27 2	4·2	7 19 1	189 0 0
IV. 150–250	11	201	11·7	330–411	28 4	3·3	7 5 8	222 12 1
V. over 250	12	356	18·0	286–435	26 5	2·6	8 4 4	316 19 0

It will be noted that the conditions under which the farming is carried on in the various groups show no material differences as between one group and another, except in the matter of area. There is a tendency for rent to fall as the size of the holdings increases, but it is not pronounced, and in one case (Group IV.) the percentage of grass land to arable land is considerably higher than in the rest; but, considering the variations which must be expected in the conditions prevailing over any area of fifteen square miles in extent, it may be claimed that in respect of altitude, quality of land, and proportion of arable to grass the holdings in these five groups are fairly comparable. Taking the results as they stand, the fact emerges that employment and production vary inversely with the size of the holding, but that the production per man employed varies directly with the size of the holding. Thus, on the one hand, the advocates of closer settlement and the intensive methods which must necessarily follow if men are to live by the cultivation of small areas of land would seem to be justified, in that the results shown by the survey indicate the highest amount of employment and the greatest product-value in the smaller groups. On the other hand, the advocates of more extensive methods of farming can point to their justification in that it is clear that the efficiency of management is greatest in the larger groups if the standard of measurement be that of product-value per man employed.

However, it is clear that either party is drawing conclusions from incomplete data. The efficiency of any farming system can only be judged by an examination of the extent to which all the factors of production are utilised and balanced under it. Each of the assumptions made from the figures above ignores entirely the factor of capital. Land, labour, and capital are all required for production, and the *optimum* system of farm management is that which utilises all three together so as to secure the maximum result from each. If information were available as to the capital utilised in each of the five groups in the survey it might be found that in the smaller groups labour was being wastefully employed, and that an equal number of men working on a larger area of land with more capital, in the form of machinery equipment, would produce an equal product-value per unit of land with a higher rate of output per man employed. Equally it might be found that in the larger groups the use of more labour, or a reduction in the area of land, might produce the same product-value per man with a higher rate of output per unit of land. Obviously there can be no absolute answer to the question of what constitutes the most economical unit of land for farm production. The quality of land in certain cases, and market, transport, and climatic conditions in many more, make it impossible to determine even within wide limits the size of the holding on which the principal factors of production can be employed with maximum effect. Within similar areas, however, and in limited districts, much work can and should be done by agricultural economists to collect evidence on this point for the information of all concerned with the administration of land.

Another matter of the utmost importance to the farmer and to

the public alike, and one which is crying out for investigation on a large scale, is the distribution and marketing of farm produce. Attention has been drawn at many times to the discrepancy between the price realised by the producer and the price paid by the consumer for the same article. In connection with market-garden produce, for example, the Departmental Committee on the Settlement or Employment on the Land of Discharged Sailors and Soldiers stated in their Report (Cd. 8182, 1916) that 'the disparity between the retail prices paid for market-garden produce in the big towns and the small portion of those prices received by the growers is utterly indefensible. It demonstrates a degree of economic waste which would ruin any other industry.' No evidence was published by the Committee as to the facts upon which this conclusion was based, but a recent inquiry made by the Ministry of Agriculture into the prices prevailing at various stages in the distribution of vegetables in London may be quoted in confirmation of it. Figures were collected to show the amount received by the producer, the wholesaler, and the retailers for various classes of everyday garden stuff, with results as shown below.

PRODUCER'S, WHOLESALER'S, AND RETAILERS' PRICES FOR MARKET-GARDEN PRODUCE, JANUARY 1921.

—	Cabbages, medium grade, per doz.	Cabbages, bottom grade, per doz.	Cauliflowers, top grade, per doz.	Sprouts, top grade, per 28 lb.	Turnips, medium grade, per cwt.
	s. *d.*	*s.* *d.*	*s.* *d.*	*s.* *d.*	*s.* *d.*
Producer . . .	0 3	0 2½	3 0	3 6	3 0
Wholesaler . .	1 0	0 9	5 0	—	5 6
Retailers—					
(*a*) Stalls and barrows	2 6	2 0	6 0	—	14 0
(*b*) Suburban shops . .	3 0	2 6	8 0	—	14 0
(*c*) Stores and high-class-shops .	4 0	3 0	10 0	14 0	18 8

One has only to glance at the prevailing methods of distribution to realise their wastefulness. The street in which I live contains ten houses, and each day four milk-carts, three bakers' carts, three grocers' carts, and two butchers' carts deliver food to them. Twelve men, horses, and carts, not to mention a host of errand-boys on foot and on cycles, to deliver food to ten families! While we are content with such a loose organisation of distribution as this represents, we must not wonder if the prices received by producers seem disproportionate to those paid by consumers, particularly when the produce partakes of the nature of market-garden stuff, bulky, perishable, and of low value. But apart from the question of methods of distribution, and the advantages to producer and consumer alike which would accrue from some co-operative organisation directed towards the elimination of unnecessary retailers who do no real service to either of them, an investigation of transport and marketing costs would show to what extent they are being exploited by the distributor. The farmer suffers equally with the market-gardener. At the present time I am getting 1*s.* 9*d.* per

gallon for milk sold to a middleman from my farm, and for this milk my wife is charged 3*s*. per gallon. I am selling lamb at 1*s*. 4*d*. per pound for which she is charged 2*s*. 6*d*. per lb., and if the drought had not upset the crop I should be selling potatoes at an equal disparity as between wholesale and retail prices. Can anyone say whether these figures do or do not represent a fair division of total cost as between producer, retailer, and consumer? It may be asserted with confidence that no one can speak with authority upon the subject. The only figures which we have been able to collect at Oxford on the cost of distribution relate to milk, and the most recent that we have are those for the year 1918. In that year in a Midland manufacturing town we found that the distribution costs of a large producer-retailer were as follows:—

	£	s.	d.
Labour — Manual and clerical	1,242	10	2½
Labour — Horse	497	0	9½
Rent	75	0	0
Sundry purchases, depreciation, general expenses, &c.	430	2	1
Total cost	£2,244	13	1
Number of gallons of milk distributed	112,833		
Cost of distribution per gallon	4·77*d*.		

Doubtless the conditions have changed since that year, nor is it possible to generalise from a single example; but, nevertheless, the figure for the gallon-cost seems to indicate that both farmer and consumer are suffering in the interests of the distributor, though it is impossible to say without further investigation whether the profit secured by retailers generally is excessive, or whether the difference between distribution cost and the margin out of which it is paid is necessary owing to an excessive number of distributors.

As to the other points named, meat and potatoes, no evidence exists at all, and the position with regard to them and also to milk is only indicated to emphasise the need for a full investigation of the economics of distribution.

At the present time labour problems afford a useful example of the need for further investigation of the economic problems of agriculture. The agricultural industry has been fortunate in that it has escaped the serious labour troubles which have shaken many other industries so badly during the past few years. This has been due in part, no doubt, to the closer personal relations which exist between employer and employed in agriculture than in other enterprises, and in part to the intervention of that often unfairly criticised body, the Agricultural Wages Board, but agricultural employers have also to thank the fact that agricultural labour is difficult to organise. Much controversy in the past would have been avoided, and the possibility of future difficulties could be faced with more confidence, if all the facts relating to labour had been and were being studied over the country generally. The labourer is often blamed for results which are due to the inefficiency of the farmer as a manager. When wages were low it may have been that the labourer was the cheapest machine, but in proportion as his remuneration approaches more nearly to the standard of reward in competing industries, so will the necessity for making his work more

productive be intensified. The value of the output from the farm per man employed is not the only measure by which to gauge the efficiency of the management, but is certainly one of primary importance. A man with a spade can dig an acre of land in about two weeks at a cost to-day of about 4*l.* 10*s.*; a horseman and a pair of horses can plough an acre in about a day and a-half at a cost of about 1*l.* 15*s.*; a farm mechanic on a tractor can break up an acre in about a quarter of a day, and although in the absence of sufficient data the comparison cannot yet be completed by reference to the cost of motor ploughing, it is fairly safe to suggest that when all the factors are considered—speed, less dependence upon atmospheric and soil conditions, as well as actual cost—there will be a still further advantage to be derived by investing the manual worker with the control of mechanical power. Thus it may be that high labour costs to-day are due in many cases less to the inefficiency of labour and more to the inefficiency of management. In a recent issue of *The Times* an agricultural writer expressed the view that if the means existed for determining the proportion of the net returns of agriculture accruing to-day to labour, it would be found that labour was taking an excessive toll of farming results. This view is probably very generally held, and it affords a good example of the misconceptions which may and do arise in people's minds in the absence of exact information upon which to base their assertions. This happens to be one of the questions which have been the subject of investigation at Oxford, though only on the small scale that the means at the disposal of the University has admitted. An investigation was made before the War of the Distribution of the Net Returns of Agriculture as between landlord, farmer, and labour. The net returns are calculated from the net output, and the net output was ascertained by the method followed in the Final Report on the First Census of Production of the United Kingdom, 1907 (Cd. 6320). Under this method the cost of materials at the works is deducted from the value of the output at the works, and the difference constitutes for any industry the fund from which wages, salaries, rent, royalties, rates, taxes, depreciation, advertisement, and sales expenses, and all other similar charges, have to be defrayed, as well as profits. The same basis of calculation was adopted in the Report of the Board of Agriculture and Fisheries on the Agricultural Output of Great Britain (Cd. 6277) made in connection with the Census of Production Act, 1906. In applying this measure of net output to the agricultural industry the method is to value the farmer's capital at the beginning of the year and to add to this figure all live and dead stock bought during the year, foods, manures, tradesmen's bills, on-cost and establishment charges, &c., and to deduct the total from the sales during the year added to the valuation of the farmer's capital at the end of the year. Only in the case of the workers is their share of this net output available as net income. The landlord has to incur a considerable expenditure upon the farm in the way of repairs and maintenance, and this must come out of his share of the net output. From an inquiry conducted by the Land Agents' Society in the year 1909 it appeared that about 30 per cent. of the rent received by the landlord is expended by him in repairs, insurance, management, and similar payments neces-

sary to maintain the property in a condition to produce the rent. The farmer, too, may have certain expenses to meet not covered by those deducted in arriving at the net output, and his share of this figure has also to cover some rate of interest on his working capital besides the reward due to him for the exercise of his managerial functions. Thus, in considering the distribution of the profits of agriculture between the three interests concerned, it is necessary to distinguish between net output as defined in the Census of Production and what may be termed the net returns. The net returns are ascertained by deducting from the net output any additional expenses of the business not already allowed for; a sum representing about 7 per cent. interest on the farmer's capital (this figure being based on current rates for money), and one-third of the amount of the rent.

This method for calculating net returns was applied in 1913 to six farms scattered all over the country and differing from each other in almost every way as to systems of management, soil, locality, and so forth, and it was found that the proportions accruing to each of the three interests varied hardly at all, and that it would be safe to say that 20 per cent. of the total was going to the landlord, 40 per cent. to the farmer, and 40 per cent. to labour. Owing to the disorganisation of the work arising out of the War it was not possible to carry on the investigation on each of these six farms, but it was continued in connection with one of them down to the year 1920. This farm may fairly be described as typical of 'average to rather indifferent' conditions. It was a tenant-farm, about a thousand acres in extent, commanding a rent of less than 1*l*. per acre, about three-quarters arable, situated on light to medium land, seven miles from a station, and farmed mainly for production of corn and meat. Taking the above proportions, namely 40 per cent. each to farmer and labour and 20 per cent. to landlord as the pre-war rate of distribution, and calling each of these shares 100, the proportion of distribution between the three interests varied during the following six years as shown below:—

Distribution of the Net Returns from Farming between Landlord, Farmer, and Labour during the years 1913-14—1919-20.

Year	Landlord	Farmer	Labour
1913–14 (Standard) .	100	100	100
1914–15 . . .	97	104	99
1915–16 . . .	94	108	98
1916–17 . . .	91	115	94
1917–18 . . .	90	111	99
1918–19 . . .	87	115	98
1919–20 . . .	89	109	102

The figures are interesting in several ways. In the first place they seem to disprove the suggestion referred to above, that labour has been taking an undue share of the net returns from farming, for an examination of the figures in the 'Labour' column shows that until the

institution of the Agricultural Wages Board in 1917 the tendency was in the direction of a slight but steady reduction in the proportion coming to the workers; the effect of the Wages Board Orders was to steady this tendency and, ultimately, to bring labour back approximately to the position it occupied in 1913-14. If the figures could have been continued for another year it is likely that they would show a material increase in the workers' share, but, even so, it would be found that this increase had been achieved without reducing the farmer's share below his pre-war proportion. In the second place, the figures confirm the experience of landowners in that the landlord has received no part of the increased prosperity of farming, whilst, as everyone knows, his expenses of maintenance have enormously increased. Briefly, the situation is that, thanks to the Agricultural Wages Board (and its appointed members may take heart from the fact), the workers have been maintained in the same position as regards their share in the net returns as that in which they were before the war, whilst the farmer has received his share in the increase realised during the past few years, together with that which would have gone to the landlord had the pre-war scale of distribution been maintained. Rents and wages under normal conditions are slow to adjust themselves to changes in farming fortune, and, except in a time of violent economic upheaval, it is right that this should be so, for if the landlord may be regarded as a debenture holder, and labour as a preference shareholder, then the farmer, as the ordinary or deferred shareholder, has to bear the brunt, and if he must take the kicks so also is he entitled to the halfpence.

Turning now from problems in which either the nation generally or whole classes of the industry are concerned, it may be stated that there are many economic problems arising on the farm itself in the solution of which the individual farmer should be able to derive help from the economist. Some of these problems are so simple that their solution should be obvious, but the fact remains that waste in its most easily eliminated forms is constantly to be met with on the farm. The need for the study of the economic use of manual labour has already been referred to in another connection, but, granted that the balance between the employment of land, capital and labour on any farm has been established, cases are continually met with where labour is being mismanaged. It is a not uncommon practice at threshing-time to take the horsemen from their work to assist at threshing, and as this operation can only be performed in dry weather, it may be assumed that the horses might usually be employed on threshing days. With manual labour costing about 7*s.* 6*d.* a day and horses about 5*s.* a day, the advantage of hiring casual labour for threshing, even at high rates of pay, will be obvious when it is remembered that the horseman whose horses are standing idle represents a daily cost for the manual work performed by him of some 18*s.* On a Midland Counties farm, where the maximum possible horse-hours in a certain week in November last were 238, the time actually worked by horses was found to be eighty-seven, owing to threshing operations, and the wastefulness of the labour-management in such a case is obvious. Again, employers in certain cases object to

paying Saturday overtime to men willing to work, because overtime payments are at a higher rate than those for ordinary time, but they overlook entirely the fact that the Agricultural Wages Board provides no overtime payments to the horses, and thus the cheapest horse-labour on the farm is that performed on Saturday afternoon at overtime rates of pay to the horsemen.

Everyone realises, of course, the importance of keeping horses busy, but not everyone thinks how heavily the cost of manual labour is increased by idle horses. The maximum number of working days in a year is 312, a total obviously impossible of attainment in practice. Such records as are available show that the days actually worked by horses on the farm will not usually exceed four-fifths of the maximum. More time may be lost in summer than in winter, a fact not generally realised, and the period of maximum unemployment falls between haymaking and harvest. The busy seasons are, of course, the autumn and the spring, when the preparation of the ground for winter and spring corn is going actively forward. In the year 1918 figures were collected to show the percentage of days worked compared with ‘ possible days ’ in each month on four farms distributed pretty evenly over England, and the results, thrown together, are as follows:—

PERCENTAGE OF DAYS WORKED TO POSSIBLE HORSE-DAYS ON FOUR FARMS IN 1918.

	%		%
January	67	July	38
February	82	August	65
March	77	September	78
April	74	October	80
May	70	November	67
June	56	December	64

Although the figures represent an average of four farms, it is noteworthy that the results on the individual holdings varied one from another in degree only, and that the months of maximum and minimum employment were the same in every case. The loss of time is far more serious than many people realise. The maximum possible horse-days in the year are 312, and the cost per day of the horses on the above four farms on this basis was 2*s.* 7*d.* whereas, owing to the time lost, the cost on the basis of days worked was 3*s.* 7*d.* Whilst some difference is inevitable, so great a discrepancy as these figures reveal can be avoided by skilful management, and one of the tests of the farmer's efficiency is provided by an examination of the distribution of horse-labour throughout the year on his farm. His cropping and other work should be so contrived as to provide for the uniform utilisation of horse-labour month by month. Under skilful management the differences in the number of days worked by horses from year to year are extraordinarily slight. On an East Midlands farm, employing twenty-three horses, the days worked per horse during the past six years have been as follows:—

Year	1913–14	1914–15	1915–16	1916–17	1917–18	1918–19
Days worked per horse	250·25	247	243	236	243	244·5

It may be noted, in passing, that figures such as those given for the seasonal employment of horse-labour emphasise the need for a study of the place of the agricultural tractor in farm management, for the busiest times of the year synchronise, more or less, with the seasons when the weather is more uncertain and suggest that the application of speedier mechanical power to field operations, in substitution for slower horse-power, would result in economic advantages in certain cases.

In connection with the study of economics on the farm the question of agricultural costings naturally suggests itself. Farmers, as a class, are not accountants and much less are they cost accountants, but this has not deterred many of them from taking part in discussions of farming costs which have been going on in the Press and in the Food Controller's offices for some time past, and the confusion of thought on the question of what cost of production really is which these discussions have revealed is evidence of the need for study and education in costing processes. Few things can be of greater service to the farmer than scientific book-keeping carried out and interpreted with proper understanding, but few things can deceive him more than costing wrongly conducted or misinterpreted. The need for accurate thinking is evidenced in nothing, perhaps, so much as in connection with the question of the valuation of the raw materials grown on the farm, the hay, straw, roots, pasturage, &c., produced for home consumption in the process of manufacturing milk and meat. There can be only one basis of value possible, namely, their cost to the farmer, but it is contended, almost universally, that their market price should be substituted for the sums that he has actually paid for them. As a matter of fact, the bulky feeding stuffs usually produced and consumed at home rarely have any market value at all. A market value is one that can be realised in the market. Thus, corn, meat, and certain other commodities have clearly market value because they are always saleable, but if all the farmers in the country decided to sell their mangolds they would find that the market for mangolds is non-existent, and that the prices quoted in market reports represent a few deals to satisfy an infinitesimal demand. The same is true of straw and, in a slightly less degree, of hay in normal times. Even if the difficulty of fixing the market prices of certain products, such as turnips, straw, or hay, be ignored, and if it be assumed that there be a free market in such things, a fuller consideration of what the farmer really does in feeding them to his stock will show how inapplicable such values are to his case. The market value of an article is the figure at which a willing buyer and a willing seller can agree to do business. The farmer who contends that he is justified in 'selling' his roots or hay to his stock is selling them, in point of fact, to himself, and seeing that there is only one party to the transaction there can be no market and, consequently, no market price. In the majority of cases each of these things is grown because the farmer has need of them in the production of the article or articles of food towards which his management is directed. If he could buy them more cheaply than he can grow them he would surely do so, but to regard himself as a merchant instead of as a manufacturer, and

then to trade with one department of his farm against another is to involve himself in paper transactions which have no foundation in fact, and which may lead to disastrous conclusions. To take, for example, the cost of milk production. It is usual to argue that hay consumed by the cow should be charged at its market price. It may well be that in consequence of a temporary or of a local demand it will pay a farmer better to sell hay rather than to produce milk, and one of the main functions of book-keeping is to enable him to make a decision on such points as this. But he cannot expect to have it both ways; if he sells hay he cannot produce milk, and *vice versa.* Many farmers contract at summer prices for their winter's supply of feeding stuffs, but a man who has bought linseed cake at a pound per ton less than the price current at the time when he is consuming it would hardly think of charging it to bullocks at any other price than that which he actually paid, and it is this figure, the actual cost to him, which must be the measure of the value of all raw materials, whether they be bought in the market, or whether, for the sake of convenience and economy, they be grown on the farm.

Lastly, I want to urge, and particularly before a gathering such as this, the importance of agricultural economics in agricultural education. The fact is realised, no doubt, by many teachers, but until a sufficient body of data bearing on the study of farm management can be made available to them it is impossible for them to give to the teaching of practical agriculture that solid economic basis which is fundamental, and the teacher is driven to include in his instruction much to which the economic test has never been applied and to exclude more for which no basis for teaching exists at all. Given the requisite body of information it would not only be possible but necessary to recast the whole foundations upon which the teaching of practical agriculture rests.

I am not one of the few who appear to derive satisfaction from making comparisons unfavourable to British agriculture with that of other countries, but, when we look at the work which is being done in the United States, in Italy, Germany, Switzerland, and even in Russia before the War, it is surprising to reflect that the agriculturists of the nation which produced Adam Smith, Ricardo, and John Stuart Mill should have been so slow to realise the need for a fuller organisation for the study of agricultural economics.

REPORTS ON THE STATE OF SCIENCE,

ETC.

Seismological Investigations.—*Twenty-sixth Report of Committee* (Professor H. H. Turner, *Chairman*; Mr. J. J. Shaw, *Secretary*; Mr. C. Vernon Boys, Dr. J. E. Crombie, Sir Horace Darwin, Dr. C. Davison, Sir F. W. Dyson, Sir R. T. Glazebrook, Professors C. G. Knott, *and* H. Lamb, Sir J. Larmor, Professors A. E. H. Love, H. M. Macdonald, *and* H. C. Plummer, Mr. W. E. Plummer, Professor R. A. Sampson, Sir A. Schuster, Sir Napier Shaw, Dr. G. T. Walker, *and* Mr. G. W. Walker). *Drawn up by the Chairman, except where otherwise mentioned.*

General.

The Committee has to deplore the death of Professor J. Perry, who had been associated with its work from the early days when he was in Japan with Professor John Milne. The first Report of this Committee (1896, Liverpool) contains an account of the 'Perry Tromometer' by its inventor, and a note on trials of one form of it at Shide by Milne—so sensitive that the guns fired five miles away (at the funeral of Prince Henry of Battenberg) caused movements of 1 foot in the light spot. Perry was continuously a member of this Committee from its inception till his death (1920, August 5), and a regular attendant at its meetings.

The clerical work at Oxford is still being carried on in the room in the 'Students' Observatory' mentioned in the last Report, as the tenant of the house (purchased by Dr. Crombie's benefaction) found at the last moment that his arrangements for vacating it in September, 1920, had broken down. This has naturally hampered the work; and the serious illness of Miss Bellamy for several months has also had an inevitable effect on the current reductions. The Committee is much indebted to her uncle, Mr. F. A. Bellamy, for the way in which he has minimised the loss by his own personal exertions.

Until the Royal Commission on the Universities of Oxford and Cambridge has reported, and the Geophysical Union has considered the international situation (as expected at Rome in April next), no further steps of a general kind can be taken.

The Committee received its annual grant of 100*l*. from the Caird Fund in January, 1921. In place of the 100*l*. formerly granted by the British Association at its annual meeting, it was resolved at the Cardiff (1920) Meeting to forward to the Board of Scientific and Industrial Research a recommendation that this sum should be granted from its funds. The Chairman interviewed the officials of the Board on the matter, at their request; and it was ultimately decided that, instead of the application to the B.Sc. and Ind. Res., application should be made to the Government Grant Fund for 300*l*. in place of the former 200*l*. This grant of 300*l*. was made in March, 1921, and placed at the disposal of the Committee in June, 1921.

It may be mentioned that a member of the Committee, Dr. C. Davison, has during the year published an excellent 'Manual of Seismology' (Camb. Univ. Press, Geological Series).

Instrumental.

The Milne-Shaw seismograph erected in the basement of the Clarendon Laboratory at Oxford has worked well throughout the year, except that the coal strike led to a diminution of gas-pressure and consequent illumination for certain hours, which made the record almost useless. Mr. Bellamy and Mr. J. J. Shaw arranged an electric-light substitute which has performed fairly well.

It may be mentioned that miniature copies of the films on quarter-plates have been found very useful. Prints made from these show under a lens practically all that is required, and are very readily stored or sent by post.

It would thus be feasible to collect the records of several observatories for comparison without taking up an inconvenient amount of storage space. We should at Oxford welcome exchanges of this kind (for days of large earthquakes) with observatories that would consider a mutual arrangement.

During the year Milne-Shaw machines have been despatched to Bombay, Rio de Janeiro, Wellington (N.Z.), Cairo, and Hong-Kong.

Mr. J. J. Shaw has been hard at work all the year on the construction of Milne-Shaw machines, and as each approaches completion he has seized opportunities for experiment. The most valuable and laborious of these experiments are referred to later under the heading of 'Microseisms,' but another instance is represented in the following note supplied by him :—

Wanderings of the Zero.

'In the Report for the year 1917 attention was drawn to the great difference in the stability of two adjacent sites 60 feet apart (at West Bromwich), but there was a possibility that the wandering of the zero was an effect of changes of temperature upon the instruments. The base of the seismograph is poised upon three brass feet, the effect of which might be to tilt the instrument if one side of the chamber was warmer than the other.

'To investigate this point three feet of invar steel were substituted, but the wandering of the zero from day to night continued as before.

'An attempt was made to counteract the tilt by using invar on one side of the instrument and brass on the other. If any effect was produced, it was too small to be noticed.' (J. J. S.)

Just as this Report was going to press, Mr. Shaw sent a further note of an important series of observations on the effect of solar radiation. 'Yesterday was a special day here,' he wrote on August 4; 'the sky was intensely blue, with huge banks of fleecy cumulus clouds, so that the front of my house was at one moment in brilliant warm sunshine, and at another in cool shadow.' He noted the times of transition, and found almost *immediate* responses of the seismograph in the cellar. The matter will, of course, be further investigated, and fuller details given later.

Change of Site from Shide to Oxford.

The departure from Shide (rendered necessary by the return of Mrs. Milne to Japan) and removal to Oxford involves discontinuity, which is liable to affect scientific results more or less, sometimes in details which are not realised

TABLE I.

NUMBER OF EARTHQUAKES REGISTERED.

	Shide				Oxford		
	1916	1917	1918	1919	1918	1919	1920
January .	5	6	1	2		2	2
February .	7	3	5	3		3	11
March .	4	4	3	3		2	4
April . .	9	6	2	7		6	2
May . .	6	4	5	10		10	10
June . .	4	6	2			5	9
July . .	2	13	7			7	6
August .	15	5	5			6	10
September .	6	1	3			10	17
October .	6	3	8		8	13	15
November .	9	1	10		7	8	6
December .	7	4	4		4	2	16
Total .	80	56	55			74	108

until too late to remedy the defect. The most noticeable change up to the present has been decidedly advantageous, viz. the steadiness of the trace has been immensely improved. This is probably not a consequence of change of

geological site, but merely of the installation in the basement of the Clarendon Laboratory, where the temperature is kept very steady (whereas in the old stable on ground-level at Shide it varied considerably : see the Twentieth and Twenty-first Reports). But in the present imperfect state of our knowledge it is well to keep an eye on anything which may suggest change in sensitiveness of site. The counts of the numbers of earthquakes registered at Shide and Oxford near the break, given in Table I on page 207, seem to show that no very serious discontinuity of this kind has been experienced. Miss Bellamy went through all the records and noted any disturbance of the trace, however slight, that could be attributed to an earthquake.

Bulletins and Tables.

The Bulletins are now considerably in arrear, owing to the imperfection of correspondence during the War, which is only slowly disappearing. Several times the results for 1917 have had to be revised owing to the receipt of delayed material; but they are now being put into shape for printing, which will be pushed on (for this and following years) as rapidly as possible. Any corrections to tables must necessarily await the completion of this work.

Earthquake Periodicity.

It was mentioned in the last Report how the long period of 240 to 300 years suggested by the Chinese records of earthquakes had perhaps been identified in the growth of trees as exemplified in the records collected by Mr. A. E. Douglass. If so, there were apparently two interfering periods of 284 and 303 years. During the past year, by the courtesy of the Meteorological Office, a copy of the independent work by Professor Ellsworth Huntington, 'The Climatic Factor as illustrated in Arid America,' was lent for some months in order that the measures there given might be analysed. They were accepted as given by Professor Huntington in Table G on p. 323, which gives a summary of growth of *Sequoia Washingtoniana.* Column (H) gives final corrected average. What follows refers to this column subsequent to date — 1085, before which the numbers are wild and would seriously upset the analysis.

(*a*) When analysed in an adopted period (adopted for convenience of arithmetic) of 280 years the phases showed a progressive increase, indicating a longer *mean* period, but at the same time a reversal in the middle of the series showed that a single term would not suffice to represent them completely.

(*b*) A period of 284 years was then assumed and separated out. The remainders indicated a term, *not* of 303 years as expected, but of 327 years. Since the values 284 and 303 for the components were adopted from discussion of the corresponding pair near one century (95 and 101 years), this new series was next examined for cycles near one century, and a term of 109 years declared itself unmistakably.

Hence it will be necessary to reconsider the former adopted values, and to see how far the whole evidence will support modified values of the periods.

Breaking Submarine Cables.

It was Milne's opinion, several times publicly expressed, that submarine earthquakes were often responsible for breakages of cables, which occasionally occur without assignable cause. If so, we should have an important link between a scientific study and the business world.

During the past year opportunities have arisen in several independent ways for testing this hypothesis. In some cases definite dates and places of cable breakages were supplied, with inquiries whether shocks fitting in with these data had been recorded. In no case could an affirmative answer be given after scrutiny of the records; while in some of them the trace seemed to be almost maliciously quiet for many hours near the date and time provided. After some experience of this kind other inquiries were initiated by the Chairman without better success. The cable companies concerned do not wish the details published, for business reasons; but the main facts are as stated. It would seem that if submarine shocks of the kind are responsible, then for some reason they do not affect our seismological records.

Explosions.

During the past year also there have been one or two explosions of which notice has kindly been given, so that effects on the trace might be looked for. In these instances previous experience suggested a negative result, and no serious expectations were raised. There was ultimately nothing to report.

Standard Time.

This Committee in its earlier years naturally took much interest in the establishment of Standard Time in various parts of the Empire. The Colonial Office courteously continues to inform the Committee of any advances in this direction. Thus in 1919, August, the Colonial Secretary informed us that the 'Standard Time of one hour fast on Greenwich will be introduced in Nigeria on the 1st of September next,' forwarding at the same time a copy of the Ordinance. On 1920, January 2, the Colonial Secretary forwarded copies of Ordinance No. 18 of the Gold Coast, No. 11 of Ashanti, and No. 8 of the Northern Territories, which enacted that Standard Time shall be twenty minutes in advance of Greenwich from September 1 to January 1 in each year; and Greenwich time for the rest of the year.

Alterations of twenty minutes at various seasons formed part of Mr. Willett's original scheme of 'Daylight Saving,' but were given up in deference to urgent representations from various quarters. As an alteration of this kind seems far more likely to cause inconvenience and confusion than an alteration of a whole hour, an inquiry to the Colonial Office has recently been adventured whether it would be possible to find out how far experience of the change had proved satisfactory; and an answer was received promising that inquiries should be made. On August 12 the reply was transmitted, to the effect that the changes had been found so beneficial in practice that they were to be retained.

The Earthquake in New Guinea on 1919, May 7.

Acknowledgment may be also made here of the courtesy of the Colonial Office in forwarding, under date 1919, September 10, a copy of a Report from the Administration at Rabaul (late German New Guinea) relative to a 'severe earthquake shock' on 1919, May 7. 'Reveille had blown at 5.30 . . . and a few men who were snatching some minutes' extra sleep received a violent reminder that it had blown by being pitched off their stretchers and having the stretchers overturned on top of them.' In acknowledging the Report, opportunity was taken to ask for details of locality, which led to a further Report from the Administrator, received here on 1921, January 4, specifying the centre of disturbance as the 'semi-active volcano Glaie or Tavurvur,' in long. 152° 8′ 55″ E., lat. 4° 14′ 20″ S. (2 miles S.E. of Rabaul), which was in violent eruption in 1878 (January-February), when (February 14) Vulcan Island was upheaved (ref. to autobiography of Rev. George Brown, and R. Geog. Soc.; chart of H.M.S. *Blanche*, 1872). At that time there was little or no white settlement. There was another severe shock on 1916, January 1, for which the details adopted (by Shide, I.W.) were

$$T_0 = 13^h\ 20^m\ 13^s, \quad . \quad . \quad 154^\circ{\cdot}0\ E. \quad 5^\circ{\cdot}5\ S.$$

Possibly the actual position of the volcano would suit the data equally well (see 'Large Earthquakes' of 1916, published by this Committee). The Administrator adds :—

'The line of disturbance is South-West from the volcano Glaie to the large active volcano called the Father on the north coast of New Britain. In fact, the earthquakes are most severe when the Father is quietest. The line extends then westerly towards the west end of New Britain, where there are semi-active volcanoes; thence on to the Island of Manam, off the coast of New Guinea, which is a very active volcano.'

The Great Earthquake of 1920, December 16.

A very severe earthquake occurred on December 16 near the city of Pingliang, in Kansu, China. Father Gherzi has already published a valuable preliminary report on this disaster, but the following notes are chiefly from sources inde-

pendent of his: 'The reported loss of life varies from 1,000,000—a Chinese official report—to 100,000, a "conservative" foreign estimate.' Part of the population live in caves, and were buried alive by the collapse of the hills. Others sleep on brick platforms with a fire underneath, and were either burnt in the fire or died of cold and exposure from the fire being extinguished. Letters have been received (in reply to inquiries kindly suggested by the Royal Geographical Society) from a number of missionaries and others, giving details of the terrible disaster in various localities. A valuable report from Mr. E. J. Mann in Lanchow (103°.9 E., 36°.0 N.) deserves special mention. He gives a sketch map of the district most affected which extends from

Haicheng (105°.95 E., 36°.35 N.): 'No walls left; no people left';
Ta-la-Chih (105°.36 E., 36°.62 N.): 'Important large market; entirely destroyed'

(these as northern limit) to

Tonguei (105°.12 E., 35°.22 N.: 'No wall standing; not quite so flat as Haicheng';
and Ma-ing (104°.97 E., 35°.30 N.): Same note as Ta-la-Chih.

The mean co-ordinates of these four places are 105°.3 E., 35°.9 N., and, so far as it is possible to specify a single point for a disaster of such magnitude, we might take this for the epicentre. Mr. Mann draws attention to two volcanoes N. and S. of the district most affected. The positions given above are taken from his sketch map by comparison with a few well-known points: and the positions he records for the volcanoes are

°	°	Mean
105·79 E	37·26 N	105°·24 35°·95
104·69 E	34·64 N	

The point midway between them is, as Mr. Mann observes, remarkably close to the middle of the affected district, as above estimated. The northern volcano 'does not emit fire but is always smoking.' Close to the southern, which is also a 'smoking hill,' is a 'noted boiling-water spring to which people go for medical baths.' Between the hills are a 'large number of warm or hot springs. There is a real hot one close to the ruined city of Tonguei, and there are many warm ones in the district of Ching-yuan' (105°.08 E., 36°59 N.).

Other points of interest in Mr. Mann's letter may be briefly summarised thus:—

(A.) Percentage of houses destroyed at

	°	°	°	
Ching-yuan . . .	105·08 E.	36·59 N.	Δ 0·78	80 to 90 per cent.
Ching-ning . . .	105·59	35·41	0·60	80 "
Ku-yuan . . .	106.24	35·65	0·90	60 to 80 "
Tsinchow . . .	105·15	34·49	1·40	50 "
Huei-ning . . .	105·03	35·67	0·28	40 "

The distance Δ from the epicentre deduced above is estimated from the map. The percentage for Huei-ning suggests that the epicentre should be moved further away from it, probably to the East, which would bring it nearer Ku-yuan. This is in accordance with the rough shading on the map by which Mr. Mann has indicated the devastated area. Perhaps 105°.8 E., 35°.8 N., would be a better estimate, taking everything into account. Father Gherzi's contour lines suggest 106°.1 E., 35°.6 N.

(B.) Black and evil-smelling water was vomited in many places; the earth opened in others. [In the subsequent quake of December 25 a man fell into an opening up to his middle, which then closed and smashed his legs.] In several places long strips of land subsided 20 or 30 feet.

(C.) At Tsinchow (105°.0, 34°.4) is a stone tablet commemorating the rebuilding of the walls after an earthquake when half the people were killed. (Date not yet communicated.)

(D.) The barometer fell heavily before each bad earthquake; for that of December 16 was followed by a series of others, which, at the time of writing

(1921, March 17), were still continuing. By an odd coincidence a letter was received from Mr. A. Pearse Jenkin, F.R.Met.S. (of Redruth, Cornwall), suggesting, from an independent standpoint, a connection between earthquakes and barometer changes. He draws attention to p. 226 of Symons's 'Meteorological Magazine' for 1906 (vol. 41), where there are some notes by Mr. W. Gaw on the Chili earthquake.

(1) The third and second days previous to the great shocks were 'characterised by a high barometer, accompanied with rain; abnormal conditions here. (2) The day preceding the first seismic movement was marked by a sudden fall of about half an inch of barometric pressure in a comparatively short period of time.'

Mr. Jenkin's view is that 'at some spot on the earth there exists, from some exceptional cause, an area of deficiency of mass in the earth's crust, and there is, according to the theory of isostasy, an endeavour to fill up the deficient area and so restore the balance. The interior of the earth, being viscid, responds but slowly, but the atmosphere, subject to the same laws but more fluid, does its part in attempting to restore the balance more rapidly.' He does not make his mechanism entirely clear, but neither is the mechanism of isostasy yet fully understood.

Location of the Epicentre: Early Uncertainties.

It is perhaps well to put on record the uncertainties which affected the localisation of this great earthquake for some days. A single completely equipped station, such as Eskdalemuir, can assign the locality of an epicentre from its own records; but unfortunately the Eskdalemuir machines were out of action on December 16. Other English stations have as yet only partial equipment: taking them in combination, they could assign two alternative localities, one to the East and one to the West. News from America that the epicentre was only 3,000 miles from them pointed to the Western alternative, but it was very difficult to fulfil all the conditions. The direct evidence of the seismographs put the epicentre where a disaster of such magnitude would be independently recognised; it was necessary to look for a possible centre, not too far from that directly indicated, where a big earthquake might occur without revealing itself by telegraph to the civilised world—such as the Alaskan Coast, for instance. But the receipt of news from China made it clear that the American stations had been deceived by the magnitude of the disturbance into thinking it close at hand, when it was in reality far away.

A few Details for December 16.

The earthquake of December 16 was so exceptional that a few figures may be given here. The adopted epicentre (calculated before Mr. Mann's map was received) is 105°.5 E., 35°.5 N., and the time at origin $T_0 = 12^h\ 5^m\ 46^s$.

Station	Δ	Azim.	Obsd. P		O—C	Obsd. S.		O—C
	°	°	m.	s.	s.	m.	s.	s.
Calcutta	19·8	236	10	30	+ 5	14	12	+ 5
Kodaikanal	35·9	233	13	6	— 1	—		—
Vienna	63·8	312	16	19	— 4	24	58	+ 1
Padova	67·5	311	16	49	+ 2	—		—
Dyce	69·2	327	16	58	0	25	58	— 4
West Bromwich	71·8	323	17	14	— 1	26	29	— 5
Oxford	71·9	322	17	15	0	26	31	— 4
Riverview	81·5	143	18	15	+ 2	28	28	+ 2
Sydney	81·5	143	18	12	— 1	28	30	+ 4
Honolulu	82·8	70	18	24	+ 3	28	42	+ 1
San Fernando	84·1	312	18	27	— 2	28	36	—19
Saskatoon	88·0	21	18	39	—12	29	19	—19
Ottawa	99·1	1	19	27	—25	30	6	—87
La Paz	160·1	—	25	58	—	40	10	—

Disturbance at Colombo, Ceylon.

The following note was forwarded by Mr. Bamford on 1921, June 22 :—

'The shock of December 16, 1920, gave the seismograph zero a permanent shift of $8\frac{1}{2}$ mm., or about 5″, the West end of the pillar being raised. A print of the corresponding records is enclosed. Two West to East levels, one on the seismo-pillar, 15 feet deep, the other on the pillar of the new transit instrument, 2–3 feet deep, were available for examination. Their curves are shown (Curves I. and II.). The plotted points are the means of three readings, approximately at 8 A.M., noon, and 4 P.M. (I.S.T.), except on the 12th and 19th (8 A.M. readings only), and on the 18th (8 A.M. and noon readings only, for the transit level). The curves run fairly parallel, except on the 16–17. We may therefore take the effect of the earthquake on the seismo-pillar to be a shift of rather over 1″.

'The North to South level on the seismo-pillar (Curve III.) shows no effect at all, unless it is masked by a simultaneous shift due to meteorological changes, such as frequently occurs. In this connection it may be mentioned that the daily period of the seismo-pillar (the East to West component of which used to be 5″, the North to South component much smaller) was considerably reduced last year by the addition of verandahs to the North and South of the seismograph room, and by the addition of a new transit room to the East, so that the seismograph room is no longer at the extreme East of the Observatory building.'

The three curves forwarded by Mr. Bamford certainly all show a sympathetic drop on December 17, and a sympathetic rise from about December 18.5 to 20.0. Outside these limits their trends are unsympathetic and even opposed. It is possible that the movements between December 17–20 are due to the earthquake, but large changes of a seismograph trace may be due to creep of the boom needle point in its cup, which in the Colombo (Milne) instrument is hemispherical.

Disturbance at Oxford.

On the other hand, we are fortunate to have a remarkable observation at the Radcliffe Observatory, Oxford, kindly communicated by Dr. Rambaut. It appears that Mr. W. H. Robinson happened to be observing the level-error of the Transit Circle at the time of the earthquake, and after making a setting 'was astonished to see the reflected image of the wires slowly move away until it reached a distance of about 4″; it then as slowly returned to the direct wire, repeating this many times during the half-hour's watching from 12h. 55m. to 13h. 25m. At times the amplitude of the oscillation was rather greater than 4″, possibly 5″. There was a complete absence of tremor or quick vibration and diffusion such as those ordinarily observed, caused at certain times by the engine of the University Press, and occasionally by passing heavy traffic.'

The extreme readings of the R.A. micrometer were 21.277 and 21.333; on December 17 and 18 the mean readings were 21.356 and 21.353, agreeing nearly with one extreme, and certainly not lying between them. The suggestion is that 'the oscillation took place in such a way as to *lift* the Eastern pivot relatively to the Western by about 0.0006 in. (pivots 50 in. apart).' The observations were communicated by Dr. Rambaut to the 'Monthly Notices' of the Royal Astronomical Society in 1921, April, but the facts were set down on 1920, December 30.

Telegraphic Transmission of Earthquake News.

It is not often that rapid communication of seismological observations is seriously needed. For most earthquakes leisurely transmission by post is quite sufficient, for the accurate determination of an epicentre is best undertaken when all the information has been collected. As regards preliminary determinations, they can often be made from a single completely equipped station or from one or two stations near together, in friendly communication by postcard. The quake of December 16, however, drew attention to the desirability of having in reserve some means of telegraphic communication. Inquiry was

therefore made of M. Lecointe, of Brussels (who was deputed by the International Astronomical Union to deal with astronomical telegrams), whether a supplementary seismological service could be arranged. [It may be remarked that, although an International Geophysical Union was constituted at Brussels in 1919, at the same time as the Astronomical Union, it was not found possible to initiate the seismological branch of it until the formal dissolution of the former International Seismological Association had been completed. Hence the astronomical body was approached as the best available means under the circumstances.] M. Lecointe replied that a telegraphic code had already been arranged at Strasbourg, which Professor Rothé kindly communicated to me. Since then we have exchanged various telegrams in this code, with advantage, at any rate, to us. For instance, the news from America about the quake of 1921, February 4, was again misleading, but the wire from Strasbourg prevented our being perturbed by it. Inquiry has been made whether exchanges cannot be arranged across the Atlantic in this code, and negotiations are still proceeding in the matter. Meanwhile a further step has been taken in the dissemination of Strasbourg intelligence from the Eiffel Tower. Of this we have not as yet been able to take advantage, partly because of the absence through illness of Miss Bellamy, and partly because there have been few large earthquakes recently.

The following are the particulars of the code :—

Telegraphic Code for Earthquakes.

dd/aa/p p/hh/mm ss/ttt D_1D_1DDD.

dd = date.

aa = azimuth of epicentre from 10° to 10°, counting from N. through E. (01–36) based on any *clear* indications of the trace. The addition of 50 (51–86) indicates that the azimuth is uncertain by $\pm$ 180°. The figures 91–98 indicate that the direction is vague and estimated only to nearest 45°; 99 indicates that no azimuth determination has yet been made; 00 that it seems impossible.

pp refer to phases P and S; the first (1–4) concerns P, and second (5-8) S, according to the table below. The figure 9 for either P or S indicates that the minute signal interferes with beginning.

hh mm ss are hours minutes and seconds of P.

ttt is difference S—P in seconds.

D_1D_1 gives difference P—P in seconds for close earthquakes; if this difference is not clear, D_1D_1 is replaced by 99.

DDD is distance in kilometres for close quakes.

D_1D_1DDD is distance in kilometres for distant quakes.

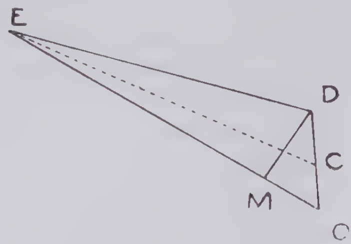
Phase P	1	2	3	4
	*i*P	P & $\overline{\text{P}}$ clear	P	*e*P
Phase S	5	6	7	8
	*i*S	S	*e*S	Uncertain

Azimuth of Epicentre from two adjacent Stations.

In the course of trying to identify the epicentre of the great earthquake of 1920, December 16, a small point of procedure suggested itself which it may be useful to note.

Suppose we have two stations, say O(xford) and D(yce), whose distances EO and ED from the epicentre E are known. We can describe circles on the

globe with O and D as centres, intersecting in two points one of which is E. But drawing these circles on a globe is not a very accurate operation, and it is convenient to calculate, if it can be done quickly, in what direction outwards from O or D to look for the epicentre.



$$\text{Let EO}-\text{ED}=2x: \text{ and put EO}+\text{ED}=2\Delta$$

Then $2x$ must be less than the distance OD (say $2d$) between the stations; and approximately $\text{OM}=2x$, $\text{OD}=2d$, so that $\cos \text{EOD}=x/d$, which gives approximately the angle made by EO with OD.

The point to which attention is here called is that this simple equation (which is, moreover, independent of Δ and thus readily tabulated) gives with considerable precision the angle ECD, where C is the midpoint of OD. This is true either for spherical or plane geometry. Consider first the latter. We have

$$2\,\text{EC}\,.\,d\,.\,\cos \text{ECD}=\text{EC}^2+d^2-\text{ED}^2=\text{EO}^2-\text{EC}^2-d^2$$

$$\therefore\ 4\,\text{EC}\,.\,d \cos \text{ECD}=\text{EO}^2-\text{ED}^2=4\Delta x$$

and

$$2\,(\text{EC}^2+d^2)=\text{EO}^2+\text{ED}^2=2(\Delta^2+x^2)$$

$$\therefore\qquad \cos \text{ECD}=\frac{x}{d}\,.\,\frac{\Delta}{(\Delta^2+x^2-d^2)^{\frac{1}{2}}}$$

Since the equation is only likely to be required when x and d are small compared with Δ, the approximation is close.

For spherical geometry the equations are

$$\sin d\,.\,\sin \text{EC} \cos \text{ECD}=\cos \text{ED}-\cos \text{EC}\,.\,\cos d=\cos \text{EC}\,.\,\cos d-\cos \text{EO}$$

$$2 \sin d\,.\,\sin \text{EC}\,.\,\cos \text{ECD}=\cos \text{ED}-\cos \text{EO}=2 \sin \Delta\,.\,\sin x$$

$$2 \cos d \cos \text{EC}=\cos \text{ED}+\cos \text{EO}=2 \cos \Delta\,.\,\cos x$$

Thus

$$\cos d \sin \text{EC}=(\cos^2 d-\cos^2 \Delta \cos^2 x)^{\frac{1}{2}}$$

and

$$\cos \text{ECD}=\frac{\tan x}{\tan d}\,.\,\frac{\sin \Delta}{(\cos^2 d-\cos^2 \Delta \cos^2 x)^{\frac{1}{2}}}$$

$$=\frac{\tan x}{\tan d}\,.\,\frac{\sin \Delta}{(\sin^2 \Delta \cos^2 x+\sin^2 x-\sin^2 d)^{\frac{1}{2}}}$$

where the approximation is clearly of the same order as before.

It may be convenient to use a flat projection of the sphere. Thus we may take O as the centre of a gnomonic projection, so that a circle of radius r round O is projected into a circle of radius $\tan r$ on the flat. The angles round C will then be projected angles, but if C is not too far away from O, the error made in setting them off uniformly round C will not be large.

There is another method of finding the points of intersection E of the two circles, which may be useful as a check on the former. Let the constants for O and D (as given on p. iii of the 'Large Earthquakes for 1916') be $(a_1\ b_1\ c_1)$ and $(a_2\ b_2\ c_2)$: and let E be denoted by (A, B, C). Then

$$\cos \text{EO}=a_1\text{A}+b_1\text{B}+c_1\text{C}$$

$$\cos \text{ED}=a_2\text{A}+b_2\text{B}+c_2\text{C}$$

Let k denote the ratio of cos ED to cos EO, which is not very different from unity. Then

$$(ka_1-a_2)\mathrm{A}+(kb_1-b_2)\mathrm{B}+(kc_1-c_2)\mathrm{C}=0$$

Considered as a relation between (A, B, C) this represents a great circle on which (A, B, C) must lie. It has also the property of being at right angles to the great circle joining OD. For the pole L of this circle has co-ordinates

$$b_2c_1-b_1c_2,\ c_2a_1-c_1a_2,\ a_2b_1-a_1b_2$$

and these satisfy the above equation, which therefore represents a great circle through L, and consequently perpendicular to OD.

We can find the point K at which this line cuts OD from the relation

$$\cos \mathrm{KD}=k \cos \mathrm{KO}$$

Since KD=KO—$2d$, this becomes

$$\cos 2d+\sin 2d \,.\, \tan \mathrm{KO}=k$$

or

$$\tan \mathrm{KO}=(k-\cos 2d)/\sin 2d$$

so that KO can be readily tabulated in terms of k for a given pair of stations. Or the positions of K for given values of k can be marked off on the projection.

Microseisms. [*By* J. J. Shaw.]

In the Report for 1920 particulars were given of some investigations made upon microseisms, and it was there shown how it was possible at stations two miles apart to identify the individual waves so precisely that there was little difficulty in determining the time of arrival at each station to within a fraction of a second.

It was therefore proposed to extend the experiments, using three stations situated about ten miles apart, with the object of measuring more accurately the rate of propagation, and to confirm the previous observation that their direction was consistently from the North.

The 1920 experiments were conducted at West Bromwich.

Inquiries were made to discover two capable observers,

(*a*) at suitable distances from West Bromwich,

(*b*) with cellars or other accommodation which would provide a stable site for the instruments,

(*c*) provided with telephones for synchronising the time circuits.

By the kindness of Mr. Harry Walker, of Sutton Coldfield, and his brother, Mr. Sidney Walker, of Solihull, who gave much time to the work, the above conditions were fulfilled; and the thanks of the Committee are here placed on record for their valuable help in the experiments.

The instruments used were three Milne-Shaw seismographs, timed by three clocks with rates of about 1 sec. per day.

Each time circuit was provided with an audible 'clicker,' which could be placed near the telephone and heard at West Bromwich each minute. By this means the time breaks were either synchronised, or the difference (if small) observed.

One machine was installed at Sutton on 1921, January 18, and the other at Solihull on 1921, February 28; and both were oriented on a line 12° East of North to conform to the only convenient orientation at West Bromwich.

All machines were used with 10 sec. period; 20 : 1 damping; and a magnification of 250 : 1.

From the outset it was noted that it was not only quite impossible to identify individual waves, but even the trains of waves would not bear comparison.

On February 28 West Bromwich and Sutton machines only were running. They showed an exceptional series of torpedo-shaped maxima for many hours; but it was rarely that the maxima were in agreement at the two stations, and then obviously by mere chance.

During February, March, and April some 150 records were taken, but beyond the average amplitude and period there was no satisfactory agreement.

In the case of 1920 experiments the outlying station was two miles distant in a direction 17° West of North.

The Sutton position lies 8·2 miles distant and 66°·5 East of North. Solihull, 10·6 miles, and 134° East of North.

Sutton is 10·6 miles from, and precisely due North of, Solihull; only 17° from parallel to the two-mile line of 1920.

It seems remarkable that the microseisms, so perfectly reproduced at a distance of two miles, should so completely change in eight to ten miles.

It is worthy of note that the two near positions used in 1920 were situated upon a narrow outcrop of Permian Sandstone and Marls, whereas Sutton and Solihull are located upon a bed of Keuper Marls, and a geological fault divides them from this Permian strata.

If this discontinuity in the underlying rocks is responsible for the change shown in the microseisms, it might be possible to locate fault lines in this way.

The earthquake which occurred South of Mexico on March 28 was recorded on the West Bromwich and Sutton machines (Solihull not running). The seismograms were quite similar in all the main features, any differences were in the tiny superimposed waves (probably microseisms) and occasionally small differences in amplitude, the excess being sometimes on one machine and sometimes on the other, notwithstanding the damping ratio was 20 : 1 and the periods the same.

To Assist Work on the Tides.—*Report of Committee* (Professor H. LAMB, *Chairman*; Dr. A. T. DOODSON, *Secretary*; Colonel Sir C. F. CLOSE, Dr. P. H. COWELL, Sir H. DARWIN, Dr. G. H. FOWLER, Admiral F. C. LEARMONTH, Sir J. E. PETAVEL, Professor J. PROUDMAN, Major G. I. TAYLOR, Professor D'ARCY W. THOMPSON, Sir J. J. THOMSON, Professor H. H. TURNER). *Drawn up by* Dr. A. T. DOODSON, *Tidal Institute, University of Liverpool.*

§ 1. The Committee was appointed to investigate the degrees of accuracy obtainable in the analysis and prediction of tides. A great deal of work on tidal records has now been done under the superintendence of the Secretary at the Tidal Institute, and some definite conclusions on the subjects of reference have now been arrived at. These are restricted to short-period tides.

(1) On the basis of previous methods of harmonic analysis and prediction the errors of prediction for certain British stations may amount to more than a foot, apart from errors due to the use of predicting machines. At these stations the range of tide is not exceptional.

(2) About half of this error may be due to the inadequate treatment of shallow-water effects.

(3) The remaining half of the error is due to tidal constituents which are not included in the schedules given by Sir G. H. Darwin in 1883, and whose origin is not definitely known.

(The constituents scheduled by Sir G. H. Darwin will be spoken of as '1883' or 'Darwinian' constituents.)

(4) While the methods of analysis and prediction are restricted as hitherto to the consideration of the '1883' constituents only there can be no material improvements in either analysis or prediction. This is a direct consequence of (3).

(5) Time devoted to the modification of harmonic 'constants' by repeated analyses would probably be better spent in analysing for new constituents.

These conclusions are based upon the work of which an account is now given. The Report is divided into three parts :—

I. A general account of procedure and results.

II. A statement of methods. (Some of these may be of interest to those not concerned with tidal applications—*e.g.* the methods of calculation and summation of harmonic terms.)

III. A report on the behaviour of predicting machines.

The subjects of reference have not been fully investigated; the long-period tides and meteorological effects have yet to be considered, and the residues mentioned in (3) above need thorough examination. The Committee, therefore, asks to be reappointed.

PART I.

General Account of Procedure and Results.

§ 2. Some indirect evidence as to the errors of analysis and prediction is furnished by a comparison of observations and predictions such as is contained in the 'Report on the Harmonic Prediction of Tides' by the Secretary in 1920. But such evidence is not sufficient to indicate the causes of the discrepancies, and the only satisfactory course at the outset seemed to be that of the continuous subtraction of such partial tides as could be determined, together with examinations of the successive residues. Obvious tidal constituents, or such constituents as were indicated in the standard schedules (or otherwise), were to be removed and the residues successively treated until there would be nothing left save weather effects and other non-periodic perturbations of mean sea-level.

Success depended upon the accuracy of the observations to be treated, and upon the accuracy of calculations. Concerning the former it was concluded

that no observations were better than those taken by the Ordnance Survey, and the situation of one of their stations (Newlyn, on the coast of Cornwall) was extremely favourable for the investigations because of its situation with respect to the Atlantic Ocean. The Survey kindly placed at the disposal of the Committee the records they had taken, and the accuracy of these was greatly appreciated.

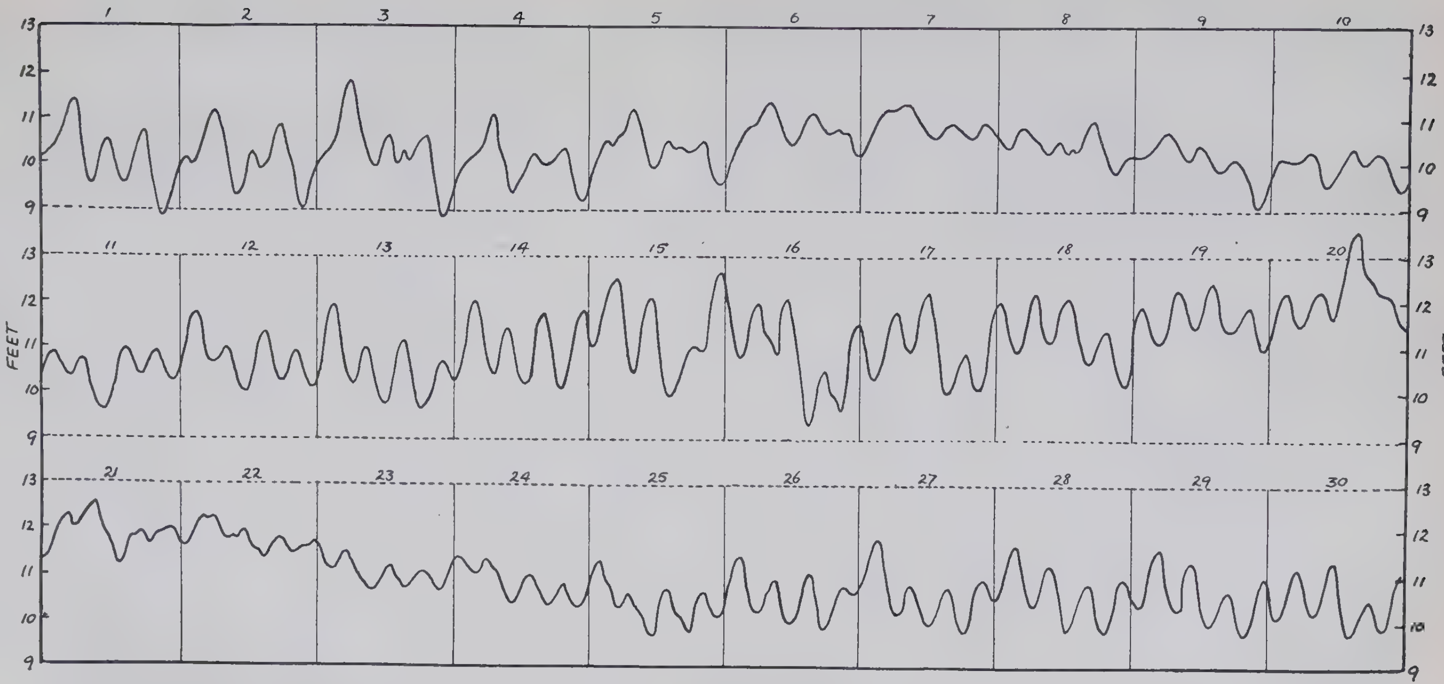
The powers of the predicting machines in summing the harmonic terms required were duly considered, but the evidence given in the Report for 1920 was sufficient for the machines to be distrusted for this work. Tests of two predicting machines have been carried out, and the results show that even with very careful reading the errors are too great for their use in calculating hourly heights; the labour of reading the curves is also very great. The results of the tests are discussed in Part III.

§ 3. The investigations were made possible by the invention of a scheme for the numerical calculation and summation of the harmonic constituents; this scheme very greatly reduced the labour of calculation, and the results of summation of one set of constituents could be relied on to within about 0·01 foot. An account of this scheme is given in Part II., §§ 11-13.

The first procedure was to remove the chief semi-diurnal constituents, or a first approximation to them. Examination of the analyses at neighbouring ports indicated the constituents M_2, S_2, N_2, K_2, and L_2 as most worthy of consideration. These were evaluated by certain inference methods (Part II., § 10) to a fairly good degree of approximation except in the case of M_2, which was modified after the residue for one month had been obtained. Application of the scheme for the calculation and summation of harmonic constituents to these five constituents, and subsequent subtraction of the partial tide, resulted in a residue which was mainly quarter diurnal. The residue for the month of January, 1918, is given in fig. 1. There is obviously some semi-diurnal residue, as was to be expected, since only a first approximation to the semi-diurnal tide was removed. There is also some diurnal tide, but this is not so prominent as the quarter-diurnal tide, and attention was first paid to the latter.

§ 4. Now the quarter-diurnal tide does not correspond directly to the generating potential, as the quarter-diurnal constituents of the potential are very small. It is well known, however, that as a wave progresses in shallow water it changes shape and the front slope becomes steeper than the rear slope. If the departure from sinuity be not too great, this change in form can be expressed by the addition of waves whose speeds are multiples of the speed of the primary wave. Theoretical considerations of a wave in a shallow canal have suggested that the quarter-diurnal constituents must have speeds which are either twice those of the primary semi-diurnal constituents or are equal to the sums of pairs of those speeds. The constituents usually analysed for are M_4, MS_4, and S_4, with speeds respectively equal to twice the speed of M_2, the sum of the speeds of S_2 and M_2, and twice the speed of S_2. Now, on carrying out analyses by the usual methods[1] for these constituents, and on subtraction of the partial tide compounded of them, it was found that they were quite inadequate to account for the whole, or a reasonable part of the whole, of the quarter-diurnal tide. Considerable attention was therefore directed to the matter, as it was known that these shallow-water effects were undoubtedly responsible for a large part of the errors in predictions. More constituents, as suggested by the theory mentioned above, were analysed for and calculated, but the rate of elimination was rather slow. It was ultimately found that there existed a very simple relation between the existing quarter-diurnal tide and the square of the semi-diurnal tide; a reduction factor and change of phase, applied to the quarter-diurnal portion of the square of the semi-diurnal tide, were sufficient to account so well for the quarter-diurnal tide that only a trace of it was left; this was possibly due to the incomplete (or approximate) semi-diurnal tide taken. This method was developed, very simple numerical formulæ were applied, and the quarter-diurnal constituents removed *en bloc.*

[1] For a *résumé* and criticism of these methods reference should be made to the Report for 1920 by Professor Proudman.



Newlyn. FIG. 1. *January*, 1918.

Residue after removing chief semi-diurnal constituents.

§ 5. Tests have been made for Liverpool and Hilbre Island, and similar methods can be applied successfully to the sixth-diurnal tide as well as to the quarter-diurnal tide.

(The methods of analyses for, and removal of, the quarter-diurnal tide are discussed in Part II., §§ 14-15.)

§ 6. After removing the quarter-diurnal tide the residue is mainly diurnal and semi-diurnal, as may be seen from fig. 2.

Inference methods for the chief diurnal constituents not proving very successful, resort was made to direct analyses of the residues, both for diurnal and residual semi-diurnal constituents. A summary of the usual methods of analysis, as applied to observations, is given in Part II., §§ 16-17, and modifications of these for use with residues are afterwards discussed in § 18. The Report by Professor Proudman (1920) shows that the chief errors in the analysis of observations are due to the presence of large constituents, so that one constituent is imperfectly isolated. The chief constituents having been practically removed, modifications in analysis were possible. The usual method is to analyse only for expected constituents, and the resulting numbers are taken on trust; the methods given in § 18 do not assume the absence of perturbing or of unknown constituents, but were designed so as to give unmistakable indications if such be present. Further, they give valuable internal evidence as to the amount of trust to be associated with any derived 'harmonic numbers.' This cautious procedure had its reward, as the results indicated that the residue contained constituents not included in Darwin's schedules. For a full discussion of these matters reference is necessary to §§ 17-18 and figs. 7-9. It was not found possible, using only six months' residues, to deduce the unknown constituents from these analyses, nor would it have been advisable to draw definite conclusions at this stage. Our sole index of reality lay in the residues. If, after taking out all the 'Darwinian constituents,' there were unmistakable signs of semi-diurnal constituents still remaining, then this would be regarded as sufficient proof of reality. On these coasts the diurnal tide is small, and no new diurnal constituents were suspected or found.

The removal of the diurnal and semi-diurnal Darwinian constituents indicated by the analyses was carried out with the results shown in figs. 3 and 4.

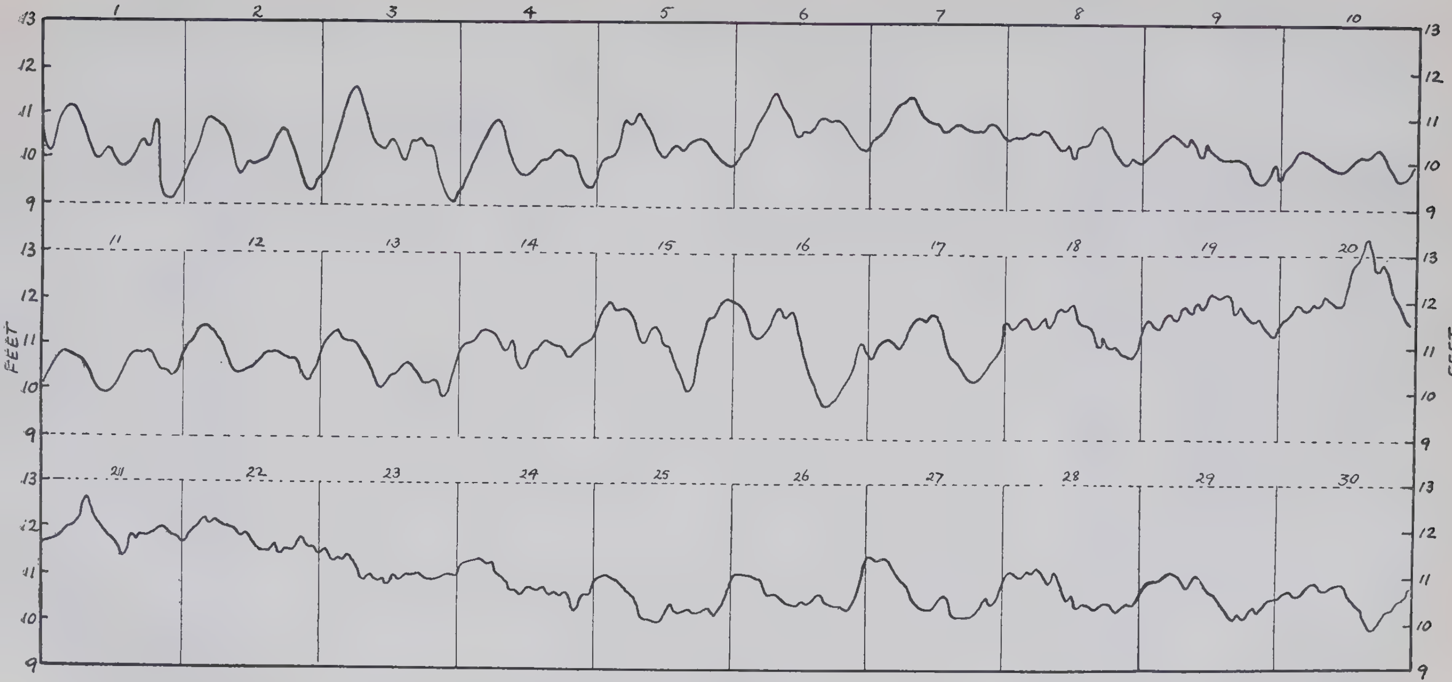
§ 7. The order of procedure so far has been as follows :—

(1) Removal of first approximation to the semi-diurnal tide;
(2) removal of quarter-diurnal tide;
(3) analyses;
(4) removal of diurnal tide;
(5) removal of second approximation to semi-diurnal tide.

No attempt has been made to analyse for, and remove, the long-period constituents, and it has not been found possible as yet to remove the residual semi-diurnal tide.

§ 8. *The residual semi-diurnal constituents.*—The residue illustrated by fig. 4 is clearly the difference between observations and predictions from Darwinian short-period constituents. (The matter of shallow-water effects and their inadequate representation will be dealt with separately in § 9.) On certain days it is seen that the residual semi-diurnal tide can give an error of $\pm$ 6 inches, and statement 2, § 1, is justified. It may be stated that at Newlyn there is not a very great range of tide.

The origin of these constituents remains obscure. Apart from the reasons given in Part II., § 18, the presence of non-Darwinian constituents may be readily shown. If we have two constituents, M_2 and S_2, say, then the range of tide varies from day to day and we get the phenomena of springs and neaps : the time between two successive springs is equal to 360° divided by the difference in speed (in degrees per mean solar day) of the two constituents; in the case mentioned this difference in speed is about 24°, so that the spring tides occur at intervals of approximately fifteen days. Now the greatest difference in speed between two Darwinian semi-diurnal constituents is about 52° per m.s.d., corresponding to 'spring tides' at intervals of about seven days. Fig. 5 shows the variation of daily range of the semi-diurnal oscillation for 180 days. The



Newlyn.

FIG. 2.
Residue after removing quarter-diurnal constituents.

January, 1918.

phenomena of springs and neaps are here rather complicated, but the general principle still holds. In the month of January there is a well-marked phenomenon where the springs recur at intervals of two or three days; that is, there is an indication of the presence of a non-Darwinian constituent.

The residue for the month of April is very much affected by the unknown semi-diurnal constituents.

A rather crude form of periodogram has been constructed from the first six months' residues at Newlyn, but it suffices to show that the constituents present in the residue are quite distinct from those in Darwin's schedules; it also shows that all the Darwinian constituents have been effectively removed. The data, however, are not sufficient for definitive conclusions to be drawn as to the nature of the residue.

§ 9. *Errors resulting from the inadequate treatment of shallow-water effects.*—The following table gives a list of the constituents that ought to be taken into account in the prediction of tides for Liverpool, in order to give a satisfactory representation of the shallow-water effects. The amplitudes are approximate only.

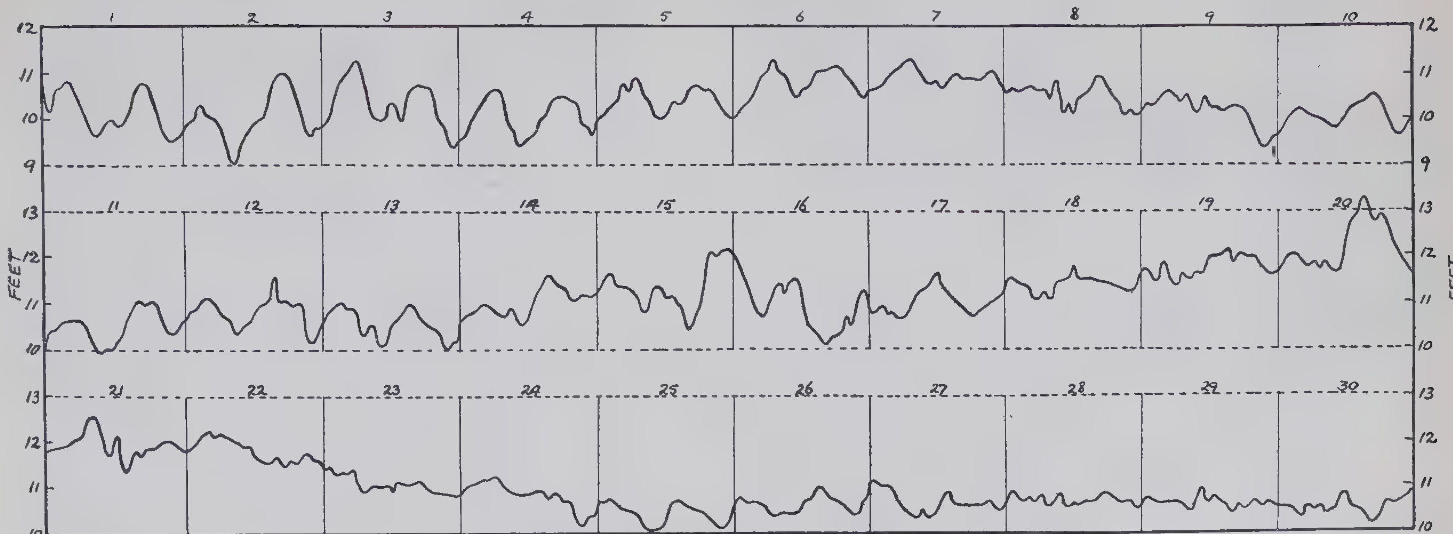
TABLE OF SHALLOW-WATER CONSTITUENTS AT LIVERPOOL.

Inferred from Harmonic Constants for M_4, M_6 *and* M_8.

$\frac{1}{4}$-Diurnal		$\frac{1}{6}$-Diurnal		$\frac{1}{8}$-Diurnal	
Origin	Amplitude	Origin	Amplitude	Origin	Amplitude
$M_2.M_2$	[·67] ft.	$M_2.M_2M_2$	[·20]	$M_2M_2M_2.M_2$	[·07]
S_2	[·42]	S_2	·19	S_2	·08
N_2	·26	N_2	·11	N_2	·05
K_2	·13	K_2	·06	K_2	·03
ν_2	·07	ν_2	·03	etc.	
L_2	·07	etc.			
T_2	·03				
2N	·03	$M_2.S_2.S_2$	·06	$M_2S_2.M_2S_2$	·03
$\mu 2$	·03	N_2	·04	M_2N_2	·03
etc.		K_2	·02	M_2K_2	·02
				etc.	
$S_2.S_2$	·07	$S_2.S_2S_2$	[·01]		
N_2	·08	N_2	·01		
K_2	·04				
etc.					

Those constituents whose amplitudes are enclosed in square brackets are the constituents usually analysed for. The residual quarter-diurnal tide, if all are neglected save $M_2.M_2$ and M_2S_2, will frequently exceed six inches, and the differences between observations and predictions also serve to justify this conclusion. For Newlyn the error will not be so large, but most British ports are situated in estuaries where the shallow-water effect will be quite comparable with that at Liverpool. Hence we base upon these facts the statement (3) of §1.

It may be mentioned that the predicting machines have not been built to cater for the constituents mentioned above.



Newlyn.

FIG. 3.

Residue after removing chief diurnal constituents.

January, 1918

PART II.

Methods.

§ 10. *Inference of chief semi-diurnal constituents.*—It is unnecessary to enter into the details of this process. A tidal constituent being expressed as R cos $(\sigma t-\epsilon)$, where t is zero at midnight on January 0-1, 1918, the following values of R and $-\epsilon$ were actually used:—

	R	$-\epsilon$
S_2 :	1·870	180·00°
M_2 :	5·633	150·33°
N_2 :	1·100	12·53°
K_2 :	0·600	37·46°
L_2 :	0·300	105·33°

An analysis for S_2 was carried out using six months' observations, and the average values of ratios of amplitudes and differences in lags for all other constituents (referred to S_2) were found from known harmonic constants for ports within a few hundred miles of Newlyn; from these were deduced the values of R and $-\epsilon$ given above.

Regarded as true representatives of the constituents, R is probably correct to the nearest tenth of a foot and ϵ may be in error by several degrees. The figures represent *precisely* what was removed by accurate arithmetical processes.

§ 11. *Calculation of harmonic constituents.*—The process on which the intensive analysis of tidal observations depends is that of the summation of harmonic constituents. Consider the calculation of the five constituents given in the previous paragraph; the calculation of the individual arguments by successive addition of hourly increments is itself no light task, and the consequent determination of the cosines and the multiplication by the appropriate amplitudes is a task of appalling magnitude. The greater part of this labour has been avoided in the scheme now to be explained.

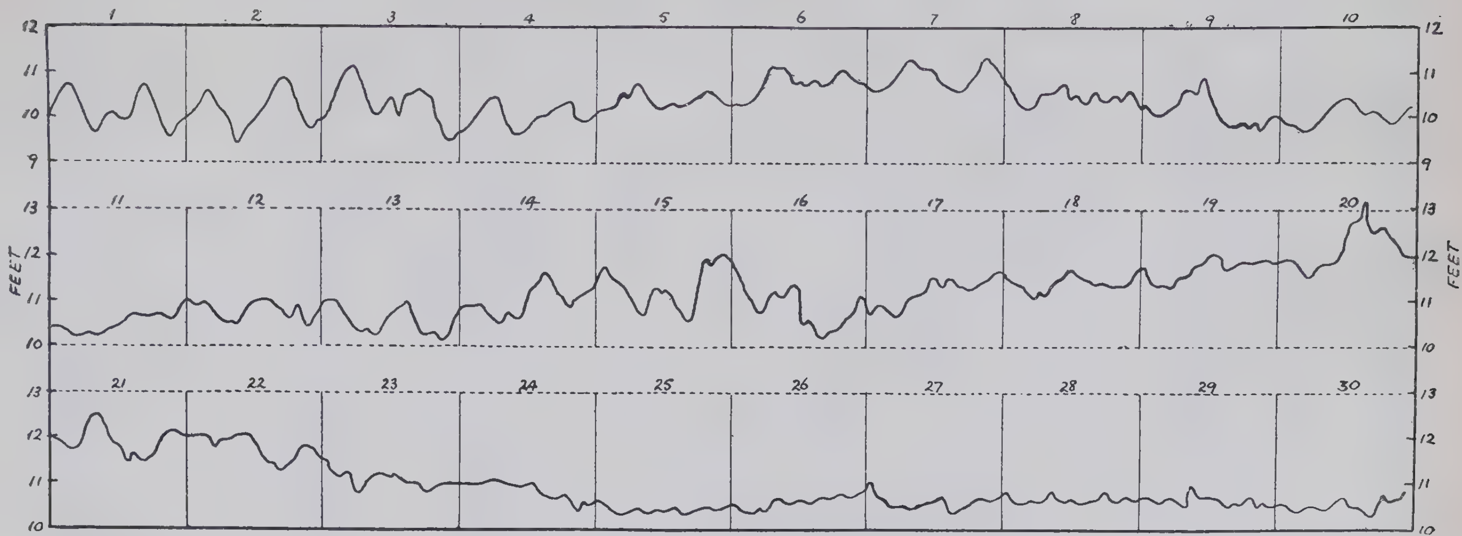
Any term R cos $(\sigma t-\epsilon)$ can be written in the form

$$D\cos(\sigma t-\epsilon+d) + D\cos(\sigma t-\epsilon-d)$$

where $2D\cos d = R$, and D can be chosen at will. By choosing $D=10, 1, 0{\cdot}1, \ldots$ we thus avoid the labour of multiplication, but double the labour of determining arguments and cosines. The latter, however, can also be avoided by the construction of a suitable abac.

Let the argument at time $t=o$ for a given harmonic term be α, and let the speed be σ, so that we have to construct $\cos(\sigma t+\alpha)$ at unit intervals of t. Suppose that we have a horizontal scale graduated uniformly in degrees (θ) on one side, and on the other side let there be the appropriate cosine scale, graduated, say, at intervals of 0·01 in $\cos\theta$. Then if we mark on the scale the values $\theta=\alpha$, $\alpha+\sigma$, $\alpha+2\sigma$, ... $\alpha+t\sigma$, ... we can at once read off, by interpolation in the cosine scale, the values of $\cos\alpha$, $\cos(\alpha+\sigma)$, ... to three decimals. This double scale avoids reference to trigonometrical tables or to a graph of $\cos\theta$.

But this method does not perform automatically the processes of adding σt to the argument α for the required values of t; moreover, a very lengthy scale would be required for us to be able to read off many values to the required degree of accuracy. The problem can be solved by cutting up the scale into sections of length $\theta=\sigma$; the sections $\theta=o$ to σ, σ to 2σ, ... are then placed parallel to one another vertically with their extremities on horizontal lines. A large number of sections can thus be drawn on an open scale and placed side by side. Suppose that the given value of the initial argument (α) be in the first section; then a horizontal straight line passing through this point will cut the vertical sections in the points corresponding to $\theta=\alpha$, $\alpha+\sigma$, ... $\alpha+\sigma t$, ... and by interpolation in the vertical cosine scales the values of the cosines can be immediately read off. It is obvious that only *one* angle (α) needs to be determined, so that it is not necessary to have the θ-scale marked on the cosine scale at all. The best procedure is to use paper ruled in quarter-inch squares with the vertical lines half an inch apart, and with one half-inch to a degree. Since we know that the vertical sections have their upper extremities corresponding to $\theta=o$, σ, 2σ, ..., then any intermediate angle



Newlyn. FIG. 4. *January*, 1918.

Residue after removing second approximation to semi-diurnal constituents.

can be readily found by marking out the scale of $\theta = o$ to σ in degrees on the extreme left of the abac: we shall call this the scale of δ, so that in general

$$\theta = i\sigma + \delta$$

where i is an integer.

If the initial argument (α) is expressed in the form $i\sigma + \delta$, then a horizontal line drawn through δ on the δ-scale will pass through the angles $\alpha - i\sigma, \ldots \alpha, \alpha + \sigma, \ldots$ and readings will commence on the line corresponding to $i\sigma$. In practice this horizontal line is lightly ruled in pencil, and is afterwards erased.

There is, however, a limit to the number of readings on one horizontal line. We shall suppose that the abac covers $r \times 360°$ and contains v complete vertical sections and one incomplete section; then the length of the incomplete section in degrees will be

$$\gamma = 360r - v\sigma.$$

But if the abac were extended we should be getting precisely the same values as if we started on the extreme left again with a new value of δ obtained by diminishing the previous value by γ. Whenever the cycle is completed it is only necessary to subtract γ from the old value of δ to get the new one. The first value of δ having been found as $\alpha - i\sigma$, it is simplest to write down the series of subsequent values of δ before commencing to draw the lines. Sooner or later, however, a value of δ will be obtained which is less than γ, but as the addition of σ to any argument leaves δ unaltered it is immaterial whether we *subtract* γ or *add* $(\sigma - \gamma)$, provided that we leave $\delta < \sigma$. Each new cycle after the first starts on the extreme left: the first cycle, as has been explained, starts on the vertical section corresponding to $i\sigma$.

A further modification is that of using $(1 + \cos\theta)$ instead of $\cos\theta$, so as to avoid negative quantities; the effect of this is that the constant $\Sigma 2D$ must be subtracted from the sum of the harmonic constituents. There are many advantages in the avoidance of negative quantities.

For negative speeds (σ negative) the changes necessary are as follows:—

(1) Start with $r360°, r360° + \sigma, \ldots$ as headings to the vertical sections; this gives a decreasing series of angles;

(2) the first value of δ will be $(r360° + i\sigma - \alpha)$, and readings will commence on the vertical section corresponding to $r360° + i\sigma$; the scale for δ is always regarded as positive;

(3) the value of γ, being positive, will be $(360r + v\sigma)$.

The only change required, apart from the construction of the abac, is that of the determination of δ; after the first cycle the procedure is the same as for positive speeds.

A small-scale illustration of the abacs used is given in fig. 6 for the case $\sigma = 37.4465°$. At the top of the diagram is a horizontal scale for θ and $\cos\theta$ to illustrate the construction of the abac. As an example we shall take $\alpha = 20°$. Then the dotted lines in the upper figure correspond to $20°, 57.45°, \ldots$ and $\cos\theta$ can then be read from the lower scale. The abac is drawn with overlapping sections commencing at $0°, 37.45° \ldots,$ and covers $2 \times 360°$. There are nineteen complete sections and one incomplete section. Hence $v = 19$, $r = 2$, $\sigma = 37.4465$, whence

$$\gamma = 720° - 19 \times 37.4465° = 8.5167°.$$

The δ scale is given for multiples of 10° in this illustration, and $1 + \cos\theta$ is given at intervals of 0.1, decimals being omitted; there are no negative values. With $\delta = 20°$ we get the following values of $1 + \cos\theta$:—

193, 154, 90, 33, 4, 12, 58, 121, 176, 200, 182, 130, 67, 17, 2, 26, 81, 145, 190.

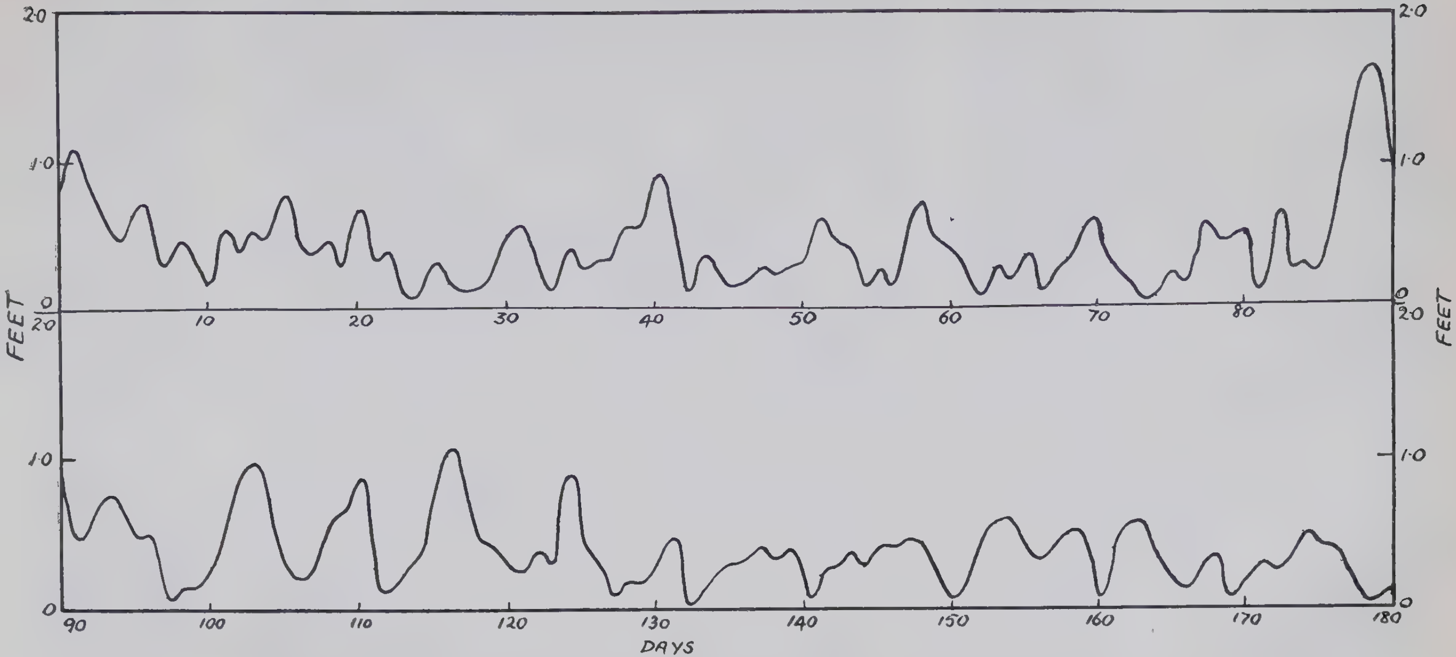
A new cycle is then necessary; this is given by

$$\delta = 20° - 8.5167° = 11.4833°,$$

and the values of $(1 + \cos\theta)$ are continued as

196, 167, 105, ...

If, originally, we had $\alpha = 132.34°$, then we should look along the top of the abac for the nearest value of $i\sigma$ which is less than α, in this case 112.34°, giving $\delta = 20°$; the readings would commence with 33.



Newlyn.

FIG. 5.
Variation of range of final semi-diurnal residue.

January-June, 1918.

§ 12. *Calculation of semi-diurnal tide.*—The calculation of the semi-diurnal tide hour by hour is most expeditiously carried out in accordance with the scheme now to be explained.[2]

Consider $\zeta_2 = \sum_r R_r \cos(\sigma_r t + \alpha_r)$, where t is given in units of one mean solar hour; σ_r is the speed in degrees per mean solar hour, and is such that $\sigma_r - 30$ is small. Then we may write

$$\zeta_2 = C_2 \cos 30°t - S_2 \sin 30°t,$$

where
$$C_2 = \sum_r R_r \cos\left(\overline{\sigma_r - 30}t + \alpha_r\right)°$$

$$S_2 = \sum_r R_r \sin\left(\overline{\sigma_r - 30}t + \alpha_r\right)° = \sum_r R_r \cos\left(\overline{\sigma_r - 30}t + \alpha - 90\right)°.$$

Both C_2 and S_2 are slowly varying quantities because $\sigma_r - 30$ is small, and therefore interpolation can be used if C_2 and S_2 are calculated direct at convenient intervals of time. Supposing that we know C_2 and S_2 at intervals of twenty-four hours, then interpolation formulæ can be applied to give the values at intervals of six hours, and simple linear interpolation is usually sufficient to give the intermediate values at intervals of one hour; but even if this were not sufficient the principle of interpolation can be used. By this method only two series of harmonic constituents have to be summed for intervals of twenty-four hours.

We shall have occasion to use the speed denoted by

$$\rho_r = 24\ (\sigma_r - 30)°$$

This is equal to the speed in degrees per mean solar day less 720°, and it is convenient to speak of it as the 'reduced speed'; we shall use T with ρ_r to signify time measured in units of one mean solar day.

The detailed procedure can now be considered. The methods of § 11 are applied with abacs constructed to give readings at intervals of ρ_r; the abac for M_2 must be on a much more open scale than the others in order to read to three decimal places of a foot: a convenient scale is one-quarter inch to 0·1° and the abacs used are in four overlapping sections. Also the speed of K_2 is such that it is preferable to construct the abac for a speed $6\rho_r$—*i.e.* to read off at intervals of six days; intermediate values are obtained by increasing the appropriate value of δ by ρ_r, $2\rho_r$, ...

When each cycle is completed it is desirable to verify that no omission has taken place, and this can readily be done by calculating independently the date (or day number) corresponding to the last reading of the cycle. Each cycle except the first adds either $(v+1)$ or v readings according to whether δ be less or greater than γ; these should be separately summed before any readings are taken from the abac. This check is very important indeed, for systematic error is fatal to success, and must be avoided. While we have now an assurance that each cycle ends on the correct date, the above check is not sufficient to ensure that a particular line has been drawn correctly. There is a check which can be applied on any day, but it is best to use it on the 15th, 30th and 31st days of each month, as these days are not covered by a check on the sums, mentioned later. Each constituent is expressed as the sum of two others with amplitudes D, say: adding the two readings and subtracting 2D gives, say, $R_r \cos(\rho_r T + \alpha_r)$, a term of C; obtaining the corresponding term of S in the same way, the sum of the squares should be constant, and equal to R_r^2. This test should be made before the summations are commenced.

After carrying out the summations for C and S a check is desirable to indicate casual errors of abac readings or of summations. The method of checking by successive differences is not very efficient in this instance, and use is made of the relation that the sum of $(m+1)$ consecutive values of $R_r \cos(\rho_r T + \alpha_r)$ is equal to the mid-value multiplied by a factor

$$f_r = \frac{\sin \frac{1}{2}(m+1)\rho_r}{\sin \frac{1}{2}\rho_r}$$

The test is best taken with $m+1=7$, whence we can test the values of C and S in a given month for the days 1-14, 16-29, inclusive; the remaining days are tested

[2] The method as given here has been applied to the reduction of the second six months' observations for Newlyn; a slightly different method, not using C_2 and S_2, was used previously.

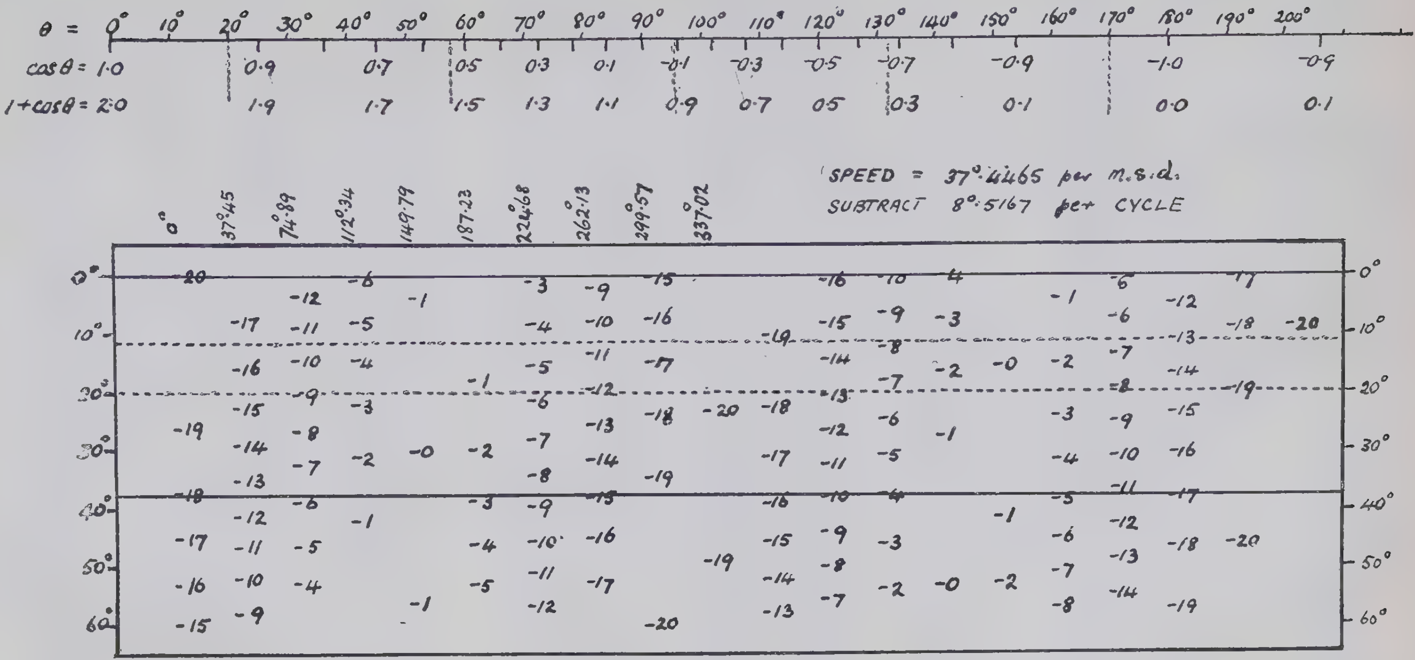


FIG. 6.

Diagram Illustrating Methods of Calculating Harmonic Constituents.

specially as previously mentioned. The values of f_r for the appropriate values of ρ_r are given in the following table:—

S_2:	7·000	L_2:	6·466	ν_2:	2·674
R_2:	6·997	λ_2:	6·294	N_2:	2·349
T_2:	6·997	M_2:	4·720	μ_2:	0·393
K_2:	6·983	2SM:	4·720	2N:	0·131

Multiplying by the above factors the corresponding terms in C on the 4th day of the month, and subtracting the constant due to the use of $1+\cos\theta$ in the abac, the result should be equal to the sum of the values of C for the first seven days of the month; the differences between the two are usually less than 0·020 foot.

The errors in the calculation of C and S as above rarely exceed 0·010 foot.

The next step is to interpolate for values of C and S at intervals of six hours, and for this purpose interpolation formulæ are used; these are modifications of the ordinary central difference formula—

$$u_x = u_0 + x(u_1 - u_0) + \tfrac{1}{4}x(x-1)(u_2 - u_1 - u_0 + u_{-1}) + \ \ldots\ .$$

which gives u_x in terms of the variates u_{-1}, u_0, u_1, u_2. It is supposed that x lies between 0 and 1. If we apply this formula to the case $x=\frac{1}{2}$ and $u_x = \cos(\rho x + \alpha)$ we get M $\cos(\frac{1}{2}\rho + \alpha)$, where $M = (18\cos\frac{1}{2}\rho - 2\cos\frac{3}{2}\rho)/16$. It is easy to show that M is always less than unity; an improvement is to write

$$u_{\frac{1}{2}} = u_0 + \tfrac{1}{2}(u_1 - u_0) - k(u_2 - u_1 - u_0 - u_1)$$

whence $M = (1+2k)\cos\frac{1}{2}\rho - 2k\cos\frac{3}{2}\rho$. We can now choose k to make M unity for a particular speed, or to make the possible error a minimum when several speeds are involved. In the present instance M_2, the largest constituent, and N_2, the constituent with the greatest value of ρ, need only be considered. Taking k successively equal to ·063, ·064, ·065, ·066, we find the values of $(M-1)$ multiplied by the appropriate amplitudes to be respectively −·003, −·001, ·001, ·003 for M_2, and respectively −·005, −·005, −·004, −·003 for N_2. Ignoring the signs, the value of $k=$·065 gives the least additive error, and therefore we adopt the interpolation formula—

$$C_{\frac{1}{2}} = -{\cdot}065C_{-1} + {\cdot}565\,C_0 + {\cdot}565C_1 - {\cdot}065C_2.$$

The errors when ρ is small are negligible whatever value we assign to k within the range ·062 to ·066. Except in rare instances the errors of interpolation will be less than ·010 foot.

A formula similarly derived could be used to get $C_{\frac{1}{4}}$ from $C_{-\frac{1}{2}}$, C_0, $C_{\frac{1}{2}}$, and C_1, but it is better to operate on the original series of values of C at intervals of twenty-four hours, as these have been carefully checked. We obtain

$$C_{\frac{1}{4}} = -{\cdot}05C_{-1} + {\cdot}80C_0 + {\cdot}30C_1 - {\cdot}05C_{-1}$$
$$C_{\frac{3}{4}} = -{\cdot}05C_{-1} + {\cdot}30C_0 + {\cdot}80C_1 - {\cdot}05C_{-1}$$

The maximum error is about ·01 foot; this could be reduced a little by taking three decimals in the coefficients, but the simplicity of the formula outweighed any advantage to be otherwise attained. Obviously $C_{\frac{1}{4}}$ and $C_{\frac{3}{4}}$ can be calculated almost simultaneously by first calculating

$$[-{\cdot}05\,C_{-1} + {\cdot}30C_0 + {\cdot}30C_1 - {\cdot}05C_2] \text{ and then adding } \tfrac{1}{2}C_0 \text{ or } \tfrac{1}{2}C_1.$$

It is helpful to write the primary values in coloured ink and to leave blank spaces for the values of $C_{\frac{1}{4}}$, $C_{\frac{1}{2}}$, and $C_{\frac{3}{4}}$. As the interpolations are all separate calculations with no risk of systematic error, and as the smaller interval makes the successive differences to diminish more rapidly than for the primary series, all the interpolations can be checked by differencing the complete series . . . C_0, $C_{\frac{1}{4}}$, $C_{\frac{1}{2}}$, $C_{\frac{3}{4}}$, C_1, ...

The interpolations for C and S for the intermediate hours are simply carried out by linear interpolation, and then we have the height of the semi-diurnal tide given by

$$\zeta_2 = C_2\cos 30°t - S_2\sin 30°t.$$

There is no great necessity for checks to be applied at this stage. Systematic error is only possible by using the formula wrongly, and examination at intervals is sufficient to test this.

At each stage of the work the maximum error is less than ·010 foot and the value of y_2 can be taken as correct to within ·020 foot—the average error, regardless of sign, will be much less than this.

§ 13. *Calculation of diurnal tide.*—The changes to be made in § 12 in order to adapt the work to the calculation of the diurnal tide are very few. We have

$$\zeta_1 = C_1 \cos 15°t - S_1 \sin 15°t$$

where

$$C_1 = \sum_r R^r \cos \overline{(\sigma_r - 15\,t + \alpha_r)}°$$

$$S_1 = \sum_r R_r \sin \overline{(\sigma_r - 15\,t + \alpha_r)}°$$

The multipliers f_r used in testing the summations are as follows:—

S_1 : 7·000	M_1 : 6·383	ρ_1 : 2·491
P_1 : 6·997	J_1 : 6·187	Q_1 : 2·167
K_1 : 6·997	O_1 : 4·553	$2Q_1$: −·008
	OO_1 : 4·211	

The formulæ of interpolation may be taken the same as for the semi-diurnal tide.

(For Newlyn the diurnal tide is small, and it was unnecessary to use the multipliers f_r: differences could be used on C_0, C_1, C_2, ...)

§ 14. *Analyses for quarter-diurnal tide.*—It has previously been mentioned that the quarter-diurnal tide (ζ_4) at Newlyn has a simple relation to the square of the semi-diurnal tide (ζ_2). There are two methods for determining this relationship numerically, the first depending upon deduction from harmonic analyses for M_2 and M_4 over fairly long periods, and the second depending upon the direct correlation between ζ_4 and ζ_2^2. The former method assumes the existence of the relationship for all constituents, and, if it be known that this is legitimate, it is probably the best method in practice. The latter method is interesting and is of value when observations over only a short interval are available; the results show whether the method is valid. Both methods have been applied to Newlyn observations, and the results are in satisfactory accordance.

The phase shift is approximately the difference between the lag of M_4 and twice the lag of M_2, and the reduction factor is approximately equal to the amplitude of M_4 divided by half the square of the amplitude of M_2. The effect of the constituents L_2 and N_2 in contributing to the M_4 term in ζ_2^2 should be allowed for, but the corrections are small.

The second method is as follows. Let the quarter-diurnal tide in the residue after removing the semi-diurnal tide be represented by

$$\zeta_4 = \sum_r q_r \cos (\sigma_r t - k_r)$$

Applying the S_4 least-square rule [3] to twenty-four hourly values would give

$$a = \tfrac{1}{12}\sum_{t=0}^{23} \zeta_4 \cos 60°t = \sum_r f_r q_r \cos (k_r + \xi_r) + \sum_r f'_r q_r \cos (k_r + \xi'_r)$$

$$b = \tfrac{1}{12}\sum_{t=0}^{23} \zeta_4 \sin 60°t = \sum_r f_r q_r \sin (k_r + \xi_r) - \sum_r f'_r q_r \sin (k_r + \xi'_r)$$

where f_r, f'_r, ξ_r and ξ'_r are dependent only on σ_r. We may therefore write

$$a = \sum_r g_r q_r \cos (k_r + \eta_r)$$

$$b = \sum_r g_r q_r \sin (k_r + \eta_r)$$

where g_r and η_r are dependent only on σ_r.

[3] For technical terms and processes see *Report* for 1920.

Now suppose that ζ_2^2 contains a quarter-diurnal portion that can be represented by

$$\Sigma Q_r \cos(\sigma_r t - K_r).$$

Then, analysing as above for A and B will give

$$A = \Sigma g_r Q_r \cos(K_r + \eta_r)$$

$$B = \Sigma g_r Q_r \sin(K_r + \eta_r)$$

where g_r and η_r, being dependent only on σ_r, are the same as before.

If we now suppose that

$$K_r - k_r = \chi$$

and

$$q_r = 2cQ_r$$

and that c and χ are independent of r, we have

$$a \cos\chi - b \sin\chi = 2cA,$$

$$a \sin\chi + b \cos\chi = 2cB,$$

whence

$$\tan\chi = \frac{Ba - Ab}{Aa + Bb} \text{ and } 4c^2 = \frac{a^2 + b}{A^2 + B^2}.$$

The value of χ can alternatively be found as

$$\tan^{-1}\frac{B}{A} - \tan\ {}^{1}\frac{b}{a}.$$

The definition of c and χ is such that if we write $R\cos(\sigma t + \alpha)$ for the semi-diurnal tide, then the quarter-diurnal tide is given by $cR^2 \cos(2\sigma t + 2\alpha + \chi)$.

In the case of Newlyn two such analyses were made on the results for February 15 and March 28, on which days the quarter-diurnal tide was prominent and free from serious perturbation by other constituents. The results were:

$$2c = \cdot 0222,\ \cdot 0229;\ \chi = 95^\circ,\ 96^\circ, \text{ respectively.}$$

These values were in accordance with reductions from the first method, and the mean values

$$2c = \cdot 0225,\ \chi = 96^\circ$$

were adopted.

Further experience with observations at Liverpool has shown that the second method should be used with some caution; it is necessary to choose days when ζ_4 is prominent and free from disturbance by other constituents, and several such days should be taken. The values of c and χ, however, do not vary much more than do the results of analyses for M_4 from year to year.

§ 15. *Calculation of the quarter-diurnal tide.*—(1) The first method of calculation tried used the values of ζ_2^2 direct. An interpolation formula was evolved for use with the hourly values of ζ_2^2. It is easily shown that

$$\cdot 0091\ (\zeta_2^2)_1 + \cdot 0164\ (\zeta_2^2)_2$$

gives the value of $(\zeta_4)_0$, where suffixes outside the brackets indicate the relative hourly values. The formula is based upon the assumption of a mean speed of 58° per mean solar hour, and it does not correspond exactly to constant values of c and χ; the variations in c and χ are small, however, and of no importance. It is easy to calculate the precise value of any constituent.

Unfortunately ζ_2^2 contains constants and long-period constituents, so that the formula gives a part L which has to be removed. It is easy to show, however,

that the average of the results of the formula over twenty-hours will give, to a very close degree of approximation, the value of L at the mid-hour. L was found at intervals of twelve hours, and intermediate values obtained by linear interpolation. It should be noted that L has to be *added* to the residue.

(2) The second method, used for Liverpool, was applied to $(\zeta_2)^2_0-(\zeta_2)^2_3$ instead of ζ_2^2, with an appropriate formula. The residual value of L is very small.

(3) The third method has been used for reducing the second half of the 1918 records for Newlyn. This method avoids the introduction of long-period constituents, and involves less labour than the first method.

Referring to § 12, we have

$$\zeta_2 = C_2 \cos 30°t - S_2 \sin 30°t = R \cos (30t+\alpha)°$$

where $R \cos \alpha = C$, $R \sin \alpha = S$, and R is slowly-varying.

Then

$$\zeta_4 = cR^2 \cos (60t+2\alpha+\chi)°$$

$$= C_4 \cos 60°t - S_4 \sin 60°t,$$

where

$$C_4 = (c \cos \chi)(C_2^2-S_2^2) - (c \sin \chi)(2C_2S_2)$$

and

$$S_4 = (c \cos \chi)(2C_2S_2) + (c \sin \chi)(C_2^2-S_2^2).$$

Now $c \cos \chi$ and $c \sin \chi$ are constants, being, for Newlyn, $-.0012$ and $.0120$ respectively. It is sufficient, therefore, to calculate $C_2^2-S_2^2$ and $2C_2S_2$ at intervals of six (or even twelve) hours, and thence to evaluate C_4 and S_4 by linear interpolation. The hourly values of ζ_4 are then given by

$$C_4 \cos 60°t - S_4 \sin 60°t,$$

and since $\cos 60°t$ is either ± 1 or $\pm \frac{1}{2}$, and $\sin 60°t$ is either $\pm .866$ or zero, it is desirable to write down $\frac{1}{2}C_4$ and $.866\, S_4$; the calculation of ζ_4 is then a very simple matter.

This method is superior in every way to either of the first two methods.

§ 16. *Analysis of observations.*—Before proceeding to discuss the analysis of residues it is desirable to consider the principles governing the methods in vogue for the harmonic analysis of tidal observations. A full discussion of these is given by Professor Proudman in the Report for 1920, and a brief statement only is here required; the matter may be considered under three headings:—(1) the isolation and analysis of the principal solar series; (2) the 'assignment'; (3) the length of record to be included.

(1) If the constituent sought be of the principal solar series then it repeats itself at intervals of twenty-four mean solar hours, or of some sub-multiple thereof. Hence the mean of the heights at intervals of twenty-four hours will tend to isolate the height due to the solar series of constituents at the given hour of the day. Other constituents will, in the long run, eliminate themselves. The twenty-four means so obtained may be submitted to harmonic analysis by the least square rule, and the separate constituents obtained. If ζ is the tidal height at time t, measured in mean solar hours, N the number of mean solar days included in the record submitted to analysis, $15°n$ the speed in degrees per mean solar hour, then the whole of the processes give

$$A_o = \frac{1}{24N} \Sigma \zeta$$

$$A_n = \frac{1}{12N} \Sigma \zeta \cos 15°nt \qquad (n=1, 2, \ldots)$$

$$B_n = \frac{1}{12N} \Sigma \zeta \sin 15°nt \qquad (n=1, 2, \ldots)$$

(2) The 'assignment' is a process whereby heights at successive mean solar hours may be utilised in 'special time.' When the constituent sought is a principal solar constituent the method of analysis is that stated above: a similar process for any other constituent would involve a knowledge of heights at 'special hours' in

place of mean solar hours. A 'special day' is the time taken for the argument of the constituent in question to increase by 360° if it be diurnal or 720° if it be semi-diurnal. As this knowledge is lacking, the heights at the nearest solar hours are assigned and attributed to the special hours. A certain correction is made depending upon the assumption of a random distribution of the difference in time between special and mean solar hours. In dealing with large constituents this correction may not be adequate.

(3) The length of record to be included in the process is determined by choosing an interval of time such that the effect of some one large constituent is made as small as possible. The effects of all other constituents are ignored, though this may not be justifiable if these constituents are large or if their speeds are very nearly equal to that of the constituent sought.

The whole purpose of the methods described is that of determining simply two numbers A and B, and there is no internal evidence to show that we are entitled to attach any significance to these numbers, though they are supposed, and are used, to define a constituent. There is nothing to indicate the presence or magnitude of perturbing constituents, and nothing to show whether the whole of the constituents have been dealt with or not. The whole process is repeated for each of the constituents *expected* to be present. As instruments for research these methods are singularly inefficient, the paucity and uncertainty of the results being in remarkable contrast to the magnitude of the work required.

§ 17. *Darwin's method of analysis of observations.*—A method which ranks as a great improvement on those discussed in § 16 was introduced by Darwin in his paper on the abacus. The method is only applied to the group of constituents whose speeds are nearly equal to those of the principal solar constituents; the constituents K_2, R_2, S_2 and T_2 are all nearly equal in speed, and are treated over a short interval of time as having the speed of S_2. For successive intervals the values of A_2 and B_2 would be constant if only S_2 were present, but in the case considered they vary harmonically, and analyses of the variations give the harmonic numbers for the four constituents.

This method will now be considered in more detail. The exposition here given is different from that by Darwin, and the method has been generalised to some extent.

Suppose that we are dealing with a constituent R cos $(\sigma t-\epsilon)$ whose value is given at intervals of one mean solar hour. Then the contribution to A_n and B_n in the analysis for solar constituents will be

$$A^n=\frac{1}{12N}\,\Sigma R\cos(\sigma t-\epsilon)\cos 15^\circ nt=\frac{1}{24N}\,\Sigma R\left\{\cos(\overline{\sigma-15n}t-\epsilon)^\circ+\cos(\overline{\sigma+15n}t-\epsilon)^\circ\right\}$$

$$B_n=\frac{1}{12N}\,\Sigma R\cos(\sigma t-\epsilon)\sin 15^\circ nt=\frac{1}{24N}\,\Sigma R\left\{-\sin(\overline{\sigma-15nt}-\epsilon)+\sin(\overline{\sigma+15nt}-\epsilon)^\circ\right\}$$

where the summations are taken from $t=24T$ to $24T+24N-1$, so that it is supposed that the summations begin at zero hour on day T and include all the hourly values of the constituent over N complete days.

These may be written as

$$A^n=f'R\cos(\epsilon'-24\sigma T)+f''R\cos(\epsilon''-24\sigma T),$$

$$B_n=f'R\sin(\epsilon'-24\sigma T)+f''R\sin(\epsilon''-24\sigma T),$$

where

$$f'=\frac{\sin 12N\,(\sigma-15n)^\circ}{24N\sin\frac{1}{2}(\sigma-15n)^\circ},\qquad f''=\frac{\sin 12N(\sigma+15n)^\circ}{24N\sin\frac{1}{2}(\sigma+15n)^\circ},$$

$$\epsilon''=\epsilon-12N(\sigma-15n)^\circ+\tfrac{1}{2}(\sigma-15n)^\circ$$

$$\epsilon''=\epsilon-12N(\sigma-15n)^\circ+\tfrac{1}{2}(\sigma+15n)^\circ=\epsilon'+15n^\circ,$$

certain multiples of 360° having been ignored.

It is now convenient to write these in the form

$$\left.\begin{aligned} A_n &= F\cos(\eta-\rho T) \\ B_n &= F\sin(\eta-\rho T), \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where

$$F\cos\eta = f'R\cos\epsilon' + f''R\cos\epsilon'',$$

$$F\sin\eta = f'R\sin\epsilon' + f''R\sin\epsilon'',$$

and ρ is the equal to $24(\sigma-15n)^\circ$: it differs from the true speed in degrees per mean solar day only by a multiple of 360°. We shall speak of F, η and ρ as the 'reduced values' of R, ϵ and σ. We now obtain

$$\left.\begin{aligned} (F/R)^2 &= f'^2 + f''^2 + 2f'f''\cos 15^\circ n \\ \text{and} \quad \tan(\eta-\epsilon') &= \frac{f''\sin 15^\circ n}{f'+f''\cos 15^\circ n}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (2)$$

As, however, we shall not have occasion to use values of N other than N = 30 the relations between F and R, η and ϵ may be given once for all in a numerical form for convenient values of ρ. The value of $\epsilon-\eta$ is practically the same for all values of n, but the ratio of R to F varies with n.

ρ	$\epsilon-\eta$	$n=0$ R/F	$n=1$ R/F	$n=2$ R/F
0°	0°	1·0000	1·0000	1·0000
1°	14·96°	1·0115	1·0101	1·0109
2°	29·92°	1·0472	1·0444	1·0459
3°	44·88°	1·1107	1·1063	1·1086
4°	59·84°	1·2092	1·2027	1·2062
5°	74·80°	1·3552	1·3461	1·3509
6°	89·76°	1·5708	1·5582	1·5649

Having obtained A_n, B_n for all the values of n, each pair of harmonic numbers is treated separately, so that we shall now omit the suffix n.

If ρ is zero then A and B are constants; if ρ is not zero then both A and B vary harmonically with the period $360/\rho$ days. We shall get what we may call 'conjugate constituents' with the same period if the values of ρ are equal and opposite in sign. Thus R_2 and T_2 with $\rho = \pm 0{\cdot}9856$ are conjugate with respect to S_2. Now, in general, we shall have several constituents occurring together so that we shall have

$$\left.\begin{aligned} A &= F_0\cos\eta_0 + \sum_r \{F_r\cos(\eta_r-\rho_r T) + F'_r\cos(\eta'_r+\rho_r T)\}, \\ B &= F_0\sin\eta_0 + \sum_r \{F_r\sin(\eta_r-\rho_r T) + F'_r\sin(\eta'_r+\rho_r T)\}, \end{aligned}\right\} \quad . \quad . \quad (3)$$

and we can consider ρ_r as positive. These may be written as

$$\left.\begin{aligned} A &= A_{c0} + \sum_r (A_{cr}\cos\rho_r T + A_{sr}\sin\rho_r T) \\ B &= B_{s0} + \sum_r (B_{sr}\cos\rho_r T + B_{cr}\sin\rho_r T) \end{aligned}\right\} \quad . \quad . \quad . \quad (4)$$

where

$$\left.\begin{aligned} A_{c0} &= F_0\cos\eta_0, & B_{s0} &= F_0\sin\eta_0 \\ A_{cr} &= F_r\cos\eta_r + F'_r\cos\eta'_r, & A_{sr} &= F_r\sin\eta_r - F'_r\sin\eta'_r \\ B_{cr} &= -F_r\cos\eta_r + F'_r\cos\eta'_r, & B_{sr} &= F_r\sin\eta_r + F'_r\sin\eta'_r \end{aligned}\right\} \quad . \quad . \quad (5)$$

Therefore we have

$$\left.\begin{aligned} F_r\cos\eta r &= \tfrac{1}{2}(A_{cr}-B_{cr}), & F_r\sin\eta_r &= \tfrac{1}{2}(A_{sr}+B_{sr}) \\ F_r'\cos\eta_r' &= \tfrac{1}{2}(A_{cr}+B_{cr}), & F_r'\sin\eta'_r &= \tfrac{1}{2}(-A_{sr}+B_{sr}) \end{aligned}\right\} \quad . \quad . \quad (6)$$

The application of the least square rule to (4) gives, for example,

$$\tfrac{1}{2}A_{cr} = \underset{M\rightarrow\infty}{\mathrm{Lt}} \frac{1}{M} \sum_{T=0}^{M-1} A \cos \rho_r T \qquad . \quad . \quad . \quad . \quad (7)$$

Theoretically, therefore, we can obtain A_{cr}, A_{sr}, . . . and thence F_r, η_r by (6); application of the table given above determines the corresponding values of R and ϵ.

In the simple case considered by Darwin the periods are exactly a year and half a year, and he was able to apply the least square rule to twelve values with N = 30 and T such that $\rho_r T$ is very nearly a multiple of 30°. The perturbation of one constituent upon another was negligible so far as constituents of the solar group were concerned. Some perturbation, however, was caused by M_2 with $\rho = -24{\cdot}38$. Taking T at intervals of about 30·5 days on the average gives ρT about $-744°$, or 'apparently' $-24°$. The residual effect of M_2 is therefore to give a perturbation which is approximately annual in period. Darwin makes corrections for this perturbation. In the present paper we shall not have occasion to consider such perturbations, as we are not dealing with observations but with residues.

§ 18. *Analysis of residues.*—When the chief constituents have been subtracted from the tidal observations there is greater freedom possible in the details of analysis. In the analysis of observations there are two kinds of errors, the first being due to the methods of assignment, which introduce errors in the constituent sought whether other constituents be present or not. The second kind of error is due entirely to the presence of other constituents. In both cases the error is proportional to the size of the constituent producing it, and if the chief constituents be removed the methods of analysis can be applied to the residues without serious error. If we consider the errors due to the assignment as negligible or adequately corrected by the multiplying factor previously mentioned, then we can apply more generally Darwin's method as given for the solar constituents.

The exposition of Darwin's method in § 17 can be generalised simply by writing 'special time' for 'mean solar time.' If the heights are given (or obtained roughly by assignment) at intervals of one special hour, then the speed per special hour is $15n°$ and ρ (in special time) is zero for the constituent appropriate to the special time. As an example the L_2-group of constituents includes L_2 with speed = 29·5285° per m.s.h. and λ_2 with speed = 29·4556° per m.s.h. In special (or L_2) time the speed of L_2 becomes 30° per L_2.h. and the speed of λ_2 becomes

$$\frac{29{\cdot}4556 \times 30°}{29{\cdot}5285} = 29{\cdot}9259° \text{ per } L_2\text{.h.},$$

corresponding to $\rho = 1{\cdot}7784°$. The constituent λ_2, therefore, will give approximately semi-annual variations in the A and B obtained by the 'L_2 process.' By the 'L_2 process' is meant the analysis as for L_2.

Darwin's method is applicable wherever there are groups of constituents whose speeds are nearly equal. Tidal constituents are supposed to be separable from one year's observations only, so that two constituents are not separable in a year unless their speeds, true or reduced, differ by a multiple of approximately 360° per mean solar year, corresponding to a difference of approximately 1° in ρ. Now the constituents occur in groups such that there is a difference in speed of about 12° per mean solar day from group to group. It is necessary to choose N such that one group has very little effect upon the results of analyses relating to another group. If we consider the account of the method in §17 it will be found that the ratio F/R depends chiefly upon f', and this vanishes for $24(\sigma - 15n)° = 12°$ when N is a multiple of fifteen days. It will be sufficient, therefore, to take N = 30, say, in all cases whether we are dealing with solar or special time.

Darwin's method is to take his thirty-day intervals running almost consecutively, but by so doing the most is not made of the material. In the present investigation the interval is taken as thirty days, but the intervals overlap, so that analyses are carried out for N = 30 and T = 0, 10, . . . This is obviously effected by taking N = 10, T = 0, 10, ..., and then averaging the values of A (and B) in threes to correspond to N = 30.

The methods are now easily explained by references to actual analyses and we shall first study the case of M_2. The hourly heights (mean solar time) were assigned

to M_2-time in the usual B.A. manner,[4] and were then treated in sections of ten lunar days. For each hour series in the section the heights were averaged, giving twenty-four values for submission to harmonic analysis, by the least square rule, for a semi-diurnal constituent. The results are shown in Table I., cols. 2 and 6; averaging these results in threes gives the values of A and B in cols. 2 and 7; these correspond to $N=30$, $T=0, 10, \ldots$ The diminution of range is obviously due to the diminution of the effects of other groups. If these results are plotted, as is done in fig. 7, a very striking effect is shown: there is a well-marked variation in the values of A and B with a period somewhat less than three months. There is nothing in Darwin's schedules of constituents to explain this perturbation; it cannot possibly arise from another group because a difference in speed of 12° or 13° per day would require a true amplitude (R) about 0·7 foot!

Thus the method has already shown the existence of one constituent previously unsuspected: it may be repeated that the ordinary methods of analysis would have given one A and one B for an arbitrarily fixed interval, and thus no new constituent would have been revealed. We can now consider the calculation of the most trustworthy values of A_{c0} and B_{s0}, corresponding to the constituent M_2; shall we simply take the average of all the values of A, or shall we take the average over a definite interval of time determined by considerations of relative speeds of M_2 and S_2, say, or by the relative speeds of the perturbing constituents indicated in the figure? The problem is rather complex if we are limited to rigid arithmetical methods, obviously, if we choose an interval to satisfy one condition it may happen that the other constituents have their greatest effect in it. Further, in this case we do not know exactly the speed of the perturbing constituent. The only satisfactory solution of the problem is to use the idea of an 'asymptotic mean' and to discard altogether the idea of attempting to fix a criterion for choosing the interval for analysis. Suppose that we sum the values of A consecutively, writing down the sum Σ in col. 4, Table I., and then divide each sum by M, the number of contributory values of A, as in col. 5, then the asymptotic mean is the limiting value of this average (Σ/M) as M increases indefinitely; *i.e.* the asymptotic mean is defined, as in § 18 (7), by

$$\underset{M\to\infty}{L}\ \frac{1}{M}\sum_{T=0}^{10(M-1)} A_T$$

Here the suffix T is used to denote the value of A in the thirty-day interval commencing on day T. The values of Σ/M are plotted in Fig. 7, and the dotted lines give approximately the tendency of the curves. The oscillations about the dotted line diminish in range as M increases, and there is a tendency for the line to reach a constant value—the asymptotic mean. Of course, in this case, with only six months' residues submitted to analysis, it is impossible to give a really definitive value to the asymptotic mean, but it is possible to give a much better value to A_{c0} than would result from numerical methods with an arbitrarily fixed interval. These curves provide an interesting commentary on the accuracy of the 'constants' usually obtained.

In the case of M_2 there are no obvious semi-annual oscillations in A and B, though there may be some annual oscillation; analyses from six months' residues do not suffice for dealing with these.

A very interesting case is that of the L_2 group. Darwin's schedules of constituents indicate only L_2 and λ_2, and the latter would give semi-annual variations in L_2 (A, B). Table (II.) gives the values of A and B by the L_2 process, in columns 2 and 6 for $N=30$. The values of Σ/M are illustrated in fig. 8.

It is rather difficult to determine A_{c0} and B_{s0} satisfactorily because of the large perturbing constituents. It is an advantage to be able to remove even crude approximations to these before proceeding further, but in this case they are small and are allowed to stay. The value of ρ for λ_2 has been given as $-1{\cdot}778°$ and we shall write

$$\rho_2=1{\cdot}778°;$$

the suffix here denotes that the variation is semi-annual.

[4] For technical terms and explanations reference should be made to the Report by Professor Proudman, 1920.

We now form A cos ρ_2T, A sin ρ_2T, B cos ρ_2T, B sin ρ_2T, as in Table III. When M is large we shall have from (7) § 18,

$$\begin{aligned} \text{Asymptotic mean of } A\cos\rho_2T &= \tfrac{1}{2}A_{c2} \\ \text{,, \quad ,, \quad } A\sin\rho_2T &= \tfrac{1}{2}A_{s2} \\ \text{,, \quad ,, \quad } B\cos\rho_2T &= \tfrac{1}{2}B_{s2} \\ \text{,, \quad ,, \quad } B\sin\rho_2T &= \tfrac{1}{2}B_{c2} \end{aligned}$$

Determining these means as nearly as possible we have

$$F_2\cos\eta_2 = \tfrac{1}{2}A_{c2}-\tfrac{1}{2}B_{c2} = -\cdot003-\cdot012 = -\cdot015$$

$$F_2\sin\eta_2 = \tfrac{1}{2}A_{s2}+\tfrac{1}{2}B^s{}_2 = \cdot000+\cdot044 = \cdot044$$

and

$$F'_2\cos\eta'_2 = \tfrac{1}{2}A_{c2}+\tfrac{1}{2}B_{c2} = -\cdot003+\cdot012 = \cdot009$$

$$F'_2\sin\eta'_2 = -\tfrac{1}{2}A_{s2}+\tfrac{1}{2}B^s{}_2 = -\cdot000+\cdot044 = \cdot044$$

Hence we have two constituents

$$F_2\cos(\eta_2-\rho_2T) = \cdot047\cos(109^\circ-\rho_2T)$$

$$F'_2\cos(\eta'_2+\rho_2T) = \cdot045\cos(78^\circ+\rho_2T)$$

The latter of these gives the constituent λ_2.

Now using the table of §17 we have (R,ϵ) respectively equal to (·047, 135°), and (·045, 52°), the latter giving λ_2.

The presence of the constituent 'conjugate' to λ_2 with $\rho=1\cdot778^\circ$ in L_2 time is very interesting, especially when it turns out to be as important as λ_2. Again we have an unsuspected constituent, but a word of caution must be given. Our analysis has assumed the presence of two constituents with equal and opposite values of ρ, and indeed this is often the case, but here we can assert nothing about the unknown constituent except that it has ρ approximately $=2^\circ$, and without more definite information as to the value of ρ the above figures are not to be considered as definitive for this constituent. It is not inappropriate to add that though we may not get pairs of values of ρ to be equal and opposite in sign yet the tidal constituents indicated by the potential are such that the relation is nearly true, and any correction necessary could easily be applied, provided that the true values of ρ are known.

It was considered to be of interest to see the results after the two disturbing constituents had been removed, and cols. 10 and 11, Table II., give the residual A and B; these are plotted in fig. 8. An independent deduction of A_{c0} and B_{s0} from these figures confirms the values previously obtained from the original A and B; we get

$$A_{c0} = \cdot010,\quad B_{s0} = \cdot025$$

whence the L_2 constituent in the residue is

$$\cdot027\cos(\sigma t-68^\circ).$$

These examples are sufficient to illustrate the methods. In practically all cases approximate quarter-annual perturbations in the A and B have been found. The cause of these is obscure and the matter is still under investigation.

TABLE I.—M_2 Process.

M	A				B			
	T=10(M-1) N=10	T=10(M-1) N=30	Σ	$\frac{\Sigma}{M}$	T=10(M-1) N=10	T=10(M-1) N=30	Σ	$\frac{\Sigma}{M}$
1	·022	·005	·005	·005	·213	·027	·027	·027
2	·009	·070	·075	·038	−·099	−·069	−·042	−·021
3	−·015	·061	·136	·045	−·031	−·081	−·123	−·041
4	·217	·095	·231	·058	−·079	−·094	−.217	−·054
5	−·018	−·003	·228	·045	−·133	·027	−·190	−·038
6	·086	−·025	·203	·034	−·070	·015	−·175	−·029
7	−·076	·048	·251	·036	·285	−·090	−·265	−·038
8	−·084	·061	·312	·039	−·170	−·171	−·436	−·054
9	·304	·073	·385	·043	−·384	−·179	−·615	−·058
10	−·037	·058	·443	·044	·041	−·068	−·683	−·068
11	−·051	−·007	·436	·040	−·373	−·039	−·722	−·066
12	·262	−·002	·434	·036	·129	·001	−·721	−·060
13	−·231	−·046	·388	·030	·126	−·004	−·725	−·054
14	−·038	−·054	·334	·024	−·251	−·006	−·731	−·052
15	·131	−·054	·280	·019	·114	−·054	−·785	−·052
16	−·255	—	—	—	·112	—	—	—
17	−·039	—	—	—	−·388	—	—	—
1	2	3	4	5	6	7	8	9

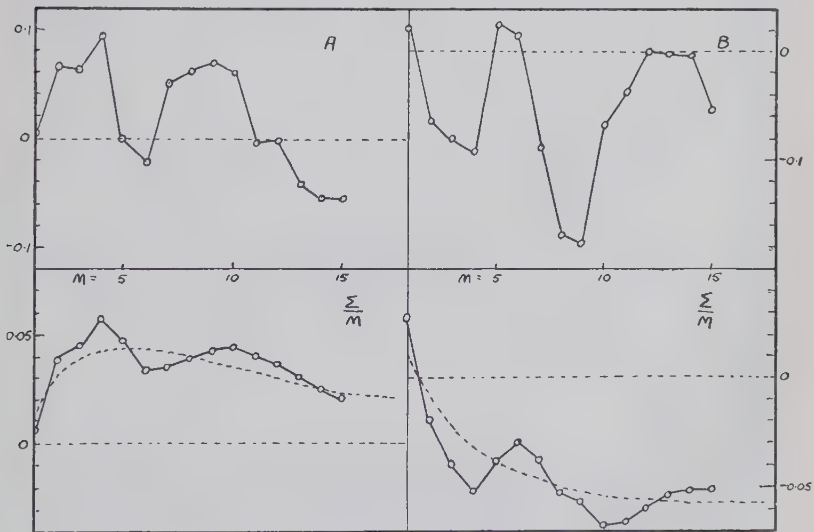


FIG. 7.—M_2 Process.

TABLE II.—L_2 Process.

M	N = 30 A	N = 30 B	cos ρ_2T	sin ρ_2T	A cos ρ_2T	A sin ρ_2T	B cos ρ_2T	B sin ρ_2T	Residual A	Residual B
1	–·046	·081	1·000	·000	–·046	·000	·081	·000	–·040	–·007
2	·003	·091	·951	·309	·003	·001	·086	·028	·008	–·002
3	·002	·131	·809	·588	·002	·001	·106	·077	·006	·045
4	–·011	·099	·602	·799	–·007	–·009	·060	·080	–·007	·016
5	–·032	–·002	·326	·946	–·010	–·031	–·007	–·002	–·030	–·054
6	–·095	·036	·018	1·000	–·002	–·095	·001	·036	–·095	·010
7	·108	·089	–·292	·956	–·031	·103	–·027	·085	·106	·092
8	·122	·065	–·559	·829	–·068	·101	–·036	·054	·119	·094
9	·053	–·075	–·788	·616	–·040	·031	·059	–·046	·049	–·020
10	–·129	–·079	–·940	·342	·121	–·044	·074	–·027	–·135	–·005
11	–·057	–·120	–·999	·035	·057	–·020	·120	–·042	–·063	–·032
12	–·002	–·074	–·966	–·259	·002	·000	·071	·019	–·007	·018
13	·035	–·068	–·889	–·545	–·029	–·019	·057	·037	·030	·020
14	·034	·029	– 629	–·777	–·021	–·026	–·018	– 023	·031	·104
15	·134	·035	–·358	–·934	–·048	–·125	–·012	– 033	·134	·090
1	2	3	4	5	6	7	8	9	10	11

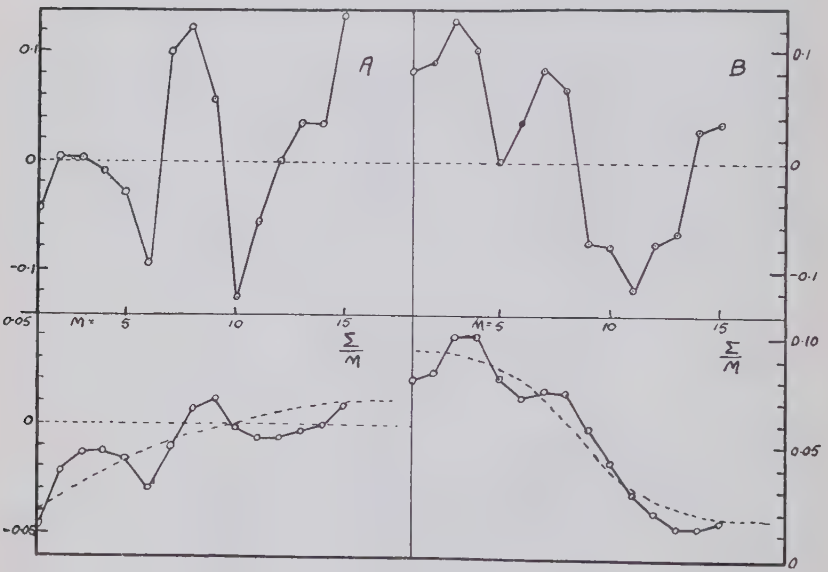


FIG. 8.—L_2 Process.

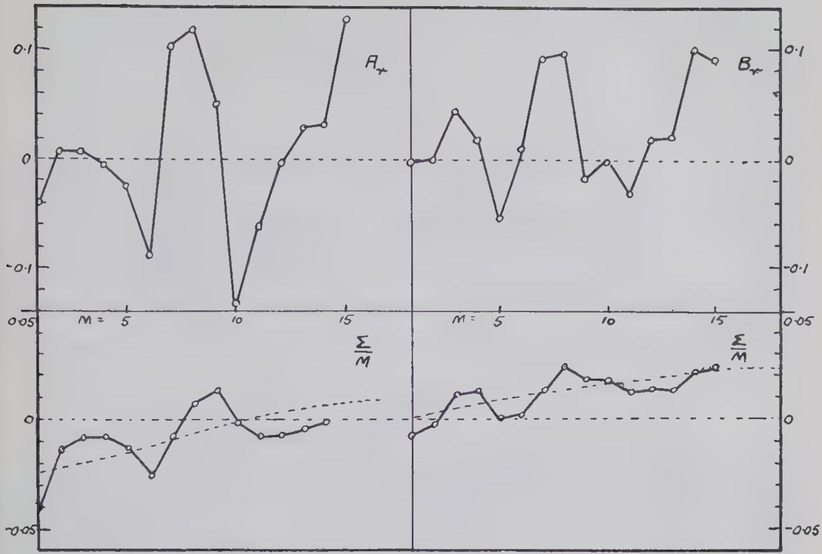


FIG. 9.—L_2 Process.
(After removal of λ_2 and its conjugate.)

§ 19. *Results of analyses.*—For the constituents M_2, S_2, N_2, K_2, L_2 the results of analyses of the residues are as follows; each constituent is expressed in the form R cos $(\sigma t-\epsilon)$.

	M_2	S_2	N_2	K_2	L_2
R	·060	·055	·166	·100	·027
ϵ	297°	87°	251°	87°	68°

The full list of constituents removed from the record for 1918 is now given: the results for the five constituents given above have been combined with the first approximations given in § 10.

	S_2	R_2	T_2	K_2	L_2	λ_2	M_2	2SM	ν_2
R	1·868	·020	·110	·502	·273	·047	5·636	·100	·190
ϵ	178·31°	2·00°	174·00°	320·41°	255·35°	52·00°	210·28°	323·00°	110·00°

	N_2	μ_2	2N	S_1	P_1	K_1	O_1	Q_1
R	1·094	·182	·130	·010	·071	·202	·182	·062
ϵ	338·78°	319·00°	132·00°	155·00°	118·00°	89·00°	78·00°	177·00°

There has also been removed the quarter-diurnal tide represented by the factor ·0225 and phase shift 96° applied to the quarter-diurnal portion of the square of the semi-diurnal tide.

The harmonic constants (H, κ) are as follows :—

	S_2	R_2	T_2	K_2	L_2	λ_2	M_2	2SM	ν_2
H	1·868	·020	·110	·503	·386	·047	5·621	·100	·189
K	189·4°	191·4°	186·8°	188·8°	156·9°	93·3°	145·8°	49·6°	119·8°

	N_2	μ_2	2N	S_1	P_1	K_1	O_1	Q_1
H	1·091	·182	·130	·010	·071	·200	·180	·060
K	122·4°	176·9°	123·7°	340·5°	113·7°	113·3°	345·3°	292·4°

These are obtained from (R,ϵ) by multiplying R by a certain factor depending upon the longitude of the moon's node, and by adding to ϵ the 'astronomical argument.'

PART III.

Tests on the Accuracy of Tide-predicting Machines.

In collaboration with the Hydrographic Department of the Admiralty and with the National Physical Laboratory, it has been possible to use the Newlyn calculations for a direct test on two tide-predicting machines. The Report for 1920 gives certain indirect tests where predictions from two machines are compared, and the absolute errors of each are unknown. The harmonic numbers used to represent the five chief semi-diurnal constituents for Newlyn were supplied to the Hydrographer and to Mr. Selby, of the N.P.L., for use with Mr. Roberts' machine (R) and the India Office machine (N) respectively. The curves for six months were forwarded to the Tidal Institute, where they were read and compared with the calculations.

Measurements of heights at intervals of four hours, and of heights and times of H. and L.W. were made on nineteen days at intervals of ten days, and every effort was made to ensure the greatest possible degree of accuracy in measurement. The machine scales were, in height, one inch to a foot, and, in time, one inch to four hours; consequently the gradients were sometimes very steep and measurements were difficult to make. The errors of measurement are essentially random, and not systematic, errors. The calculations are correct to within 0·01 foot in height and one minute in time. The errors are taken as machine value minus calculated value.

High and Low Water Heights (feet).

		Range of error	Average error	Zero error	'Throw' error
R-machine ...	H.W. errors	−·10 to −·24	−·18	−·15	−·06
	L.W. „	−·07 to −·21	−·12		
N-machine ...	H.W. „	·00 to −·13	−·07	−·03	−·07
	L.W. „	·09 to −·05	·00		

By the 'throw' of the machine is meant the distance from maximum to minimum in the tide. In these cases H.W. are systematically too low and L.W. systematically too high by about ·035 foot when the zero error is corrected.

High and Low Water Times (minutes).

		Range of error	Average error	Greatest range in one day
R-machine ...	H.W. errors	1 to 10	5·8	0 to 5
	L.W. „	0 to 10	4·8	
N-machine ...	H.W. „	0 to 12	5·2	2 to 13
	L.W. „	0 to 13	7·2	

Hourly Heights (feet).

		Average error	Maximum error
R-machine	rising tide	−0·24	−0·47
	falling „	−0·06	
N-machine	rising „	−0·15	0·45
	falling „	−0·09	

These tests indicate :—

(1) that both machines have zero errors in height, that of the R-machine being serious;

(2) that both machines have serious time errors averaging about 6 minutes;

(3) that the throw of the machines (or apparent range of tide) is deficient, but not seriously so;

(4) that the machines have no produced predictions with the accuracy required in research work.

The average (or zero) errors are easily allowed for, even if the machines are not mechanically adjusted. There are certain other errors which may be serious, *e.g.* the variation of the H. and L.W. time error from two to thirteen minutes in one day—apart from the zero error the performance of the R-machine is more creditable than that of the N-machine in this respect.

The tests have been made with the machine scales used commercially. The time scale could be altered if greater accuracy were desired. It may be stated that six mins. is represented by one-fortieth of an inch on the chart and that this is the average error with very careful reading.

Both machines agree better with one another than with calculations; but they are of the same type and were built by the same maker, so that it might be expected that similar faults would be present.

The above tests ought, perhaps, to be supplemented by others in which the machines are more fully used; the present tests have only used five constituents out of the twenty or thirty represented on the machines.

In connection with methods of research (Part II.) it may be remarked that the machines would have been useless for the purpose covered by the scheme for prediction (§ 12), and the labour of reading the curves with any pretence to accuracy is quite comparable with the labour of direct numerical calculations, but the results are not comparable in value.

Fuel Economy.—*Fourth Report of Committee appointed for the Investigation of Fuel Economy, the Utilisation of Coal, and Smoke Prevention* (Professor W. A. Bone,* *Chairman*; Mr. H. James Yates,* *Vice-Chairman*; Mr. Robert Mond,* *Secretary*; Mr. A. H. Barker, Professor P. P. Bedson, Dr. W. S. Boulton, Mr. E. Bury, Professor W. E. Dalby, Mr. E. V. Evans,* Dr. W. Galloway,* Sir Robert Hadfield, Bart.,* Dr. H. S. Hele-Shaw,* Mr. D. H. Helps, Dr. G. Hickling, Mr. D. V. Hollingworth, Mr. A. Hutchinson,* Mr. S. R. Illingworth, Principal G. Knox, Professor Henry Louis,* Mr. H. M. Morgans, Mr. W. H. Patchell,* Mr. A. T. Smith, Dr. J. E. Stead, Mr. C. E. Stromeyer, Mr. G. Blake Walker, Sir Joseph Walton, M.P.,* Professor W. W. Watts,* *and* Mr. C. H. Wordingham*).

Note.—* Denotes a member of the Executive Committee.

Owing to two unforeseen causes, namely (1) the unprecedentedly difficult and anxious industrial situation during the past autumn and winter, accentuated, as it was, by the stoppage of the coal mines in October last, and culminating in the great coal-strike of this year, and (2) the sudden and serious illness of the

Chairman in February last (now, however, safely passed through), necessitating his relinquishing all his professional work and public duties for three months, the Committee has not been able to fulfil the programme of work which it proposed a year ago. Accordingly, it cannot present this year anything in the nature of an extended Report, but must confine itself to a brief general statement as to the present fuel situation, and to the importance of its future work and plans in relation thereto.

The Coal Situation.—In its previous Reports, and particularly in the one presented at Bournemouth in 1919, the Committee has repeatedly warned the country of the serious economic dangers attendant upon the rapidly increasing cost of producing coal in British mines. Last year it published official statistics showing how rapidly our coal export trade was declining. Some important statistics upon these points were given in a paper upon 'The Economics of the South Wales Coalfield,' which Mr. Hugh Bramwell read at the joint meeting of this Committee with that of the South Wales Institute of Engineers at Cardiff on August 26, 1920. Among them were the following which, referring to a group of South Wales collieries, '*illustrate in detail the relation between the production per person employed per pit working day and the earnings per person employed, also per pit working day, for the past six years, plotted weekly, and averaged each six months*' :—

Period.	Duration.	Earnings per person employed per pit day.	Production per person employed per pit day.
		s. d.	Tons.
1	No. 1 Pay 1915 to No. 22 Pay 1916. Prior to 15 per cent. advance . .	7 1·30	0·768
2	No. 23 Pay 1916 to No. 37 Pay 1917. Period of 15 per cent. advance . .	8 11·40	0·758
3	No. 38 Pay 1917 to No. 26 Pay 1918. 1st War Wage addition . . .	10 8·79	0·742
4	No. 27 Pay 1918 to No. 1 Pay 1919. 2nd War Wage addition . . .	11 11·03	0·718
5	No. 2 Pay 1919 to No. 29 Pay 1919. Sankey Wage addition . . .	13 7·84	0·677
6	No. 30 Pay 1919 to No. 17 Pay 1920. Hours reduced 8 to 7 and piecework rates increased by 14·2 per cent. .	14 3·15	0·561
7	No. 18 Pay 1920. Additional 20 per cent. on earnings	—	—

On March 11 last the Secretary for Mines officially reported the following comparative statistics concerning our British coal industry in the years 1913 and 1920, respectively :—

Average Cost of Producing a Ton of Coal in Great Britain.

	1913	1920
	s. d.	s. d.
Wages	6 4	25 9½
Timber, Stores, and other costs	1 10	7 8
Royalties	0 5½	0 7¾
Total Cost . .	8 7½	34 1¼
Add Owners' profits	1 6	2 6½

Average Cost of Producing Coal—continued.

	Tons.	Tons.
Total coal raised at mines	287,412,000	229,295,000
Total coal raised per person employed at mines	259	190
Amount of coal shipped abroad:—		
As cargoes	73,400,118	24,931,853
As bunkers	21,031,550	13,840,360
Total	94,431,668	38,772,213

The position thus revealed may be summed up as follows: (*a*) The average cost of producing coal at the pithead had in seven years increased nearly fourfold, whilst (*b*) the amount raised per person employed had diminished by more than 25 per cent., and (*c*) our coal exports to foreign markets (excluding bunkers) had fallen to one-third the pre-war amount.

The rapidly increasing costs of producing coal in Great Britain have already reacted most detrimentally upon its finances and trading position. For some time prior to the recent strike, the cost of producing coal at the pithead had exceeded its selling price by several shillings per ton; the difference had been made up by a subsidy from the public funds, at the expense of the taxpayer. The crisis was precipitated by the decision of the Government that, after March 31 last, this subsidy must cease, and that the coal industry must revert to its former position of being self-supporting.[1] The strike came as a final blow to the country's industries, already crippled by intolerably high coal prices. The coal-export trade, which for some time had been rapidly declining, was brought to a standstill, with disastrous effects upon the shipping trade. It must also be remembered that coal is the principal mineral which this country has to export, and that in the past our coal exports have not only given British ships outward cargoes and freights, but have materially helped to pay for the large amounts of raw materials and foodstuffs which must be imported to maintain our factories and workers. The recent marked contraction in British coal exports, which in pre-war days easily dominated the overseas markets to our great advantage as a maritime Power, has coincided with a great expansion in American coal exports, which now seriously threatens our once unrivalled position.[2]

Whilst the Committee has steadily refrained from intervening in what may be regarded as the political aspects of the coal question, and therefore expresses no opinion as to the various *ex parte* proposals which have been put forward for the future organisation of the coal trade, it is nevertheless well within its province to urge the necessity, from the point of view of national economy and well-being, of a substantial reduction in the cost of producing coal in this country.

In this connection the Committee would draw attention to the weighty declarations made in May last by Sir Hugh Bell (as President of the Cleveland Mineowners' Association) and Mr. Alfred Hutchinson (one of the members of the Committee and President of the Cleveland Ironmasters' Association) to the effect that, even were the differences in the coal trade to be settled immediately, there could be no resumption of work in the iron and steel industry on the old scale without a very drastic reduction in coal prices. Wages, they said, are governed by a sliding scale; but even when wages have been reduced to bed-rock, Cleveland pig iron cannot be manufactured, excluding all question of profit, at the price at which foreign pig iron is being delivered into this country, unless coke of good quality can be delivered to the furnaces at about 27s. per ton. Mr. Maurice Deacon has recently pointed out that an even lower figure than this would be required if the iron trade of the Midlands is to continue in being. Without necessarily accepting any particular figure, the Committee would again emphasise the view, which it already expressed in its second Report—namely, the absolute dependence of the country's industrial system upon its ability to produce relatively cheap coal, which is, indeed, the keystone of its whole economic structure.

[1] When, however, the strike was finally settled on June 28 last, the Government agreed (subject to the consent of Parliament, which was afterwards given) to grant a sum not exceeding 10,000,000*l.* in subvention of miners' wages.

[2] *Vide* the Appendix to this Report.

Oil Fuel.—During the recent coal strike successful attempts have been made in several directions in this country to substitute oil fuel for coal. Oil has thus been used with good results in public electric power stations, for driving steam locomotives on the railways, and also in the case of several large ocean liners. So many advantages are claimed for oil as against coal firing, in regard to cleanliness, labour saving, and general efficiency, that the question of how far such substitution can be economically kept up or extended in future will depend largely upon the prospects of ensuring regular and adequate supplies of fuel oil at reasonable prices that can be established. It would undoubtedly be advantageous to the country if its power stations, railways, and other such public services could be rendered less dependent upon coal than they have hitherto been. Such a consideration makes it more than ever important that our future sources of supply of liquid fuel should be thoroughly explored, and therefore the Committee proposes in the immediate future to include such an inquiry in its programme of work.

Federation of British Industries Fuel Economy Committee.—The Committee has learned with much satisfaction of the establishment by the Federation of British Industries of a special Committee (of which Sir Robert Hadfield, Mr. H. James Yates, and Professor Bone are members) to assist manufacturers in economising coal in their operations, and of the good and effective work that it has already accomplished in this direction. The Committee hopes, through the said three members, who are common to both, to keep in touch with and help forward the work of this new Committee.

The Board of Trade Gas Committees.—Since the Committee was last reappointed, the Board of Trade, under powers conferred upon it by the Gas Regulation Act, 1920, set up two *ad hoc* Special Committees to deal with the important question of whether or not it is necessary or desirable to impose any limitation as to the amount of (*a*) carbon monoxide, and (*b*) incombustible constituents ('inerts') permissible in public gas supplies. As the Committee had, in its Second and Third Reports, already expressed the view that such limitations are desirable, it appointed a Sub-Committee to arrange for its views being formally represented to the Board of Trade Committee. Upon the question of carbon monoxide, the Sub-Committee had the advantage of conferring with Dr. J. S. Haldane, who expressed himself entirely in agreement with the Committee's views that the CO-content of a public domestic gas supply ought not to be allowed to exceed 20 per cent. Dr. Haldane himself gave evidence to this effect before the Board of Trade CO-Committee on February 10 last, and subsequently Mr. Robert Mond presented to it the considered views of this Committee upon the subject, in accordance with its previous Reports. Owing, however, to the Chairman's illness, no formal representation was made about the Committee's views as to the question of the limitation of 'inerts,' although the Board of Trade was aware that they were in agreement with the recommendations made in 1918 by the Fuel Research Board.[3]

Changes in Membership.—Since its last reappointment, Mr. W. B. Woodhouse has resigned from the Committee, owing to pressure of other work; and Mr. S. R. Illingworth, of the Treforest School of Mines (South Wales), has been co-opted as a new member.

Future Work.—In view of the serious position of the coal mining and consuming industries, of the increasing attention that is being given to the possible substitution of other fuels for coal, and therefore of the consequent continued need of a body of disinterested scientific experience and opinion that can be brought to bear upon the various aspects of the fuel question, the Committee asks for reappointment for another year, for the purpose of completing the investigations outlined in its Third Report a year ago, with a grant of 35*l.* The patronage and help of the Committee has also been requested in connection with the proposed Smoke Abatement Exhibition to be held in London, in March and April 1922, under the auspices of the Coal Smoke Abatement Society.

[3] Three members of the Committee (Messrs. E. V. Evans, D. H. Helps, and H. James Yates) were, however, of the opinion that, in view of the provisions of the Gas Regulation Act, 1920, for the future sale of gas on a thermal basis, gas undertakings should be allowed a free hand in regard to carbon monoxide and inerts.

APPENDIX.

Some Comparative Statistics for British and American Coal Exports for the Years 1913 *and* 1920 *respectively.*

In amplification of what has been said in the main Report about the threatened loss of our once dominant coal-export trade, the following comparative statistics (recently published in 'Imperial Commerce and Affairs,' Vol. II., No. 6, pp. 30-31) may be quoted :—

(1) In the year 1913 the United States exported altogether 20,708,582 tons of anthracite and bituminous coals, of which, however, only about five million tons went overseas; some 14,482,929 tons of bituminous coals went overland into Canada. In the same year Great Britain exported to other countries no less than 73,400,118 tons of coal (excluding ships' bunkers). Thus it would appear that in 1913 Great Britain sent overseas about fifteen tons of coal to every ton sent overseas from the United States.

(2) In the year 1920 the British coal exports (excluding bunkers) had declined to 24,931,853 tons, whilst those of the United States had increased to 39,215,030 tons, of which nearly 25 million tons went overseas. In other words, whilst our overseas coal trade had shrunk to about one-third of its pre-war dimensions, that of the United States had increased fivefold.

(3) The United States has already made great inroads into the European coal markets, selling 10,240,422 tons of bituminous coals (besides some anthracite) there in 1920. She has also almost captured the Central and South American coal markets, where we were once supreme; for whilst our coal exports to Central and South America had fallen from upwards of 17 million tons in 1913 to rather less than 3.7 million tons in 1920, those of the United States had increased something like tenfold.

London, June 30, 1921. WILLIAM A. BONE.

Non-aromatic Diazonium Salts.—*Report of Committee* (Dr. F. D. CHATTAWAY, *Chairman*; Prof. G. T. MORGAN, *Secretary*; Mr. P. G. W. BAYLY, *and* Dr. N. V. SIDGWICK. *Drawn up by* Prof. G. T. MORGAN *and* Mr. HENRY BURGESS).

RECENT investigations have shown that several series of non-aromatic primary amines possess in varying degrees the property of diazotisability. In certain cases the existence of a diazo-derivative is inferred from the property of coupling to form azo-derivatives or from the fact that the diazo-group can be replaced by other radicals such as chlorine, but in other instances diazonium salts have actually been isolated. These non-aromatic diazonium salts vary considerably in stability from the exceptionally stable diazonium salts of the pyrazole series to the explosive diazo-derivatives of the thiazole group. Orientation plays an important part in the stability of these compounds, as is shown below in the case of the pyrazole and pyridine derivatives.

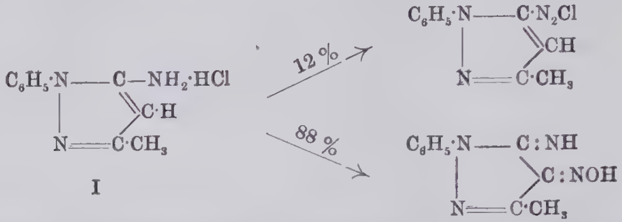
The requisite properties for diazotisability appear to be the presence of the group $H_2N-C\langle$ (with a double bond and a single bond on the carbon) and the possession of a certain degree of unsaturation in the cyclic system in which this carbon atom is included. But it must not be assumed that any base having the foregoing group and belonging to an unsaturated cyclic system is necessarily diazotisable. The absence of diazotisability is noteworthy in the thiophen, furane and pyrrole series in spite of the close relationship between the first of these series and the aromatic compounds.

Pyrazole Series.

In this group the effect of orientation on the stability of the diazonium salts is well marked. 4-Amino-3 : 5-dimethylpyrazole when diazotised in the usual manner furnishes 3 : 5-dimethyl pyrazole-4-diazonium chloride stable in hot aqueous solutions

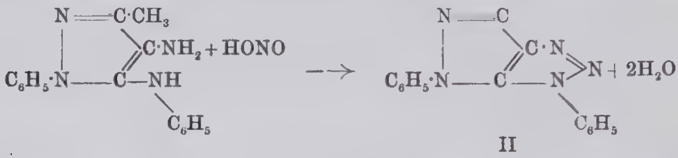
and up to 150° C. in the dry state. This salt, which retains its diazo nitrogen and coupling power for an indefinite time, gives rise to sparingly soluble diazonium platinichloride and aurichloride.[1] Other 4-aminopyrazole derivatives yield similarly stable diazo-derivatives.[2]

When the diazonium group is in position 5 the product is distinctly less stable, for 1-Phenyl-3-methyl-4-ethylpyrazole-5-diazonium chloride decomposes at room temperature in a few hours, and quickly on warming.[3] 1-Phenyl-3-methyl-5-aminopyrazole I containing a labile hydrogen atom in position 4 gives only 12 per cent. of diazonium compound.[3]

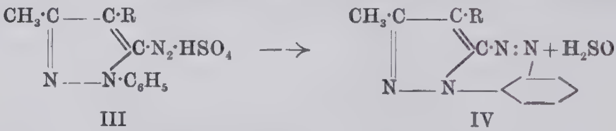


Substitution of the labile hydrogen by an alkyl group prevents the second reaction, and diazotisation takes place quantitatively.

In the case of 1-Phenyl-3-methyl-4-amino-5-anilinopyrazole an azimino compound II is formed unless diazotisation is carried out in strong acid.[4]

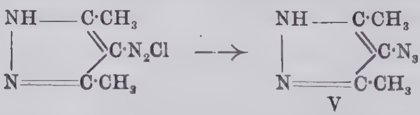


On adding a solution of any diazonium salt of the general formula III to boiling dilute sulphuric acid the diazonium nitrogen is retained and a triazine IV is produced.



R = an alkyl group.

In this series replacement of the diazonium complex by iodine[5] and by the triazo group has been effected. It is noteworthy that 4-triazo-3 : 5-dimethylpyrazole V, a well-defined crystallisable compound, has the remarkable property of giving distinctive colour reactions with all phenolic substances not containing nitro-groups.

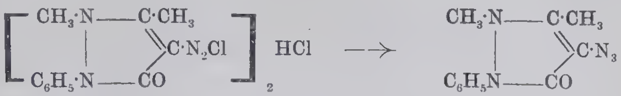


[1] Morgan & Reilly, *T.* 1914, **105,** 435. [2] Knorr, *B.* 1895, **28,** 717; Michaelis & Schafer, *A.* 1915, **407,** 229; Michaelis & Bressel, *A.* 1915, **407,** 274. [3] Mohr, *J. Pr. Chem.* 1914, **90,** (ii) 509. [4] Michaelis & Schafer, *loc. cit.* [5] Knorr, *loc. cit.*

Pyrazolone Series.

Very stable salts are obtained on diazotising the hydrochlorides of the aminopyrazolones; the products are crystallisable from hot aqueous solutions, and are stable in a dry state for an indefinite time.[1] The acid diazonium salts, VI, from amino antipyrine[2] are exceptionally stable and re-crystallisable and in these respects are

unlike the decomposable acid diazonium salts of the aromatic series.[3] Double salts with auric chloride and platinic chloride have been prepared and are stable up to 120° C. in the dry state.[2]



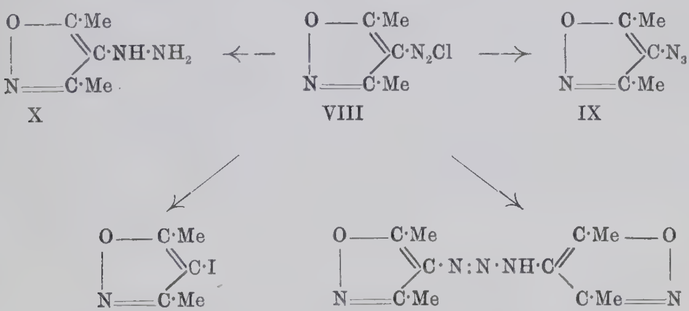
VI VII

Diazoamino derivatives are formed with fatty amines such as dimethylamine and are stable up to 120° C., when they begin to decompose into the amine and a complex pyrazolone derivative, the constitution of which has not been settled.[4] In this series, as in the pyrazoles, the diazonium complex has been replaced by iodine[5] and by the triazo group[6] VII, but reduction with stannous chloride does not lead to a readily isolated hydrazine, probably owing to condensation taking place with the carbonyl group in the ring.[2]

[1] Knorr & Geuther, *A.* 1896, **293**, 55; Knorr & Stolz, *A.* 1896, **293**, 58; Michaelis, *A.* 1906, **350**, 288. [2] Morgan & Reilly, *T.* 1913, **103**, 808, 1494. [3] Hirsch, *B.* 1897, **30**, 1148; Hantzsch, *ibid.* 1153. [4] Stolz, *B.* 1908, **41**, 3849. [5] Michaelis, *loc. cit.* [6] Forster & Muller, *T.* 1909, **95**, 2072.

Iso-oxazole Series.

4-Amino-3·5-dimethyl*iso*-oxazole has recently been prepared and diazotised. The diazonium chloride VIII is very soluble and is much less stable than the corresponding pyrazole compound. The diazonium aurichloride is sparingly soluble and stable on keeping in the dry state at the ordinary temperature.[1]



X VIII IX XI XII

The diazonium complex is readily replaced by the triazo group IX and by iodine XI. The diazonium chloride reacts, with the base forming a well-defined colourless diazoamine XII, and is reduced by stannous chloride to the hydrochloride of the hydrazine X.

[1] Morgan & Burgess, *T.* 1921, **119**, 697.

Glyoxaline Series.

The bases of this group have not been studied exhaustively as regards diazotisability, but 2-aminoglyoxaline when treated successively with nitrous acid and alkaline β-naphthol gave a brownish-red, soluble azo derivative, which does not appear to have been isolated.[1]

[1] Pyman & Fargher, *T.* 1919, **115**, 247.

Triazole Series.

When 5-aminotriazoles are diazotised in hydrochloric acid solution the resulting salt decomposes fairly quickly at 0° C. into nitrogen and the corresponding chlorotriazole.[1] If, however, oxy-acids are used instead of hydrochloric acid, more stable diazonium salts are obtained, although these are too unstable to be isolated.[2] The sparingly soluble diazonium aurichloride has been isolated and is stable in the dry state. When a solution of the diazonium nitrate is evaporated to dryness in vacuo over potassium hydroxide a colourless *iso*diazo hydroxide is obtained, which is quite stable at 100° C. and couples only after being acidified. The diazonium complex is replaced by hydrogen on reduction with alcohol, and with stannous chloride the hydrazine is obtained.[1]

[1] Thiele & Manchot, *A.* 1898, **303**, 33. [2] Morgan & Reilly, *T.* 1916, **109**, 155.

Thiazole Series.

2-Aminothiazole reacts in the same way as aminotriazole with nitrous acid. In hydrochloric acid solution an unstable diazonium chloride is produced. This decomposes rapidly at 0° C. into nitrogen and chlorothiazole. Oxy-acids give more stable salts, especially sulphuric acid. The perchlorate is explosive even at 0° C. The yellow diazonium aurichloride is sparingly soluble and is stable in the dry state but becomes brown on washing with water. In feebly acid solution aqueous sodium nitrite gives an unstable orange-red powder, probably an *iso*diazo compound.[1] Reduction of the diazonium sulphate with alcohol causes the replacement of the diazonium complex by hydrogen.[2]

[1] Morgan & Morrow, *T.* 1915, **107**, 1291. [2] Hantzsch & Popp, *A.* 1889, **250**, 274.

Pyridine and Quinoline Series.

Nitrous acid acts on the amines derived from these bodies in the same way. The α and γ amines when treated in hydrochloric acid solution yield forthwith the corresponding chloro derivatives. This reaction suggests a rapid decomposition of a probable intermediate diazonium chloride.[1] Oxy-acids have not been used and these would probably give better results. The β-amines form diazonium chlorides, which do not appear to have been isolated but have been coupled with phenols.[2] The corresponding $\beta\beta$-diazoaminopyridine is a yellow, crystalline, stable compound.[3]

[1] Marckwald, *B.* 1898, **31**, 2496; *B.* 1894, **27**, 1325; H. Meyer, *Monatsch.* 1894, **15**, 173; Claus & Howitz, *J. Pr. Chem.* 1894, (ii) **50**, 23. [2] Mohr, *B.* 1898, **31**, 2495; Watson & Mills, *T.* 1910, **97**, 743. [3] Mohr, *loc. cit.*

Photographs of Geological Interest.—*Twentieth Report of Committee* (Professors E. J. Garwood, *Chairman, and* S. H. Reynolds, *Secretary*; Mr. G. Bingley, Dr. T. G. Bonney, Messrs. C. V. Crook *and* W. Gray, Dr. R. Kidston, Mr. A. S. Reid, Sir J. J. H. Teall, Professor W. W. Watts, Mr. R. Welch, *and* Mr. W. Whitaker). *Drawn up by the Secretary.*

The Committee have to state that since the issue of the previous Report (Bournemouth, 1919) 223 photographs have been added to the collection which now numbers 6,069.

The most extensive set numbering 46 is one illustrating the Suffolk coast; this was presented some years ago by the late W. Jerome Harrison, but has not hitherto been listed. The Secretary contributes sets from the Bristol district, West Yorkshire and Galloway, and Mr. B. Hobson photographs from Cornwall, Devon, and various parts of Scotland and Ireland. Special mention must be made of an admirable series by Mr. J. Ritchie illustrating the Bennachie storm-burst of 1891. Mr. Ritchie also sends some excellent views from Banff.

The Committee are very glad to welcome new contributors in Mr. J. J. Hartley who sends prints from the Lake District and Jersey, in Mr. R. Parker Smith who illustrates subjects from Stafford and Cardigan, in Mr. E. C. Martin who sends an excellent and well-described set from Carnarvon, and in Mr. E. W. Tunbridge who contributes a valuable series from Pembroke. The Committee are much indebted to Mr. J. F. N. Green for further detail regarding some of the views from the Lake district and Pembroke.

Other prints have been sent by Mr. C. Buckingham and Mr. P. C. Dutton and a set of Scottish subjects has been printed from negatives taken by the late G. W. Palmer, M.A.

In their previous report the Committee expressed the hope that before the issue of the next report the new series of Geological photographs which had so long been under consideration would be published. With this end in view, a selection of suitable subjects was made, and circulars were sent out to former subscribers and to practically all the universities and other institutions likely to subscribe all over the world. Probably owing to the general impoverishment due to the War, the response was most disappointing, only thirty-five applications for the new issue having been received, while for the previous issue the subscribers numbered 235. Still fewer applications were received for the reissue of the earlier set. It is clear, therefore, that nothing can be done at present as regards a new issue, but it is intended to move again in the matter when conditions become more favourable.

The Committee recommend that they be reappointed.

TWENTIETH LIST OF GEOLOGICAL PHOTOGRAPHS.

From September 1, 1919, to August 31, 1921.

List of the geological photographs received and registered by the Secretary of the Committee since the publication of the last Report.

Contributors are asked to affix the registered numbers, as given below, to their negatives, for convenience of future reference. Their own numbers are added in order to enable them to do so. Copies of photographs desired can, in most instances, be obtained from the photographer direct. The cost at which copies may be obtained depends on the size of the print and on local circumstances over which the Committee have no control.

The Committee do not assume the copyright of any photograph included in this list. Inquiries respecting photographs, and applications for permission to reproduce them, should be addressed to the photographers direct.

Copies of photographs should be sent, unmounted, to

Professor S. H. Reynolds,
The University, Bristol.

accompanied by descriptions written on a form prepared for the purpose, copies of which may be obtained from him.

The size of the photographs is indicated as follows :—

L=Lantern size.	1/1=Whole plate.
1/4=Quarter-plate.	10/8=10 inches by 8.
1/2=Half-plate.	12/10=12 inches by 10, &c.

E signifies Enlargement.

England.

CORNWALL.—*Photographed by* B. HOBSON, *M.Sc., F.G.S.,* 20 *Hallamgate Road, Sheffield.* 1/4.

5846	(v.4) Gunwalloe . . .	Folded Veryan strata. 1913.

CUMBERLAND.—*Photographed by* J. J. HARTLEY, *Church Walk, Ambleside.* Postcard size.

5847	(5) Between Watch Hill and Elva Hill, Cockermouth.	Columnar felsite. 1920.
5848	(10) Caldew Valley . . .	Water-worn surface of altered and contorted Skiddaw slate. 1920.
5849	(11) Threlkeld	Slickensided fault plane in Microgranite. 1920.

DEVONSHIRE.—*Photographed by* B. HOBSON, *M.Sc., F.G.S.,* 20 *Hallamgate Road, Sheffield.* 1/4

5850	(263) Horse Cove, Dawlish .	Honeycomb weathering and fault in L. Sandstone (Permian). 1906.

DORSET.—*Photographed by* E. C. MARTIN, *B.Sc.,* 20 *Derby Road, Woodford, London, E.* 18. 1/4.

5851	(273) 'Fossil Forest,' Lulworth .	Tufa-coated tree stumps. 1910.

GLOUCESTER.—*Photographed by* Professor S. H. REYNOLDS, *M.A., Sc.D., The University, Bristol.* 1/2 *and* 1/4.

5852	(9·1919) Avon Section, N. end .	K-beds and top of O.R.S. 1/2. 1919.
5853	(48·1918) ,, ,, ,, .	Succession K_m to Z_1. 1/2. 1918.
5854	(49·1918) ,, ,, Press' Quarry	Thrust fault traversing horizon β. 1/2. 1918.
5855	(50·1918) ,, ,, Black Rock Quarry.	Succession K_2 to base of C_1. 1/2. 1918.
5856	(8·1919) Avon Section . .	Succession Z_1 to C_1. 1/2. 1919.
5857	(51·1918) ,, ,, from Sea Walls.	General view showing succession Z_2 to D_1. 1/2. 1918.
5858	(52·1918) ,, ,, from Sea Walls.	Ditto. 1/2. 1918.
5859	(54·1918) ,, ,, The Gully	Succession γ to C_2. 1/2. 1918.
5860	(55·1918) Avon Section, the Gully Quarry.	Succession *laminosa*-dolomite to *Caninia*-dolomite. 1/2. 1918.
5861	(7·1919) Avon Section, Black Rock Quarry to Great Quarry.	Succession Z_2 to S_2. 1/2. 1919.
5862	(58·1918) Avon Section, N. part of Great Quarry.	S_1 and lower part of S_2. 1/2. 1918.
5863	(56·1918) Avon Section, N. end of Great Quarry.	Lower S_1-beds. 1/2. 1918.
5864	(62·1918) Avon Section, Great Quarry and cutting to N.	Succession C_2 to D_1. 1/2. 1918.
5865	(59·1918) Avon Section, Great Quarry.	S-beds. 1/2. 1918.

5866	(60·1918) Avon Section, Great Quarry.	S-beds. 1/2. 1918.
5867	(61·1918) Avon Section, S. part Great Quarry.	Succession, top of S_1 to base of D_1. 1/2. 1918.
5868	(12·1919) Avon Section, Observatory Hill.	Succession S_2 to D_1 repeated by the Observatory Hill Fault. 1/2. 1919.
5869	(64·1918) Avon Section, Observatory Hill.	S_2-beds repeated by the Observatory Hill fault. 1/2. 1918.
5870	(65·1918) Avon Section, Observatory Hill.	S_2 and D_1 beds repeated by the Observatory Hill Fault. 1/2. 1918.
5871	(75·1918) Avon Section, Great Quarry.	Penecontemporaneous brecciation in *Seminula*-Oolite (S_2). 1/2. 1918.
5872	(76·1918) Avon Section, Great Quarry.	*Seminula*-Pisolite (base S_2). 1/2. 1918.
5873	(14·1919) Avon Section, Great Quarry.	*Seminula*-Pisolite (S_2). 1/2. 1919.
5874	(15·1919) Avon Section, Great Quarry.	*Seminula*-Pisolite (S_2). 1/2. 1919.
5875	(11·1920) Avon Section, N. end of Black Rock Quarry.	Small thrust faults in Z_1. 1/4. 1920.
5876	(20·1920) Avon Section, N. end of Black Rock Quarry.	Patchy dolomitisation in Z_1. 1/4. 1920.
5877	(21·1920) Avon Section, S. end of Black Rock Quarry.	Patchy dolomitisation in γ. 1/4. 1920.
5878	(9·1920) Avon Section, Great Quarry.	Bedding plane of shale covered with *Seminula*. 1/4. 1920.
5879	(22·1920) Avon Section, Great Quarry, N. end.	Algal Limestone in S_1. 1/4. 1920.
5880	(13·1920) Avon Section, Great Quarry.	'Stick bed' (S_1). 1/4. 1920.
5881	(3·1920) Avon Section, between Bridge and Old Zigzag path.	*Seminula*-Pisolite (S_2). 1/4. 1920.
5882	(15a·1919) Avon Section, Great Quarry.	Block of china-stone with 'worm tubes.' 1/4. 1919.
5883	(17·1920) Avon Section, Observatory Hill, Clifton, approach to Suspension Bridge.	Algal Limestone (S_2). 1/4. 1920.
5884	(6·1920) Avon Section, riverside exposure S. of Point Villa.	Pseudobreccia in D_1. 1/4. 1920.
5885	(24·1920) Avon Section, S. of Point Villa.	Band of pseudobreccia (D_1). 1/4. 1920.
5886	(25·1920) Avon Section, S. of Point Villa.	Pseudobreccia in D_1. 1/4. 1920.
5887	(8·1920) Avon Section, S. of Point Villa.	,, ,, ,, ,,
5888	(43·1920) Avon Section, S. of Point Villa.	Sandy pseudobreccia in D_2. 1/4. 1920.
5889	(19·1920) Avon Section, Observatory Hill, Clifton.	Bedding plane of 'Concretionary beds.' 1/4. 1920.
5890	(77·1918) Westbury-on-Trym, near Greenway Farm.	Block of Algal Limestone from top of C_2. 1/2. 1918.
5891	(22·1919) Near Bury Hill, Wickwar	Unconformity, Dolomitic Conglomerate on Carboniferous Limestone. 1/2. 1919.
5892	(20·1919) Knowle Quarry, Brentry	Concretionary beds (S_2). 1/2. 1919.
5893	(21·1919) ,, ,, ,,	,, ,, ,,
5894	(24·1919) Chipping Sodbury Quarry	,, ,, ,,
5895	(25a·1919) ,, ,, ,,	Algal nodules, 'Concretionary beds' (S_2). 1/4. 1919.
5896	(23·1919) Near Bury Hill, Wickwar	Algal nodules (S_2). 1919.
5897	(56·1905) Bridge Valley Road, Clifton, Bristol.	Coarse Dolomitic Conglomerate. 1/2 1905.
5898	(57·1905) Bridge Valley Road, Clifton, Bristol.	Coarse Dolomitic Conglomerate. 1/2. 1905.

5899 (58·1905) Bridge Valley Road, Clifton, Bristol. Coarse Dolomitic Conglomerate. 1/2. 1905.
5900 (59·1912) Avon Section, Great Quarry. Masses of *Lithostrotion martini* (S_1). 1/4. 1912.
5901 (11·1919) Avon Section, N. of Point Villa. D-beds and Dolomitic Conglomerate of Bridge Valley Road. 1/2. 1919.

Photographed by TAUNTS, *presented by the executors of the late* H. B. WOODWARD.

5902 Kemble Station, Cirencester . . Quarry in Great Oolite.

HAMPSHIRE (ISLE OF WIGHT).—*Photographed by* (purchased). Postcard size.

5903 ( ) Freshwater Bay and Cliffs . The gap in the Chalk escarpment due to Western Yar.
5904 ( ) The Cliffs, Chale . . Succession Chalk to Lower Greensand.
5905 ( ) ,, ,, ,, . . Lower Greensand Section.
5906 ( ) Near Blackgang . . Upper Greensand Section.

Photographed by E. T. W. DENNIS & SONS, LTD., *London and Scarborough* (purchased). Postcard size.

5907 ( ) Blackgang Chine, Ventnor Lower Greensand succession and features of a ‘ Chine.’

KENT.—*Photographed by* C. BUCKINGHAM, 13 *York Road, Canterbury.* 1/2.

5908 (169) Drillingore Nailbourne . Usual saturation level in Olkham Valley. 1904.
5909 (170) ,, ,, . Usual saturation level in Olkham Valley. 1904.
5910 (171) ,, ,, . Eroded road, Olkham. 1904.
5911 (172) ,, ,, . Source of 1903 and 1904 flow. 1904.

OXFORDSHIRE.—*Photographed by* E. C. MARTIN, *B.Sc.*, 20 *Derby Road, Woodford, London, E.* 18. 1/4.

5912 (301) Enslow Bridge Quarry . Clay band separating Forest Marble and Great Oolite. 1910.

SOMERSET.—*Photographed by* Professor S. H. REYNOLDS, *M.A., Sc.D., The University, Bristol.* 1/2 *and* 1/4.

5913 (72·1918) Avon Section (left bank) Block of Algal Limestone (Km). 1/2. 1918.
5914 (79·1918) Avon Section (left bank) Brecciated Algal Limestone (Km). 1/4. 1918.
5915 (81·1918) Avon Section (left bank) Block of Algal Limestone (Km). 1/4. 1918.
5916 (67·1918) Avon Section (left bank) Temporary spring in quarry 1. 1/2. 1918
5917 (69·1918) Avon Section (left bank) qu. 3. C_1 and lower part of C_2. 1/2. 1918.
5918 (78·1918) Avon Section (left bank) Block of ‘ Concretionary ’ beds (S_2). 1/2. 1918.
5919 (33·1920) E. of Gurney Slade, Mendips. Coarse Dolomitic Conglomerate on Millstone Grit. 1/2. 1920.
5920 (34·1920) E. of Gurney Slade, Mendips. Coarse Dolomitic Conglomerate on Millstone Grit. 1/2. 1920.
5921 (35·1920) E. of Gurney Slade, Mendips. Coarse Dolomitic Conglomerate on Millstone Grit. 1/2. 1920.

5922	(36·1920) Railway Cutting N. of Maesbury Station.	K_2 Section. 1/2. 1920.
5923	(40·1920) Moon's Hill Quarry, Stoke Lane, Mendips.	Quarry in Silurian Lava (pyroxene Andesite). 1/2. 1920.
5924	(37·1920) Waterlip Quarry . .	Bedding planes Z_2-beds. 1/2. 1920.
5925	(38·1920) Waterlip, near Shepton Mallet.	Chert in γ of main quarry. 1/2. 1920.
5926	(39·1920) Waterlip, near Shepton Mallet.	Chert in γ of the small quarry. 1/2. 1920.
5927	(41·1920) Mells Quarry . .	Bedding planes of pseudobreccia D_1. 1/2. 1920.
5928	(42·1920) Gurney Slade, Mendips.	Rhætic infilling in Carboniferous Limestone. 1/2. 1920.
5929	(27·1919) Long Ashton, near Bristol	Swallet near the golf course. 1/2. 1919.

Photographed by R. Vowell Sherring, *Hallatrow, near Bristol.* 10×8.

5930	( ) Gurney Slade . . .	Infilling of Rhætic left after removal of Carboniferous Limestone.
5931	( ) ,, ,, . . .	Infilling of Rhætic left after removal of Carboniferous Limestone.

Suffolk.—*Photographed by the late* W. Jerome Harrison. 1/2.

5932	(231) The Denes, Lowestoft .	Sandy strip accumulated N. of the Harbour. 1901.
5933	(262) Corton, cliffs 100 yards S. of the Gap.	Mid-glacial sand on loam. 1901.
5934	(263) Corton, the Gap, south side	Mid-glacial sand on loam. 1901.
5935	(264) Corton, 150 yards N. of the Gap.	Mid-glacial sands, 1901.
5936	(268) Corton, $\frac{3}{4}$ mile N. of the Gap	Glacial sands and loam, Forest Bed at beach level. 1901.
5937	(269) Corton, $\frac{3}{4}$ mile N. of the Gap	Surface of Forest Bed near beach level. 1901.
5938	(270) Corton, $1\frac{1}{2}$ miles N. of the Gap.	Glacial sands and loam. 1901.
5939	(272) Coast N. of Corton (looking S. from near League Hole).	Forest Bed below Glacial sands. 1901.
5940	(285) Corton, cliffs a little S. of the Gap.	Glacial sands. 1901.
5941	(286) Corton, cliffs 100 yards N. of the Gap.	,, ,, ,,
5942	(287) Corton, cliffs S. of the Gap	Mid-glacial sands on loam. 1901.
5943	(289) Corton, cliffs about 200 yards N. of the Gap.	False-bedded Mid-glacial sands on loam. 1901.
5944	(290) Cliffs N. of Corton Gap .	Glacial sands on loam with peaty Forest Bed at base. 1901.
5945	(291) ,, ,, ,, ,, .	Mid-glacial sands on loam. 1901.
5946	(297) Cliffs 150 yards S. of second Gap S. of Pakefield Lighthouse.	,, ,, ,, ,,
5947	(298) Cliffs S. of Pakefield . .	,, ,, ,, ,,
5948	(300) Cliffs S. of Pakefield . .	Mid-glacial sands. 1901.
5949	(301) Cliffs $\frac{1}{2}$ mile S. of Pakefield Lighthouse.	,, ,, ,,
5950	(302) Cliffs half-way between Pakefield and Kessingland.	,, ,, ,,
5951	(304) Cliffs N. of Kessingland .	Chalky boulder-clay upon Glacial sands. 1901.
5952	(305) Cliffs N. of Kessingland .	Chalky boulder-clay upon Glacial sands. 1901.
5953	(308) Pakefield Cliffs . .	Upper 2/3 of Section Mid-glacial sands. 1901.

5954	(311) Pakefield Cliffs 100 yards S. of the Lighthouse.	Mid-glacial sands. 1901.
5955	(312) Pakefield Cliffs, 400 yards S. of Lighthouse.	,, ,, ,,
5956	(314) Pakefield Cliffs . .	Mid-glacial sands. 1901.
5957	(315) Pakefield Cliffs, one of the Gaps.	,, ,, ,,
5958	(316) Pakefield Cliffs, S. side of a Gap.	,, ,, ,,
5959	(319) Pakefield Cliffs . . .	,, ,, ,,
5960	(320) Pakefield Cliffs . . .	Mid-glacial sands. 1901.
5961	(321) ,, ,, . . .	,, ,, ,,
5962	(322) Pakefield Cliffs, S. side of second Gap.	Mid-glacial sands on loam. 1901.
5963	(330) Pakefield Cliffs, S. side of Lighthouse Gap.	Loam at base of Mid-glacial sands. 1901.
5964	(332) Pakefield Cliffs, ¼ mile S. of the Lighthouse Gap.	Mid-glacial sands on loam. 1901.
5965	(333) Pakefield Cliffs, 500 yards S. of Lighthouse Gap.	Mid-glacial sands. 1901.
5966	(339) Cliffs 1½ mile N. of Kessingland.	,, ,, ,,
5967	(340) Cliffs S. of Pakefield . .	,, ,, ,,
5968	(343) Cliffs S. of Pakefield . .	False-bedded Mid-glacial sands. 1901.
5969	(344) ,, ,, ,, . .	Chalky boulder-clay resting on sand. 1901.
5970	(347) ,, ,, ,, . .	Mid-glacial sands. 1901.
5971	(348) ,, ,, ,, . .	False-bedded Mid-glacial sands. 1901.
5972	(349) Cliffs 100 yards N. of Pakefield Gap.	,, ,, ,, ,,
5973	(351) Pakefield Cliffs, The Gap, N. side.	,, ,, ,, ,,
5974	(352) Pakefield Cliffs . .	False-bedded Mid-glacial sands. 1901.
5975	(364) Pakefield Cliffs, N. end .	Westleton Beds. 1901.
5976	(365) Pakefield Cliffs, N. end .	Westleton Beds. 1901.
5977	(366) ,, ,, ,, .	,, ,, ,,
5978	(367) ,, ,, ,, .	,, ,, ,,

STAFFORDSHIRE.—*Photographed by* P. C. DUTTON, 65 *High Street, Stone.* 1/2.

5979	( ) S. of Oulton Mill, Stone .	Glacial Gravels and Clay on Keuper Sandstone. 1920.

Photographed by the late W. JEROME HARRISON. 1/2.

5980	(20) Midland Railway Cutting, near Aldridge.	Fossiliferous Wenlock shale. 1900.
5981	(21) Midland Railway Cutting, near Aldridge.	Fossiliferous Wenlock shale. 1900.

Photographed by R. PARKER SMITH, *Perse School, Cambridge.* 1/2.

5982	( ) Peadstone Rock, near Cheadle.	Keuper Sandstone cemented by barytes.

WARWICKSHIRE.—*Photographed by the late* W. JEROME HARRISON. 1/2.

5983	(2087) Chapel End, near Nuneaton.	Basal Carboniferous and Stockingford shales. 1898.

WESTMORLAND.—*Photographed by* J. J. HARTLEY, *Church Walk, Ambleside.* Postcard size.

5984	(6) Easedale Tarn . . .	Erratic of Concretionary Tuff. 1920.
5985	(7) Mardale, Ill Bell . . .	Columnar Andesite. 1920.
5986	(8) Cawdale Moor . . .	Harrath Tuff. 1920.
5987	(9) Red screes, Scandale Valley .	Banded ash and lava. 1920.
5988	(12) Garburn Pass . . .	Coniston Limestone and shale. 1920.

YORKSHIRE.—*Photographed by* Professor S. H. REYNOLDS, *M.A., Sc.D., The University, Bristol.* 1/4.

5989	(45·1919) Ingleborough from N. .	Great Scar Limestone plateau in foreground. 1919.
5990	(46·1919) „ „ .	Great Scar Limestone plateau in foreground. 1919.
5991	(47·1919) North-western flanks of Ingleborough.	Great Scar Limestone plateau. 1919.
5992	(48·1919) North-western flanks of Ingleborough.	Grikes, Great Scar Limestone plateau. 1919.
5993	(43·1919) Chapel-le-Dale, looking S.W.	Erosion valley in Carboniferous Limestone. 1919.
5994	(50·1919) S. of Gate Kirk, Chapel-le-Dale.	Dry water-course. 1919.
5995	(61·1919) Rowton Pot, Kingsdale	Large pot-hole. 1919.
5996	(51·1919) Great Dowk, N.W. flanks of Ingleborough.	Outflow of stream. 1919.
5997	(60·1919) Hull Pot, Penyghent .	One of the largest of the Yorkshire Pots. 1919.
5998	(59·1919) Hunt Pot, Penyghent .	Inner Chasm. 1919.
5999	(56·1919) Gaping Gill, Ingleborough.	1919.
6000	(66·1919) Norber, near Clapham .	Erratics of Coniston Grit on Carboniferous Limestone. 1919.
6001	(67·1919) „ „ „ .	Erratics of Coniston Grit on Carboniferous Limestone. 1919.
6002	(64·1919) S.E. of Horton-in-Ribblesdale.	Cleaved Silurians. 1919.
6003	(65·1919) Dry Rig, Horton-in-Ribblesdale.	Calcareous concretions in Horton Flags 1919.

Wales.

CARDIGANSHIRE.—*Photographed by* R. PARKER SMITH, *Perse School, Cambridge.* Postcard size.

6004	( ) Devil's Bridge, Aberystwith.	Pothole, River Mynach.
6005	( ) „ „	River Gorge.

CARNARVONSHIRE.—*Photographed by* E. C. MARTIN, *B.Sc.*, 20 *Derby Road, Woodford, London, E.* 18. 1/4.

6006	(506) Coast, $\frac{1}{2}$ mile S. of Borth-y-gest, Portmadoc.	'Hassock bedding' in Ffestiniog beds. 1912.
6007	(507) Fechan Point, Portmadoc .	Effect of sand blast on Maentwrog beds. 1912.
6008	(509) „ „ .	'Caves in the making' (1) in Lingula Flags. 1912.
6009	(510) „ „	'Caves in the making' (2) in Lingula Flags. 1912.
6010	(514) Hen Fynwent, Tremadoc .	Small thrust-plane subsidiary to the Penmorfa Fault. 1912.

6011	(526) Moel-y-gest, Portmadoc .	Escarpments N. of the Penmorfa fault. 1912.
6012	(530) Criccieth Castle . .	Boulder clay on head on Felsite. 1912.
6013	(531) ,, ,, . .	Boulder clay on head on Felsite (detail). 1912.

PEMBROKE.—*Photographed by* E. W. TUNBRIDGE, *Castel Froma, Leamington Spa.* 1/4.

6014	(123) St. Non's Bay, St. Davids	Precambrian and Cambrian Section. 1911.
6015	(126) Solva	Drowned valley and dolerite intrusion. 1911.
6016	(127) Newgale Sands . .	Millstone Grit with high seaward dip. 1911.
6017	(129) ,, ,, . . .	Storm-beach. 1911.
6018	(113) Ogof Golchfa, Whitesand Bay.	Menevian faulted against Pebidian. 1911.
6019	(111) Whitesand Bay, St. Davids	Blown sand on head on Boulder clay on Menevian. 1911.

Photographed by H. MORTIMER ALLEN, *Tenby.* 1/1.

6020	( ) Bullslaughter Bay, Stack Rocks.	Erosion of Carboniferous Limestone.
6021	( ) Huntsman's Leap, Stack Rocks.	Erosion along shale band in vertical Old Red Sandstone.

Scotland.

ABERDEENSHIRE.—*Photographed by* J. RITCHIE. *Hawthorn Cottage, Port Elphinstone, Inverurie, Aberdeenshire.* 1/2.

6022	(1) Bennachie, near Oyne . .	Beginning of trench made by cloudburst of August 2, 1891. 1891.
6023	(2) ,, ,, . .	View near head of trench. 1891.
6024	(3) Bennachie, near Oyne . .	Deepest part of trench. 1891.
6025	(4) ,, ,, . .	Upper part of gap in wood. 1891.
6026	(5) Bennachie, near Oyne . .	Erosion due to cloud-burst of August 2, 1891. Upper part of trench after four years' weathering. 1895.
6027	(6) ,, ,, . .	Lower part of trench after four years' weathering. 1895.
6028	(7) Bennachie, near Oyne . .	Deepest part of trench after four years' weathering. 1895.
6029	(8) ,, ,, . .	Upper part of gap in wood after four years' weathering. 1895.
6030	(9) Bennachie, near Oyne . .	Gap in wood made by Bennachie cloudburst of August 2, 1891, after four years' weathering. 1895.
6031	(10) 'Mithertap' Bennachie .	Weathering of granite.

ARGYLE.—*Photographed by the late* G. W. PALMER, *M.A., of Christ's Hospital.*

6032	( ) Kerrera Is. S. of Oban .	Joint cutting through pebbles of Old Red Conglomerate.

BANFFSHIRE.—*Photographed by* J. RITCHIE, *Hawthorn Cottage, Port Elphinstone, Inverurie, Aberdeenshire.* 1/2.

6033	(11) Cullen Bay	Raised coast-line. 1900.
6034	(12) ,, ,, . . .	The 'Red Craig' a raised sea-stack of Old Red Sandstone. 1900.

6035	(13)	Cullen Bay	Old Red Sandstone of Red Craig (detail). 1900.
6036	(14)	„ „	Cave in Old Red Sandstone of the Bore Craig.
6037	(15)	Cullen Bay . . .	Raised sea-cave in Old Red Sandstone. 1900.
6038	(16)	„ „	Natural Arch in Old Red Sandstone. 1900.
6039	(17)	Bridge of Alva . . .	River Gorge in Old Red Sandstone. 1906.

BUTESHIRE. *Photographed by* B. HOBSON, 20 *Hallamgate Road, Sheffield.* 1/4.

6040	(368) (J8)	N. of Drumadoon Point, Arran.	Dyke of Banded Felsite. 1907.

EDINBURGH.—*Photographed by* B. HOBSON, 20 *Hallamgate Road, Sheffield.* 1/4.

6041	(S6)	Salisbury Crags, Arthur's Seat.	Teschenite dyke in Carboniferous Sandstone. 1912.

FORFARSHIRE.—*Photographed by* B. HOBSON, 20 *Hallamgate Road, Sheffield.* 1/4.

6042	(S1)	Whiting Ness, Arbroath .	Fault in Old Red Sandstone. 1912.
6043	(S2)	„ „ „ .	„ „ „ „

INVERNESS.—*Photographed by the late* G. W. PALMER, *M.A., of Christ's Hospital.* 1/4.

6044	(1)	Canna	Bedded tuffs and dolerite sill.
6045	(2)	„	„ „ „ „
6046	(3)	Dun Beag, Canna . . .	Dolerite sills and volcanic conglomerate.
6047	(4)	„ „ „ . . .	Volcanic conglomerate and overlying dolerite sill.
6048	(5)	„ „ „ . . .	Volcanic conglomerate and overlying dolerite sill.
6049	(6)	Eigg	The Scuir from opposite the landing place.
6050	(7)	„	Part of S. face of Scuir.
6051	(8)	Blaven, Skye, from Loch Slapin.	Gabbro mountains.

KINCARDINESHIRE.—*Photographed by* B. HOBSON, 20 *Hallamgate Road, Sheffield.* 1/4.

6052	(R4)	Crawton, near Stonehaven.	Basalt on Old Red Conglomerate.

KIRKCUDBRIGHT.—*Photographed by* Professor S. H. REYNOLDS, *M.A., Sc.D., The University, Bristol.* 1/4.

6053	(32·1919)	Talnotry, N.E. of Newton Stewart.	Junction of Cairnsmore-of-Fleet granite with overlying Silurian grit. 1919
6054	(33·1919)	Talnotry, N.E. of Newton Stewart.	Junction of Cairnsmore-of-Fleet granite with overlying Silurian grit. 1919.
6055	(28·1919)	Cairnsmore-of-Fleet from Talnotry Road.	Great granite intrusion. 1919.
6056	(31·1919)	Cairnsmore-of-Fleet from near Creetown.	1919.

RENFREWSHIRE.—*Photographed by* B. HOBSON, 20 *Hallamgate Road, Sheffield.* 1/4.

6057	(389) (M5)	Cutting $1\frac{3}{4}$ mile W. of Lochwinnock.	Basalt dyke in Carboniferous.

WIGTOWNSHIRE.—*Photographed by* Professor S. H. REYNOLDS, *M.A., Sc.D., The University, Bristol.* 1/4.

6058	(37·1919)	Near Inverwell Point .	Bedding and cleavage of Silurian grits and slates. 1919.
6059	(38·1919)	,, ,, ,, .	Bedding and cleavage of Silurian grits and slates. 1919.
6060	(39·1919)	,, ,, ,, .	Contorted quartz veins in Silurian grit. 1919.
6061	(40·1919)	,, ,, ,, .	Calcareous concretions in Silurian grit. 1919.

Ireland.

GALWAY.—*Photographed by* Professor S. H. REYNOLDS *M.A., Sc.D., The University, Bristol.* 1/4.

6062	( )	Top of Curraghrevagh, Lough Nafooey.	Vertical Silurians. 1913.

LEITRIM.—*Photographed by* B. HOBSON, 20 *Hallamgate Road, Sheffield.* 1/4.

6063	(Q6)	Glencar	Valley due to landslip. 1912.

MAYO.—*Photographed by* B. HOBSON, 20 *Hallamgate Road, Sheffield.* 1/4.

6064	(P6)	Cathedral Rocks, Achill Island.	Marine erosion of bedded quartzites. 1912.
6067	(Q1)	Mallaranny Gap . .	Section of drift mound in railway cutting. 1912.

Channel Islands.

JERSEY.—*Photographed by* J. J. HARTLEY, *Church Walk, Ambleside.* Postcard size.

6068	(1)	Tête des Hougues . .	Cambrian basement conglomerate. 1921.
6069	(2)	St. Laurence Valley . .	Highly inclined Precambrian shale. 1921.
6070	(3)	St. Laurence Valley . .	Contorted Precambrian shale. 1921.
6071	(4)	Archirondel . . .	Columnar Rhyolite. 1921.

Zoological Bibliography and Publication.—*Report of Committee* (Professor E. B. POULTON, *Chairman*; Dr. F. A. BATHER, *Secretary*; Mr. E. HERON-ALLEN, Dr. W. E. HOYLE, and Dr. P. CHALMERS MITCHELL).

1. The circulation among editors of scientific journals of recent recommendations by this Committee (as desired by the Committee of Section D) has been favourably received by several of them, and has led to further correspondence.

2. Mr. Maurice Cossmann, on his own initiative, reprinted the Committee's circular in the *Revue Critique de Paléozoologie* for December 1920, which involved republication in the *Revue de Géologie*. A French translation prepared

by your Secretary arrived too late for this, and was, through Mr. Cossmann's kind intervention, printed in the Compte Rendu de la Société Géologique de France for February 7, 1921. Reprints of this French edition were furnished by the Société Géologique.

3. In the *Naturalist* for September 1, 1920, the chief recommendations were quoted and contributors asked to adhere to them. Several requests for further copies of the circular and for previous reports were received and complied with.

4. The editor of the Yorkshire Geological Society consulted the Secretary of the Committee on the correct way of writing specific names. Since the particular instances occurred in a paper on palæobotany, the reply sent was kindly read and approved by Dr. A. B. Rendle. Many otherwise competent zoologists seem unaware that an author's name should be enclosed in brackets, *e.g. Dalmanites caudatus* (Brünnich), only when the species has been transferred from the genus in which the author originally placed it, *e.g. Trilobus caudatus* Brünnich. It would be equally correct to write *Dalmanites caudatus* Brünnich sp.

5. There is also confusion in some minds as to the use of brackets in connection with generic and subgeneric names. The trilobite just mentioned was long placed in the genus Phacops; this fact may be indicated thus—*Dalmanites* [*Phacops*] *caudatus*. At first Dalmanites was regarded as a subgenus of Phacops, and this would have been indicated correctly by: *Phacops* (*Dalmanites*) *caudatus*.

6. A similar question was raised, and the opinion of the Committee asked, by Mr. J. C. Moulton, who, as editor of the R. Asiatic Society's Journal, Straits Branch, has adopted the following method of printing trinomials :—

CHLOROPSIS VIRIDIS Horsfield *viriditectus* Hartert. Here the essential departures from ordinary usage are the difference of type for the subspecific component of the name, and the insertion of the name of the author of the species after the specific component.

The Committee agrees that the alterations introduced by Mr. Moulton tend to increased clearness. If it be ever necessary to give the name of the author of the species, it is no less necessary when the form referred to is one of the subspecies into which the species has been divided, and Mr. Moulton's method of introducing it seems unexceptionable.

The Committee does not wish by this expression of opinion to encourage the insertion of authors' names in general writing, except when they are needed to avoid ambiguity. Mr. Moulton's devices are best suited for such systematic lists as those in which he has employed them.

7. In postage of the above correspondence the Committee has spent the sum of 4*s*. 9*d*., leaving an unexpended balance of 15*s*. 3*d*. The postage account is likely to be heavier in the coming year. The Committee therefore applies for its reappointment, with a grant of 1*l*. to cover such possible expenditure.

Abrolhos Islands.—*Report of Committee* (Professor W. A. HERDMAN, *Chairman*; Professor W. J. DAKIN, *Secretary*; Professors J. H. ASHWORTH and F. O. BOWER) *appointed to conduct an investigation of the biology of the Abrolhos Islands and the north-west coast of Australia (north of Shark's Bay to Broome), with particular reference to the marine fauna.*

The investigation of the Abrolhos Islands in the Indian Ocean (marine fauna and flora and formation of islands) was undertaken with the help of grants from the Percy Sladen Fund, the Government Grant Committee of the Royal Society, and the British Association.

Two expeditions were arranged and extensive collections were made in 1913 and 1915. The grants were exhausted with the exception of a small amount out of that made by the Government Grant Committee of the Royal Society. During the years 1915 to date the collections have been distributed to various specialists, some of whom have already reported upon the same.

Professor Dakin has written a narrative of the expeditions and a report upon

the structure of these coral islands. The report was published in the *Proceedings of the Linnean Society* for 1917. He has also written a report on a new Enteropneust from the islands, published in *Proc. Linn. Soc.*, 1916.

The vertebrate fauna has been reported upon by Mr. W. B. Alexander, M.A. (*Proc. Linn. Soc.*, 1921). A report upon certain crustacea by Dr. Tattersall is in process of publication, and Professor Dendy has the report upon the sponges well in hand. A report on the seaweeds has been received from Mr. Lucas, M.Sc., of Sydney. Professor Hickson has written a short account of the few sea pens captured. The Echinoderms, with the exception of the Holothuria, are now in America, and a report from H. L. Clarke is exported shortly.

The Polychæt worms have been investigated by Professor Fauvel, and this paper is now in the press (*Proc. Linn. Soc.*, 1921). Other collections are in the hands of Miss Thorpe, B.Sc., at Liverpool (Alcyonaria), S. Kemp, M.Sc., at Calcutta (certain Crustacea), Dr. J. Pearson at Colombo (Holothurians), and S. K. Montgomery, B.A., B.Sc. (Brachyura).

The remaining specimens are being distributed. The small amount of grant remaining has been allotted by the Committee of the Royal Society for aid in the publication of the papers.

The Committee does not apply for reappointment.

Experiments in Inheritance of Colour in Lepidoptera.—

Report of the Committee (Prof. W. Bateson, *Chairman*; Hon. H. Onslow, *Secretary*; Dr. F. A. Dixey, Prof. E. B. Poulton).

Diaphora (*Spilosoma*) *mendica* and var. *rustica.*—The experiments with this variety were concluded, a sufficient number of the F_2 generation having been obtained. The results confirm what has been previously reported; the incompletely dominant white variety segregating from the buff-coloured heterozygous as well as from the black type insects. A full report will appear in the *Journal of Genetics.*

Boarmia consortaria and var. *consobrinaria.*—A few more families were obtained, which point to the view that the melanic variety is hypostatic to the intermediate variety, and the latter hypostatic to the type; but the experiments are being continued.

Hemerophila abruptaria and var. *fuscata.*—The melanic variety behaves as a simple Mendelian dominant. The excess of melanics observed by Hamling and Harris must have been due to abnormal circumstances. In captivity a second brood may be obtained, and it was observed that a number of such insects, among which the mortality was very high, showed a considerable excess of melanic over type insects. Probably this excess of melanics was due to a mortality which favoured the black form. This supports the view that one of the main causes of the spread of melanic forms is due to their constitutional hardiness. A full account will appear in the *Journal of Genetics.*

Zygæna filipendulæ, and the yellow variety.—The families reared in 1920 indicate that the normal red colour is dominant to the yellow. The experiments are being continued, but the imagines of 1921 have not yet emerged.

Abraxas grossulariata and var. *varleyata.*—The black variety behaves as a Mendelian recessive. These experiments were completed, and a full report will appear in the *Journal of Genetics.* In addition, it was found that the black pattern of the type form is restricted to about one-third of the total area in the fore wings. The factor which causes the limitation of this black pattern is dominant to that causing completely black fore wings (var. *hazeleighensis*). This factor is connected with femaleness in such a way that the males always appear to have most pigment.

Abraxas grossulariata and var. *actinota.*—This is a radiated variety of the melanic form *varleyata.* It is sex-linked with maleness, and a female has not yet been bred. This is therefore the first case (except in *Drosophila*) in which a character has been found sex-linked both to maleness and femaleness—*i.e. lacticolor* and this radiated variety *actinota.* The method of inheritance is complicated and not yet understood, but it appears to be involved with a lethal

factor, which suggests an analogy with the case of 'notch' wing in *Drosophila*. The experiments will be continued.

Abraxas grossulariata vars. *lacticolor* and *varleyata*.—These experiments have been considerably extended, and it is hoped to carry them farther. The distribution of the sexes appears to agree with the suggestion made last year. As was expected, moreover, anomalous sex-ratios have been observed in the crosses *grossulariata*×*varleyata*, *grossulariata*×*lacticolor*, and *grossulariata*× *exquisita*, when the female parent is *grossulariata* and heterozygous for both *varleyata* and *lacticolor*. A full report will appear in the *Journal of Genetics*.

Zoology Organisation.—*Report of Committee, appointed to summon meetings in London or elsewhere for consideration of matters affecting the interests of Zoology, etc.* (Professor S. J. Hickson, *Chairman*; Dr. W. M. Tattersall, *Secretary*; Professors G. C. Bourne, A. Dendy, J. Stanley Gardiner, W. Garstang, Marcus Hartog, W. A. Herdman, J. Graham Kerr, Mr. R. D. Laurie, Professors E. W. MacBride and A. Meek, Dr. P. Chalmers Mitchell, and Professor E. B. Poulton).

MEMORANDUM ON THE TEACHING OF NATURAL HISTORY IN SCHOOLS.

Prepared by the Zoology Organisation Committee at the request of the Committee of Section D.

'Make the boy interested in Natural History if you can; it is better than games; they encourage it in some schools.'—(*Last words of* Captain R. F. Scott, *Antarctic explorer.*)

It does not need any profound investigation of the various Matriculation and School-leaving Examinations to be convinced that the great majority of our boys and girls when they leave school at the age of seventeen or eighteen years are almost entirely devoid of any sound knowledge of the structure and physiology of the animal body or the elementary principles of the science of animal life.

It is true that in ordinary conversation they will use such words as brain, heart, liver, or kidneys as if they were familiar with their meaning. They will talk about the birds, the fishes, the reptiles, or even the worms and corals, as things they know something about in a general way, and in the course of time they will gather some information from various sources concerning all these things, which enables them to pass muster as educated men and women.

But what the schoolmasters, as a rule, do not realise, and educational authorities invariably do not realise, is that boys and girls have not been given the educational opportunity they need to enable them to gain an intelligent interest in their own bodies and in the animate world with which they come into daily contact.

The tragic events of the last six years have brought home to the minds of most educated persons the national importance of scientific training and research, and an effort has been made to remove, to some extent, our national neglect of Science. There has been a larger endowment for scientific research, new laboratories have been built and old laboratories enlarged for the study of Chemistry, Engineering, Agriculture, and other Sciences in our Universities, and in many of the schools a little more encouragement is given now to the boys and girls who 'take the modern side' than before the War. But as regards the teaching of Natural History and the animal side of Biology there has been

no improvement in the schools, and there are strong reasons for believing that the situation is steadily becoming worse.

It is true, as Captain Scott said, that 'they encourage it (*i.e.* Natural History) in some schools,' but in a great many of these the encouragement that is given to boys or girls with a taste for the subject and a determination to pursue it amounts to little more than toleration. They are allowed to make collections of butterflies and shells, or even to keep a few live pets under conditions that are usually unfavourable for a healthy existence, and in some cases there are prizes or awards for collections or observations made in the holidays. But the most lamentable thing about it is that there are so few masters or mistresses, and these only in some of the larger schools, whose education and training enable them to give any sound guidance or assistance to boys or girls in their study of Animal Life.

It does not need many years of experience as a teacher in a University to discover that the graduates who have taken a degree with Honours in Zoology are not in demand for masterships in secondary schools, and it is only on rare occasions that they succeed in getting good posts.

There is a demand for botanists, and apparently the demand is increasing; but there can be no doubt that, at the present time, botanists who can give instruction in two other subjects such as Chemistry and Mathematics, or Chemistry and Physics, have a better chance of making a successful application for teaching posts in secondary schools than the botanists who have also had a good training in Zoology.

The effect of this neglect of the teaching of Animal Biology in the schools is reflected in the Universities, in which we find large classes of students attending the higher courses of introduction in Biology and very small classes attending the corresponding courses in Zoology. Students coming up to the Universities from our secondary schools naturally suppose that it will be to their advantage to continue the study of subjects in which they have already received some preliminary introduction, and the longer they have remained at school after reaching the Matriculation stage the less is their inclination to start a new subject. Moreover, students in deciding upon a career at the Universities must be influenced, in most cases, by the opportunities the career offers for earning a living when they have graduated.

It is not surprising, therefore, that our boys and girls, having received no systematic instruction whatever on the animal side of living organisms at school, and finding that the study of Zoology offers very few opportunities, under our present system, for obtaining positions as teachers, either in schools or in Universities, very seldom choose Zoology as a subject of University study. There is, indeed, in this respect a vicious circle in our educational system: on the one hand, the masters and mistresses in our schools, the members of the governing bodies, and indeed His Majesty's school inspectors, with very rare exceptions, almost entirely ignorant of the first principles of Biological Science, and therefore inclined to discourage the subject in the schools; and, on the other hand, the Universities unable, for many reasons, to attract sufficient numbers of students to the courses in Zoology and Animal Physiology even to the standard of an ordinary degree.

One can understand that the masters and mistresses of schools, troubled with the multiplicity of subjects and the expense of scientific laboratories, should be inclined to welcome the discouragement of another science subject, but it is astounding that any body of educational experts, asked to consider the educational needs of the country, could issue such a report in respect to Zoology as that published by the Secondary Schools Examination Commission under the authority of the Board of Education.[1] The report in question has reference to the first examinations only, that is to say, to examinations corresponding to the standard of the Senior Local Examinations of the Cambridge University Syndicate or the Matriculation Examinations of the Northern Universities Joint Matriculation Board, examinations which are usually taken by boys and girls of sixteen to seventeen years of age.

[1] Secondary Schools Examinations Council.—Report of the Investigations of the First Examinations. Subjects Reports, Group III., p. 10. H.M. Stationery Office. 1919. Price 4*d*.

The reports of the Investigators of the Higher Certificate Examinations have not yet been published.

The passage in the report to which grave exception must be taken is as follows :—

'Very few of the candidates offer this subject (Natural History), and it seems very doubtful whether it is worth while to maintain it as qualifying for a Pass with Credit in Science. The principles of Biological Science can be better illustrated by means of Botany, especially as Physiology occupies a far more important place in this subject than in Zoology, which does not readily lend itself to experimental treatment.'

The Science of Biology, as the word is now used, is the Science that deals with living things, animal and vegetable, and it is difficult to understand how the principles of the subject can be taught, even in the most elementary stages, in a course of study from which the problems and features of animal life are entirely excluded. The study of Botany can afford illustrations of the life of plants, but it cannot give correct or reasonable instruction in the principles of Biology. If a student has attended such a course of Botany, is it possible that he could understand anything that could reasonably be called the first principles of Biology, when he is entirely ignorant of the structure and functions of the heart, the nervous system, the sense organs, the locomotor organs, and the reproductive organs of animals? Many of us who have had long experience of the teaching of Biology are convinced that the conceptions of the principles of Biology that such a student gains are both incorrect and misleading. From the standpoint of first principles, the whole life of a green plant, from its dependence on so highly specialised a substance as chlorophyll, is rather an anomaly than a self-sufficient illustration. There may be some truth in the statement that Zoology does not lend itself so readily to experimental treatment as Botany; but even in this respect a properly trained teacher can devise valuable experiments upon animals which do not involve injury to or death of the subjects of the experiments. It is perfectly simple, for example, to demonstrate colour changes in the skin of living frogs, by comparing two specimens, one of which has been kept in a dark cupboard and the other in the sunlight for half an hour, to show the reactions of different kinds of protozoa to light and heat, to demonstrate the beating of the heart of a daphnia, or the circulation of the blood in a tadpole or a worm under the microscope, without injuring it or sacrificing its life.

It is equally feasible to apply exact, if qualitative, experimentation to the study of the many respiratory and environmental adaptations of aquatic animals such as crabs, molluscs, and many insects and their larvæ. There are, in fact, many ways in which Zoology even at this stage can be treated as an experimental science with the simplest apparatus.

But it is not in this aspect of the subject that its chief educational training lies. Its place in the curriculum is advocated because it is pre-eminently the subject that can be used with effect for education in careful observation and comparison and inference.

At the very beginning of school life it has the advantage that nearly all boys and girls possess a natural interest in the living, moving things they see around them, and consequently it is an easy matter to catch and to keep their attention for a whole school period. From this beginning it is not difficult to proceed to successful exercises in independent observation, and from these to simple deductions as to the why and wherefore of organic structures.

And to the older student, what science can present problems of such enthralling interest? Which can give so furiously to think as Zoology, with its various theories of evolution and heredity, bearing as these do alike on the past and the future of man, with the universal drama of sex, the almost incredible intricacy of the 'web of life,' and, above all, the picture of the living organism, most delicate and fragile of living objects, standing for ever poised upon the brink of destruction, yet, thanks to the life that is in it, master of its fate? Surely this is a subject to be taught if intellectual stimulus be needed.

In the hands of competent teachers the subject is one which can be used with great advantage, not only as a training for the faculties, but also as a

means for imparting that knowledge of the simpler anatomical structures and elementary physiological principles of the animal body which should be a part of the equipment of every man and woman.

Many strange objections have been brought forward in the past against the introduction of the teaching of Natural History of animals in schools. Perhaps the most serious of these is the statement that there is not time in the school curriculum for the introduction of another subject, that the time-table is already overloaded with subjects, and that it is most undesirable that the number of school hours should be increased. This is an objection that it is difficult to answer without a full discussion of the present school curriculum, but from our knowledge of what can be done in the way of giving some sound teaching of Zoology in a few schools at the present time it does not appear to be an objection that is insuperable.

It is a curious fact that in England alone, among civilised countries, a boy and girl can reach the age of eighteen or nineteen years and leave school without having received any school instruction in animal physiology or the natural history of animals. In Japan, to take only one example out of many, the courses in the middle school (fourteen to nineteen years of age) include Botany, Physiology, and a two years' course in Zoology, and the official text-book for these schools shows that the standard aimed at, if not acquired, is a very high one—far higher, indeed, than that of any school in this country. And this instruction is given not only to the few scholars that are passing on to a specialised course in Science in the Universities, but to all scholars without exception.

It would be too much to expect, perhaps, that our standard of education in this respect should reach that attained in Japan immediately, but we are convinced that a few periods a week could be spared for the subject at all stages in the curriculum without impairing the educational value of the other subjects.

A second objection that is frequently raised is that the expense of providing the necessary equipment for the efficient teaching of the subject is beyond the means of the average secondary school. We think that this objection has been greatly exaggerated. Provided that there is a well-lighted laboratory, which can also be used for Chemistry and Physics, a few microscopes, which can also be used for Botany, the expenses for purely zoological teaching are not great. There should be a fresh-water aquarium, and in schools by the sea a sea-water aquarium as well, a few simple dissecting instruments and lenses, and a few prepared skeletons and other preparations which can be increased as time passes. The difficulties of obtaining specimens are not great if the teachers keep in touch with the larger departments in our Universities, and many specimens can be obtained in sufficient quantities from the fields and ponds in the neighbourhood of the schools if occasional field excursions are organised.

It is even suggested as an objection that the subject cannot be studied without inflicting pain upon animals. To this we reply most emphatically that in the school classes no vivisection and no cruelty to animals is necessary or desirable. In fact, the knowledge the boys and girls gain of the structure and organisation of animals counteracts the desire that many may possess to crush and kill the creeping things they do not understand. Knowledge does not stimulate hatred and cruelty, but does create love and sympathy. If the boy knew something about the wonderful structure of the fly or wasp that he squashes on the window-pane, he would hesitate to strike.

We plead boldly for the teaching of Zoology as an antidote to cruelty to animals, as a basis for a more general and sympathetic appreciation of Nature, and as an indispensable approach towards a sound understanding of human life itself.

Of other objections that have occasionally been raised, passing reference must be made to that which suggests that it is objectionable to refer to questions of sex in animals. To most of us, who are interested in the subject, the fact that Zoology does provide authoritative instruction on the physiology of sex in animals is one of the strongest arguments in favour of the introduction of the subject in schools. We lay stress on the fact that it is a matter of universal experience that when the phenomena of sex are taught in a series from the

unicellular animals through the simpler invertebrata to the higher animals all sense of monotony or repugnancy disappears, while the knowledge acquired is clear and precise. In this respect Botany cannot take its place. The phenomenon of sex-reproduction should be regarded as one of the most necessary, and in some respects the most important, features in the general education of our boys and girls, and we attach great importance to the study of Animal Biology as the only means by which it can be adequately taught.

As the question has frequently been asked what should be the scope of the teaching of Natural History in schools up to the standard of the first school leaving examination, we have ventured to draw up the following schedule of subjects based on a practical knowledge of what has been done and can be done.

1. The principal characters of some of the more important divisions of the animal kingdom which can be observed by a study, without dissection, of a number of selected types such as—

The sea anemone and a simple coral.
A snail or a whelk.
A whiting or a fresh-water fish.
A lizard.
A bird or a rabbit.

The study of the movements and habits of living animals should be encouraged as far as possible by observations on such animals as can be kept in an aquarium, such as Daphnia, Cyclops, Planarian worms, water snails, insect larvæ, small fishes, and in the case of seaside schools, sea-anemones, marine worms, crabs or prawns, limpets, periwinkles, and various zoophytes.

A simple terrarium can also be devised for the study of living insects, spiders, earthworms, snails, frogs, and reptiles.

2. A more detailed study of the general anatomy and of the functions of the principal organs of such types as—

Amœba or Paramecium.
Hydra.
The earthworm.
Cockroach.
Frog.
Dogfish and rabbit.

This study will require the use of the microscope and of dissections which could be made by the masters or the older boys under the direction of the master. The functions of the organs can be explained by the master in the course of the exercises, and by demonstrations which do not involve experiments on living structures.

3. The study of the development of the frog by direct observation of the spawn and tadpole stages in the spring, and in schools provided with an incubator, the first three days of the development of the chick can be studied with advantage.

The study of the metamorphoses of the butterfly, moth, harlequin fly, or gnat can also be studied practically at this stage.

In regard to junior pupils the Committee would endorse the suggestions elaborated in the Scottish Education Department's 'Memorandum on Nature Study,' which advocate the advantages of following the seasons and trying to understand their prominent features. This method is particularly applicable to country schools, but it has among its advantages that of bringing different sciences—Chemistry, Physics, Botany, Zoology—to bear on what is going on in the natural world outside. It is very important to suggest early that the various sciences work into each other's hands.

The Effects of the War on Credit, Currency, Finance, and Foreign Exchanges.—*Report of Committee, consisting of* Prof. W. R. SCOTT (*Chairman*), Mr. J. E. ALLEN (*Secretary*), Prof. C. F. BASTABLE, Sir E. BRABROOK, Dr. J. H. CLAPHAM, Dr. HUGH DALTON, Mr. B. ELLINGER, Sir D. DRUMMOND FRASER, Mr. A. H. GIBSON, Mr. C. W. GUILLEBAUD, Mr. F. W. HIRST, Prof. A. W. KIRKALDY, Mr. F. LAVINGTON, Mr. D. H. ROBERTSON, Mr. E. SYKES, and Sir J. C. STAMP.

EVEN at the distance of thirty-two months after the Armistice it is not possible to write with certainty on economic conditions during the War and the reconstruction period. Nevertheless, we have reached a certain amount of agreement on some of the principal points which have arisen during our inquiry.

Our Committee endorses the memorandum submitted by the five economists [1] to the Economic Conference at Brussels. Also the recommendations of the Commission on Currency and Exchange,[2] which were approved unanimously by the Economic Conference. These recommendations cover a considerable part of our questionnaire, *e.g.* Credit, Inflation, and the Gold Standard.

Our first question, *How far is the rise in prices in the U.K. since July* 1914 *due* (*a*) *to expansion of the currency and* (*b*) *to expansion of credit?* like the second, is evidently controversial. Economic opinion is still divided as to the relative share of credit expansion and currency expansion in causing the rise in prices since July 1914.

The majority of our members and correspondents believe that the expansion of *credit* was the main cause. As Sir J. C. Stamp writes: 'A relatively small part is due to the currency, but the greater part is due to the expansion of other credit instruments.' Sir Edward Brabrook writes: 'I take it that both causes contributed to the rise in prices, but I cannot say in what proportions.' Sir Drummond Fraser 'Has no doubt whatever that the rise in prices was mainly due (*a*) to the expansion of the currency and (*b*) to the expansion of credit.' Mr. Hirst thinks that 'the expansion of currency and the expansion of credit interact, *i.e.* one is sometimes the cause, sometimes the effect of the other.'

Mr. Lavington and Mr. Robertson (avoiding the difficulty of defining 'currency' or 'money') say that the difference between pre-war and post-war prices 'was due, in the main, to expansion of the *instruments of payment*,' they add, in agreement with Sir J. C. Stamp, that it also was due (perhaps to the extent of 10 per cent.) to a falling-off in production. Mr. Ellinger qualifies this by limiting it 'to the time of the Armistice or a little later.'

Dr. Cannan holds that the rise in prices was 'due to Government expenditure of money on a scale which could not have been reached without a diminution in the purchasing power of money. This diminution would have come about, to a large extent, even if no U.K. paper money had been issued, owing to the demonetisation of gold abroad, which reduced the value of gold. The U.K. paper money enables it to be carried further.'

Mr. Sykes draws a distinction between the expansion of the currency abroad, where it has been 'the principal cause of the increase of prices,' because it was issued by Governments in direct payment of their debts, and the increase of currency in the U.K., which has been 'almost entirely a consequence of the expansion of credit.' Without such additional currency the banks could not have met the increased potential demand for cash which the increase in the volume of credit placed in the hands of the banks' customers. Mr. Sykes believes that if the additional currency had not been created by the Government 'a voluntary emergency currency would almost certainly have been brought into existence by mutual consent of all parties.'

[1] Dr. Bruins and Professors Cassel, Gide, Pantaleoni, and Pigou.

[2] Mr. Robertson comments: 'These seem to me very difficult to apply in present circumstances.'

In our first Report, which was drafted during the summer of 1915, and presented to the Association at its Manchester meeting in September, we quoted a sentence from the memorandum of our late colleague, Sir R. H. Inglis Palgrave, as expressing the views which are taken by most economists. Sir Robert wrote : 'The effect of an increase in the paper currency on prices, if sufficiently large, is invariably to raise prices, in the same way as any other increase of the circulating medium when this is not called for by an increase in the business done.' In our interim report last year, which was not printed, we said : 'As the War went on there was a fairly constant increase of the note issue, while the gold reserve, after an early period, remained stationary at 28,500,000*l*. If the money which the Government obtained by its war loans had been subscribed entirely out of real savings the loan policy would have had no effect on prices; but from an early period the banks were encouraged to take up war loans and to make advances to their customers for the same purpose, thereby causing an inflation of credit. This inflation of credit was made possible by the increase of the currency, and was itself a cause tending to currency expansion.'

Mr. Gibson holds that 60–70 per cent. of the rise in prices up to December 31, 1919, was due to monetary influences (increase in legal tender, bank deposits, &c.). Some allowance, too, must be made for increased velocity of circulation. He reckons that up to December 31, 1919, 80 per cent. of the rise due to monetary influences was caused by expansion of bank credit and 20 per cent. to the fiduciary part of the currency note issue (these being the relative proportions between the increases of bank credit and the fiduciary part of the currency note issue).

QUESTION 2.—*Is the expansion of credit the cause or the effect of the expansion of the currency?*

The answer to our second question may almost be inferred from the answer to the first. As Mr. Robertson and Mr. Lavington put it, the expansion of credit was, in the main, the cause or the antecedent of the expansion of the currency. But, as they say, 'a readiness to expand the currency was a *necessary condition* of the expansion of credit, if the banking system was not to be allowed to go to smash.' Sir Edward Brabrook thinks that 'it is both a cause and an effect.'

Mr. Ellinger thinks that the expansion of currency came first; 'had it not come the expansion of credit could hardly have followed'; moreover, the subsequent expansion of credit would not have taken place 'unless manufacturers of credit had known that further expansion of currency would automatically follow.'

Mr. Hirst and Mr. Pethick Lawrence hold that the two things are interconnected. Commander Hilton Young holds that the expansion of currency is a *consequence* of the expansion of credit; Mr. D. M. Mason and Mr. Alfred Hoare say it is the *cause*.

Prof. Cannan declares that 'There has been *no* expansion of credit when you measure credit in an undepreciated standard.'

Dr. Hugh Dalton argues that the increase of credit could not have taken place without the increase of currency 'in this, the most fundamental, sense, increase of credit is an effect and not a cause of increase of currency, for if,' he says, 'the British Government, determining to deflate the currency, were to withdraw a number of currency notes from circulation and destroy them, it is evident that bank loans would have to be reduced in roughly the same proportion.'

Dr. Dalton goes on to say : 'This reduction would be brought about by a rise in the bank rate and market rate.'

Mr. Sykes objects to this statement and contends that 'banks might refuse to lend without increasing rates.' Dr. Dalton had gone on to argue that 'if the Government is unwilling to see the volume of credit restricted by high rates of interest, it is possible to maintain this volume above what it would otherwise be, and the banks' rate of interest below what it would otherwise be, by an increase in the volume of the currency. This is the policy which the British and other Governments have, in fact, pursued during recent years. In this sense the increase of currency has been the effect, not of an increase of credit, but of

the Government's policy in relation to credit and interest rates. There are other forms of credit besides bank loans, *e.g.* the book debts of private traders. The volume of these depends primarily upon the state of business confidence, but drastic deflation of currency, by causing a fall in prices, would cause a restriction of this kind of credit also.'

Sir Drummond Fraser writes : 'Some portion of the expansion of the currency was undoubtedly due to the expansion of credit. But I doubt whether the two are entirely interdependent. Take two extreme cases : When the Austrian Government was short of money the currency was expanded. When the British Government was short of money credit was expanded. In Austria the increase in the note issue was not accompanied by a *pro rata* increase in the bank deposits. In Great Britain the increase in bank deposits was not accompanied by a *pro rata* increase in the note issues. The expansion of currency in a country like Austria is readily accomplished, because the note issues remain in circulation. The expansion of credit in Great Britain was readily accomplished because of the effectiveness of deposit banking.'

Mr. Gibson also holds that the expansion of credit was the main cause of the expansion of the currency. But the first could not have taken place without breaking up a part of it into legal tender units for wages and retail transactions. 'Currency notes have not been forced into circulation; they have always been returnable to the banks at their full face value to the credit of customers.' Of course, some issue of paper money was required to balance the gold coins withdrawn from circulation.

Mr. G. Bernard Shaw answers Questions (1) and (2) together : 'The two are really the same. Expansion of credit is effected by issuing more currency than you have goods to back it with; in short, by inflation. Credit is one of the economic phantoms.'

Question 3.—*Was it possible, in this and other countries, to prevent the expansion of credit and currency?*

This question raises the further question, 'Should a war be paid for by loans or by taxation?' and the answer depends partly upon the view which is taken of the patriotism of non-combatant citizens, partly on their power and will to pay. Opinions differ as to the extent to which higher taxation could have been imposed, but we agree that considerably higher taxation might safely have been imposed at an earlier period of the War. Such taxation would have tended to check personal extravagance, to lessen the inevitable rise in prices, and to decrease the future burden of the War Debt. It might have helped also to abate the demand for war bonuses, which were themselves both an effect and a cause of higher commodity prices. Something more might also have been done, in 1914 and 1915, to attract savings or special profits into the Exchequer by means of continuous borrowing rather than by, or in addition to, spectacular periodic loans.

Dr. Dalton answers the original question—'Yes; by heavier taxation (or alternatively, to some extent, by offering higher rates of interest on voluntary loans). This policy, if it had been adopted in this country, would have prevented the greater part, if not the whole, of the rise in prices and money wages, and of the depreciation of the American and other exchanges. Especially in the early part of the War, it was a gross error of policy not to impose heavier taxation.' Mr. Hirst, Commander Hilton Young, Mr. Mason, and Mr. Bernard Shaw also answer Yes. Sir J. C. Stamp answers : 'Theoretically Yes, but psychologically No, the stimulus given to profit-making by the expansion is too important an ingredient for waging the War to have been left out.' Dr. Dalton comments : 'This doesn't say much for the patriotism of the business community.' Mr. Allen observes : 'A Napoleon may be compelled to mislead his countrymen, a self-governing community ought to face a war with a full knowledge of its costs and dangers. To take a classical analogy, the choice was between the policy of Pericles and that of Cleon; and Cleon's won.'

Sir Edward Brabrook says: 'It was not possible to prevent it.' Mr. Sykes takes the same view, for he doubts 'whether any Government which attempted to prevent the expansion of currency and credit would have withstood the strain, even if it were abstractly possible.' He mentions a further difficulty—'How could Great Britain have financed international purchases for war pur-

poses without an expansion of credit?' Perhaps we may draw a line, as suggested by Dr. Scott (in 'Economic Problems of Peace after War,' Second Series, page 56), between borrowing for purchases at home and for purchases from foreign countries, the second being a legitimate and unavoidable expansion.

Mr. Robertson comments on this paragraph, 'I don't feel sure that this is relevant to *inflation*, only to the different question of loans and taxes.' Mr. Lavington, too, cannot see the validity of the distinction.

Sir Drummond Fraser writes: 'The expansion of credit and currency can be prevented in this and other countries, if the money required by the Governments is raised from taxation and loans direct from the people. Great Britain has proved this. During the four hundred days on which National War Bonds were on tap—from October 1917—the whole of the home money borrowed was raised from the day-to-day proceeds from the Bonds and Savings Certificates. This wholesale transfer of purchasing power from the people to the Government, without monetary inflation, not only arrested but actually reduced the hitherto continuous rise in prices. In my opinion it was an error of judgment not to have increased taxation in the early part of the War and not to have borrowed direct from the people at a higher rate of interest than that paid for the money borrowed on the money market. The practical result was shown in the swollen bank deposits and swollen currency notes, which ought to have been tapped by a Government security of the nature of National War Bonds.'

Mr. Robertson says 'not in most countries, to any very material extent, granted that the War was to be carried on. In war the preservation of morale frequently involves a certain measure of illusion.' It was certainly the practice of the European Governments to assure their citizens that the cost of the War would be paid by the defeated enemy. Mr. Ellinger thinks that, in order to prevent expansion of currency, it would have been 'necessary (*a*) to increase taxation, and (*b*) to conscript Labour and Capital, including the Labour of supervision and organisation.' He thinks that the first might have been done in this country to a limited extent, also in France and Germany, but he doubts whether the conscription of Capital and Labour could have been carried out in any country.

Mr. Allen writes: 'There can be no doubt, I think, that all Governments ought to have increased their taxation at an early period of the War, just as the American Government did, when at last it took up arms. We must recognise that Mr. McKenna's two Budgets (September 22, 1915, and April 4, 1916) mark an immense advance on everything that was done by European Finance Ministers. Subsequent Budgets have made no appreciable improvements in Mr. McKenna's scheme of taxation.'

Professor Cannan holds that the expansion of credit could have been prevented 'by not issuing it.' He adds, 'Of course this might have stopped the War; but that isn't economics, but politics.'

Compulsory service raises the question of the conscription of wealth. Clearly, it ought not to have been possible for people who were, for any reason, whether age, sex, occupation or physical disability, free from the risks and discomforts of military service, to make fortunes or even to improve their economic condition out of the misfortunes of their country. The Excess Profits Duty was a necessary result of the postponement of táxation of 1914–15, when the Government created so much new purchasing power. It ought to have reduced the purchasing power of the public by higher taxation.

As we said in our interim report for 1920, no one has explained why the ordinary Budget at the beginning of the financial year 1915–16 did not add to taxation. 'It should have been clear that non-combatants could not make their usual demands on the national output of goods and services, if the requirements of the fighting forces were to be supplied. Consequently it was desirable that the simplest of all checks on consumption, *i.e.* taxation, should have been applied. Unfortunately the Government seemed to have other views, and "business as usual" was the popular cry.'

Mr. Lavington, taking the view expressed some time ago by Sir Drummond Fraser and Mr. Gibson, with which Dr. Dalton agrees, thinks that the expansion of currency could have been appreciably reduced had we adopted earlier

the more effective methods of borrowing, *e.g.* continuous borrowing stimulated by advertisement, which were put into force later.

Mr. Gibson replies: 'Theoretically Yes, practically No.' In the absence of a General Service Act the Government had to allay discontent and to encourage increased production by increasing purchasing power. 'Workpeople wanted higher wages.' Dr. Dalton comments: 'They wanted higher money wages, when prices rose. I doubt if, had there been no inflation, they would have pressed for higher real wages.'

Mr. Gibson also controverts Dr. Dalton's replies. Admitting that heavier taxation would have curtailed consumption, he does not think that an extra 25 per cent. could 'have been equitably distributed over all classes without causing further demands for increases of wages and the breaking up of a large number of the homes of the fixed-income class.'

Dr. Dalton answers: 'I entirely disagree. If prices rise 100 per cent., that is equivalent to an *additional* income-tax of 10*s.* in the pound on the "fixed income class." Inflation hit them hardest of all.'

Mr. Gibson continues: 'Under scarcity conditions, during the War, producers, manufacturers, and traders were in a position to pass on increased taxation to the consumer.' He denies also that higher interest rates would have attracted any *considerable* further amount of loans towards the end of the War. Why this was so he explains at some length as follows: 'Before the War the aggregate of credit balances at banks was fairly evenly divided[3] between deposit accounts and current accounts (balances due to manufacturers, traders, and other business concerns). At the end of 1919 the relative proportions had changed to 1–2, or in some banks 1–3. This change was due to Government disbursements mainly finding their way to manufacturing and trading accounts. Only bank officials were in the position to note the great change in the distribution of credit. When manufacturers were urged to subscribe large sums, they replied that, on account of the rise in prices and increase of wages, they required considerably more liquid capital than in pre-war times.[4] Also that they were nursing their liquid capital for the expected world-wide trade boom after peace was declared.'

QUESTION 4.—*How is the taxable capacity of a nation ascertained? Has it been reached and passed, as Mr. McKenna suggests, in the case of Great Britain?*

Opinions on this question are bound to vary. Mr. Robertson gives our collective view when he deprecates 'any language which suggests that the taxable capacity of the nation is an absolute amount.' Evidently there must be *some* limit, though one cannot do more than suggest symptoms which point to the conclusion that the taxable limit is being approached. Much depends upon the purpose for which the Government imposes taxation, and upon whether the money taken by the tax-collector is spent inside or outside the country.

Sir Edward Brabrook thinks that taxable capacity 'cannot be ascertained.' Mr. Robertson replies: 'Almost any taxation has *some* deterrent effect on enterprise and the accumulation of capital; the question at any time is whether this deterrent effect is justified by the importance of the objects—national defence, fulfilment of obligation, or improvement of the quality of the population—to which that part of the nation's income which is spent communally is devoted. This is a question which, from its nature, admits of no precise statistical answer.'

Mr. Shaw thinks that it cannot be ascertained 'without reference to a minimum standard of subsistence for the population, and a distinction between productive and destructive activity. There is no limit for productive activities except the limit of the citizen's capacity for production. The State may take all he produces if it supports him.'

[3] Mr. Lavington says that these were usually taken to be in the proportions of 1 to 2. Mr. Gibson replies that the proportions vary in different banks according to the meaning given to the words 'deposit account,' that his remarks apply to provincial banks and not to bank accounts in London.

[4] Dr. Dalton comments: 'But if there had been heavier taxation and less inflation, money wages and prices would have been lower.'

Mr. Hirst replies: 'It would not be easy to set a limit. If the Government could spend its money as profitably and productively as individuals, I should say, *in that case*, the revenue might safely exceed half the total aggregate incomes of the people.'

Dr. Cannan writes: 'Nohow, because it is relative to the system of taxation, disposition of the taxpayer, &c., and you can never tell whether you have the best possible conditions in this respect for raising taxation.' Mr. Lavington suggests that 'taxable limits are set by political and by economic considerations. On economic grounds alone it may be said that the taxable limit is reached when further taxation would inflict an injury on the community greater than (1) that inflicted by alternative methods of raising that revenue, or (2) that resulting from the abandonment of the purpose for which the revenue was required.' Mr. Ellinger finds the limit, 'when so much is taken out of the taxpayers' pockets that their incentive to produce is reduced, and when insufficient remains to provide the necessary capital to make up for wastage and to set to work new workers in an increasing population.' He does not think it has been reached here, especially after the abolition of the E.P.D.

Dr. Dalton draws a distinction between (*a*) the proceeds of taxation spent within the country, and (*b*) those spent in making payments outside. In the case of (*a*) he does not believe that the taxable capacity, 'as measured by a mere sum of money, or even by a percentage of the national income, can be ascertained at all.' It depends upon what particular taxes and what particular forms of public expenditure it is proposed to increase (or reduce). Dr. Dalton wishes to draw a further distinction between taxation devoted to repayment of internal debt and taxation devoted to paying for the destructive operations of war, or between a tax on spirits and a tax on the necessities of life or on savings. In the case of making payments outside the country 'the problem may be regarded as that of determining what is the maximum amount, or percentage, of the national income which can be taken away and be handed over to foreigners, without reducing the future national income.'

Mr. Sykes thinks we must face 'the possibility or probability of a democratic Government being compelled to bow to popular opposition to an increase of taxation above an uncertain limit.' Again, we have to remember that the results of excessive taxation may only be felt in a gradual economic deterioration, which may not be recognised for years. Mr. Sykes sees a need 'for more definite information in regard to the incidence of modern forms of taxation.' He cannot agree with Mr. McKenna's statement that the necessity for borrowing to pay taxes is an indication that the limits of taxation have been reached or exceeded.

Mr. Hilton Young answers: 'Directly, by a census of production only; indirectly, according to the fancy of the payer, as to what he likes to call a fair measure of it.'

Mr. Pethick Lawrence thinks 'that no definite basis can be laid down.' Mr. Mason answers: 'When it affects production unduly.' Mr. A. Hoare thinks that 'taxable capacity can only be ascertained by experimenting with taxes; Mr. McKenna was wrong.'

Mr. Gibson thinks that the limit depends mainly on the distribution of taxation, but also on the level of prices and the general willingness to work harder. He does not think that it has been reached yet, 'provided there be increased production.' But taxation should be directed against those who made fortunes out of the War, with a corresponding remission for people with 'fixed' incomes.

Sir J. C. Stamp replies: '(*a*) By reference to the total surplus of production over the minimum of consumption that is functional or necessitated by the volume of that production. (*b*) This surplus must be considered in relation to the number of inhabitants, and the proportion in which it is distributed. (*c*) It depends also upon the manner in which the taxes are raised. (*d*) There can be no absolute answer, because it depends upon the reasons for, or subjects upon, which the money is to be spent. (*e*) There is a much larger capacity if the money is to be applied to the payment of interest, and a very much larger capacity if it is to be applied to the payment of internal debt.

'It has not been reached and passed for this country, as suggested by Mr. McKenna, if these facts are borne in mind. He has treated interest and debt just as they would be treated if the same money was spent on armaments.'

Dr. Dalton agrees with (*e*).

Sir Drummond Fraser writes: 'The taxable capacity of a nation is surely reached when taxpayers are forced to borrow from the banks to pay the taxes. Excessive taxation for the heroic repayment of debt can be reduced if the Government provide for the annual redemption of debt by a statutory sinking fund of one-half per cent. in addition to the interest. This would necessitate a bond continuously on tap to replace bonds falling due or bonds accepted in payment of taxes. Thus the time for repayment would be spread over a longer number of years, and the Government debt would be spread over a larger number of people. Lancashire and Manchester give a striking example of the advantages of this financial policy. The cotton mills for forty years have been mainly financed by the short-term loans of the people. The Manchester Corporation for thirty years has also been financed in this way. All the money required has been obtained, in spite of strikes and other handicaps, and in spite of spectacular stunts for Government war borrowing at a higher rate of interest! The atmosphere thus created not only adds to public interest, but secures efficiency.'

QUESTION 5.—*Is there any economic basis for the old idea of a balance between direct and indirect taxation?*

Our members and correspondents are almost unanimous in saying, as Sir Edward Brabrook puts it, that 'there is no necessary relation between direct and indirect taxation.' Dr. Cannan, Dr. Dalton, Mr. Hirst, Mr. Hoare, Mr. Pethick Lawrence, Mr. Mason, Mr. Robertson, all say 'No.'

Sir J. C. Stamp answers: 'In theory, No; but in the practical collection of taxes from the less wealthy classes, Yes.' Mr. Hilton Young replies: 'It was a good enough way of distribution over all classes before the War. No scientific justification, nothing but a rough approximation.'

Dr. Dalton writes: 'Those who speak of a "balance" generally assume (*a*) that direct taxes are paid by the rich and indirect by the poor, and (*b*) that the totals paid by the rich and poor should remain in some constant proportion. But (*a*) is not necessarily true, for, *e.g.* an income tax on small incomes is paid by the poor, and taxes on luxuries are paid by the rich. As to (*b*), even if (*a*) were true, the proper distribution of the burden of taxation, whatever that may be, may require a change to be made in the previously existing proportion, even if the relative number and wealth of rich and poor do not change, and *a fortiori* if they do.'

Mr. G. B. Shaw replies: 'No; only a psychological basis. If men will revolt against a direct tax of threepence, and will without protest pay 1*s.* for three-halfpence worth of tobacco, direct and indirect taxation must be balanced accordingly.'

Mr. Gibson thinks that 'the theoretical proportions must necessarily vary with changing economic conditions and changes in the distribution of the national income. The fixed-income class suffers least by additions to indirect taxation. If taxation be direct, producers, manufacturers, and traders pass part of it on to the consumer; consequently the fixed-income class receives double doses.'

Dr. Dalton disagrees with Mr. Gibson's last sentence, and has some doubt whether the distinction between direct and indirect taxation has any use.

Mr. Allen writes: 'Is not this an inversion of the usual law, which says that direct taxes stick where they fall, while indirect taxes are passed on to the consumer? It is not easy to say whether some taxes—*e.g.* the Excess Profits Duty—are direct or indirect, and it is possible that wage-earners and the salaried class would make an addition to their income tax a ground for claiming higher pay. In a primitive or partly-developed country it is natural for a Government to raise its revenue by taxing commodities. In the highly developed civilisation of to-day taxes on ordinary articles of consumption seem out of date; they can have little relation to the ability of the taxpayer, and among people with small incomes they become a tax on families.'

Mr. Lavington writes: 'I imagine that Mr. Gibson is right in holding that producers may shift *a part* of direct tax; but I greatly doubt if this is of any practical importance. Fixed-income classes could also shift such tax *a little* by restricting their savings and so slightly raising the rate of interest.'

Mr. Sykes observes: 'I think that there is no foundation for this law; it

is just as easy to pass on a direct tax as an indirect tax if *circumstances are favourable.*'

In reply to the above criticism, Mr. Gibson expresses his opinion that direct taxes, such as the income tax, do not 'stick where they fall' should the person directly taxed be in a position to pass on the tax to other shoulders. To support this opinion he gives the following illustration :—

Imagine a wool merchant in normal times to be making 5000*l*. a year, after payment of income tax. Next imagine direct taxation to be increased to such an extent as to cause the merchant's net income to be reduced, say, to 4000*l*. The merchant will widen his percentage gross profit on future sales in an endeavour to restore his customary net income and standard of living. The same applies to other traders, particularly retailers, who find their net incomes reduced by increased direct taxation. To what extent it will be possible to pass the increased direct taxation on to the consumer will depend on the strength of competition, home and foreign.

For this and other reasons Mr. Gibson states he has never been in favour of a direct tax on wages. As consumers, wage-earners indirectly pay part of the direct taxation levied on manufacturers, merchants, retailers, and other classes who are in a position to pass part or the whole of increased direct taxation on to the consumer.

Referring to the Excess Profits Duty, Mr. Gibson writes : 'It was a direct tax, though it became indirect on the consumer. The report of the Committee appointed to investigate the prices, costs, and profits of the manufacture of Yorkshire tweed cloths contained the following statement (Cmd. 858, p. 4) : "In practice we find that Excess Profits Duty is added by manufacturers to the prime cost of the article, and is an important factor in putting up prices." '

QUESTION 6.—*Has the value of indirect taxation been lessened by the great increase in the number of Government employees, and by the acceptance of the principle that wages and salaries should rise as the cost of living rises? Is the last principle valid?*

This question has been rather misunderstood. The idea was that if the wages of Government employees rose with the cost of living, while the Government was employing millions of people during the War, it was little use to put taxes on commodities, because to do so would be to raise the cost of living and so increase the Government's expenditure.

Mr. Hilton Young replies : 'Undoubtedly it has. Now that wages are so sensitive to the cost of living it is vain to try to load the burden on to the wage-earning class by means of indirect taxes. They can pass it straight on.' Dr. Cannan takes the opposite view of the first part of the question; but he answers the second part—'Certainly not; it's a kitten chasing its tail!' Mr. Hirst (perhaps misunderstanding the intention of the question) replies : 'I do not see why Government employees should not smoke and drink as much as private employees. When the cost of living rises in consequence of capital having been wasted in war, an attempt to raise wages and salaries in proportion to the rise in the cost of living is bound to end in unemployment and disaster.'

Mr. Robertson, drawing a distinction between the effect of inflation on prices and that of broader economic causes, replies : 'It is reasonable that a rise in prices arising out of the expansion of the instruments of payment should be followed by a roughly proportionate rise in money wages and salaries; otherwise, those whose money incomes fluctuate with prices—*i.e.* the recipients of business profits—make a gain at the expense of the other classes. But it is not reasonable that the recipients of wages and salaries should never be called upon to bear a share, in the shape of rising prices, of a general national burden (occasioned, *e.g.* by war or a general decrease in the productivity of industry) except in cases where their incomes were already so low as to be barely compatible with decency or efficiency. The general argument for raising part of the revenue by well-devised indirect taxes is not, therefore, destroyed by the fact that of recent years money wages have been progressively raised in order to compensate for the effect of expansion of the instruments of payment.

How far are such taxes, even though desirable, rendered ineffective by the fact that they are refunded in part to railwaymen, Government employees, and others, whose money wages vary with the price of commodities? I have not studied this question, but I should have thought that (1) the articles taxed

or likely to be taxed are either not included at all or play a comparatively small part in the indices of price-changes on which wage-changes are based, (2) that the proportion of taxpayers affected by such arrangements is still comparatively small; I should not imagine, therefore, that any weighty arguments against the continuance or increase of indirect taxation could be founded upon these considerations.'

Sir Edward Brabrook replies: 'The cost of living is not the only or the principal element in the determination of the rate of wages.'

Dr. Dalton writes: 'I am not sure that I understand the first part of the question. Evidently, if the prices of certain commodities, which enter into the cost-of-living index-number, are raised by taxation, and if wages and salaries are linked to the cost of living by means of a (proportionate) sliding scale, the effect of these taxes on wages and salaries will be neutralised. But (*a*) tobacco and alcohol are not included in any British cost-of-living index-number; (*b*) the difficulty, if there is one, works both ways, for if the prices of tea, sugar, &c., were reduced by remission of taxation, wages and salaries on a simple cost-of-living sliding scale would also be reduced; (*c*) the difficulty can easily be got over, as in some recent wage arbitrations, where money wages based on the cost-of-living index-number had been reduced by an allowance for a typical wage-earner's contribution to national taxation.

'The principle of making wages and salaries rise (and fall) with the cost of living is not, in any general sense, "valid." But it is convenient as a means of reducing unrest and wage disputes during periods when the price-level is undergoing rapid changes chiefly due to monetary causes. In effect, wage-bargains are made, not in terms of money, but in terms of purchasing power, and during such periods the revisions required in real wage-bargains will be smaller, and therefore more easily brought about, than the revisions which would be required in money wage-bargains. Compare the present state of the railways, where money wages move on a sliding scale, with that of the coal mines, where they do not.'

Mr. Pethick Lawrence, dividing the question into three, says 'No' to (*a*), 'Yes' to (*b*), and 'Yes' to (*c*). 'For small salaries, so long as there is a margin of taxable capacity elsewhere, but as the salary rises it should not completely apply.' Sir J. C. Stamp replies: 'To some extent. The principle is not wholly valid. It is true that wages should rise and fall with prices, but not to an equal extent or range—only enough to preserve the *proportion* of the actual total volume of production as the reward of any single service. So far as prices are up because production is 20 per cent. down, then real wages should be 20 per cent. down—*i.e.* money wages should rise only to 80 per cent. of the price-rise.'

Mr. Mason says 'Yes.' Mr. Bernard Shaw replies: (*a*) 'It depends on whether the employees are productive or not. Two postmen and a costermonger will yield as much indirect taxation as two costermongers and a postman. But if you substitute sinecurists or soldiers for postmen, taxation on their consumption is illusory. (*b*) In the case of subsistence wages, Yes, obviously. Necessity is always valid.'

Mr. Allen writes: 'In my opinion the "principle" ought never to have been accepted. Surely it is hardly possible that any large section of the population should be able to maintain their pre-war standard of living during a really big war? It is no less evidently unfair that a second section of the population should have to sacrifice their lives, and that a third section should have to sacrifice a large part of their income while the first section makes no sacrifices at all. No doubt there are people on the margin of subsistence who cannot be asked to give up their bare necessities of life, unless the country is in a state of siege. But they may be asked to work harder. In any case it seems most inequitable, as well as politically inexpedient, that Government employees should have enjoyed (*a*) special exemption from military service and (*b*) war-bonus additions to their wages and salaries. At the present time there is much resentment among other wage-earners and salary-earners over the favoured position of Government employees.'

On this Sir J. C. Stamp comments: 'The *principle* is perfectly valid, see above, if it doesn't connote *extent*.'

QUESTION 7.—*It is generally taken for granted that certain transactions—e.g. the purchase and sale of stocks and shares or real property, the raising of mortgages, the hiring of a house, and so on—provide suitable occasions for taxation. Is there any justification in economic theory for a tax on transactions? Does not the more enlightened view point to the freeing of transactions as well as trade from the inquisition of the tax-collector?*

This question was thrown into a form which suggests the answer, and we are nearly agreed in saying 'Yes' to it. Thus Mr. Hirst, agreeing with the view implied in the question, says: 'It seems to me that taxes on transactions are bound to reduce and hinder trade. Hence I would certainly reduce rather than increase such taxes.' But we admit that there may be a case for continuing taxes to which people have become accustomed, especially when, as with some of the stamp duties, their payment lends a kind of additional sanction to the transaction taxed.

Sir Edward Brabrook replies: 'Freedom of transactions and of trade is essential to the welfare of the country.'

Mr. Robertson replies (Mr. Lavington and Mr. Ellinger agreeing with him): 'Convenience, ease of collection, and productivity must be given some weight in framing a system of taxation as well as justice. Even from the standpoint of justice, there is perhaps something to be said for making a special charge on those who avail themselves to a special extent of the readiness of the State to enforce contracts. I am not inclined to favour a general repeal of taxes on transactions; indeed, I should like, if practicable, to see them developed in such a way as to secure part of the increment of capital value arising in cases of speculative purchase and resale of houses, securities, &c. (unless these can be assessed to income tax).'

Dr. Dalton thinks that 'a moderate tax on transactions is neither a very good nor a very bad tax; it tends to check production less than some existing taxes, but more than others.' Mr. Hoare thinks that these taxes 'should be retained for a long time to come, so as to allow the worst taxes—*e.g.* those on tea and sugar—and local rates to be taken off before stamps are touched.'

Sir J. C. Stamp takes a different view. 'There is very little justification for taxes of this kind, except so far as they serve as convenient methods of registration, or lending validity to transactions.'

Mr. Hilton Young replies emphatically: 'A rotten tax! No relation to ability to pay; not even equitably spread over the limited area that it covers.'

Dr. Cannan replies: 'Transactions *are* trade. When you have exhausted the better taxes and still want money you take the worse, as Adam Smith said.'

Mr. Lawrence and Mr. Mason reply 'Yes' to the last part of the question. Mr. Shaw replies: 'Every tax that hampers a beneficial human activity is economically bad. But it may be psychologically expedient.'

QUESTION 8.—*If the principle of 'ability to pay' be accepted, does it not follow that the greater part of the national revenue should be raised by income tax?*

This question also suggests the answer, and again we are able to reply mainly in the affirmative. In November 1917 our Committee appointed a Sub-Committee on Income Tax Reform, and seventeen months later the Sub-Committee was invited to give evidence before the Royal Commission on the Income Tax. The Sub-Committee threw its opinions into a few short 'points' or sentences for its proof of evidence before the Royal Commission. The first six sentences ran as follows:

1. That the income tax is the fairest, cheapest, and most productive of all possible taxes.

FOOTNOTE.—We assume, of course, the existence of a constitutional Government; a despotic Government might use the income tax as an instrument of oppression.

2. That the tax requires to be adjusted to the much-increased demand for revenue.

3. That it is indefinitely elastic, and can be made to produce as much revenue as the citizens as a body think justifiable.

4. That if skilfully adjusted to the 'ability' of each taxpayer it imposes little real burden.

5. That a heavy income tax has a tendency to lower prices of commodities in general, just as an inflation of the currency increases them.

6. That a graduated income tax, unlike most (if not all) other taxes, makes for greater equality of spending power.'

Dr. Dalton comments on point 5: 'This seems to me a fallacy. You transfer purchasing power from income-taxpayers to beneficiaries of public expenditure, *e.g.* civil servants, holders of War Loan, old-age pensioners. Total purchasing power is not diminished. Why, then, should prices fall?'

Mr. Lavington also expresses a doubt about point 5 and asks, 'May not the most important effects of a heavy income tax be: (1) to compel an extension of bank loans, and (2) to check productivity; in both directions tending to raise prices?' The Sub-Committee contemplated a large reform in income-tax law and practice, of which the most important items were that the point of total exemption should be lowered until the tax was paid by all persons who could be considered as having any tax-paying ability, and that the tax should be collected at the source wherever possible—*e.g.* that in the case of salaries, wages, and other periodical payments the tax should be deducted by the person making the payment and that the taxpayer's abatement and allowances should be taken into account at the time of deduction. On the question of prices, what the Sub-Committee, which was working while the Government still made no attempt to meet its expenditure out of genuine revenue, meant was that the payment of wages and salaries out of loans instead of taxes necessarily raised prices. If the income tax had been raised sharply at the outbreak of war, and had been applied to all recipients of income above some very low point (*e.g.* the annual value of a private soldier's pay, keep, and allowances), the purchasing power of ordinary persons would have been reduced and prices would have been lower in consequence. As Dr. Dalton says elsewhere in the present report, the depreciation of the currency was equivalent to an income tax without graduations, abatements, or exemptions.

A serious fact about the income tax is that it is paid by so small a proportion of the citizens who possess the Parliamentary vote. Exemptions and abatements, which allow a married man with three children to earn almost £5 per week without being assessable to income tax, are barely compatible with democratic Government. If 'Taxation implies Representation,' ought it not to follow that 'Representation implies direct taxation?' Citizens who, by their vote at Parliamentary elections, ultimately determine national policy, ought to have the responsibility which goes with power brought home to them by high taxes if they have voted for a costly policy, or low taxes if they have voted for a policy of retrenchment.

Hardly any other tax can be adjusted to the 'ability' of citizens, for even death duties, although carefully graduated, only affect incomes from property, not the amount which individuals have to spend. Some taxes, *e.g.* those on tea and sugar, fall heaviest on the man (or widow) with a family and a small income.

Sir Drummond Fraser writes: 'In my opinion the bulk of the revenue should be raised by income tax. It forces the taxpayer to do without something else in order to pay the tax. In practice this is a transfer of purchasing power, and therefore prevents monetary inflation.'

Mr. Hirst replies: 'I agree that the income tax, if it begins on very low incomes, ought to be relied upon as the mainstay of the national revenue. And if by raising it the yield per penny were greatly diminished, I should regard that as a sign that the limit of the taxable capacity of the nation had been reached.'

Sir J. C. Stamp, Mr. Hilton Young, Mr. Mason, and Mr. Hoare answer 'Yes.'

Mr. Shaw also answers 'Yes: all of it. Include "ability to work" and you may substitute poll tax for income tax. If it were not for the phenomenon of unearned income arising through the operation of the law of rent, income tax would act as a premium on idleness.'

Mr. Robertson replies: 'Yes; if we may include death duties in income tax, but there is much to be said for having some taxes which hit people in proportion to their *expenditure* (especially luxurious expenditure) rather than their *income*, and thus discriminate, to some extent, in favour of saving.

And I should not be in a hurry about discarding at present any taxes which people have become tolerably accustomed to.'

Professor Foxwell has expressed the same views, and is still more in favour of taxing expenditure and exempting savings.

Dr. Dalton (who refers to an article on 'The Study of Public Finance' in *Economica*, June, 1921) approves of the present exemptions and abatements. He thinks that 'ability to pay' is an ambiguous phrase. 'It may have reference either to effects on production, to effects on distribution, or to current views of equity. I agree, however, that a large proportion of the national revenue should be obtained from income tax.'

Sir J. C. Stamp comments: 'The phrase is not really ambiguous in general use, only made so by a few writers who import new ideas into it.'

Mr. Lawrence replies 'Yes, except that in order to be really equitable the income tax would have to be very inquisitive; it is not a bad plan to tax luxuries as well, and for practical purposes to have some other taxation.'

Dr. Cannan, however, replies shortly 'No; why should it?'

Mr. Gibson prefers to avoid 'a multitude of taxes,' and most economists will agree with him. He would like to sweep away all existing taxes except the Estate Duties and Liquor Duties, and to substitute a single tax. He suggests a flat-rate income tax with the present abatements, and would impose additional graduated rates on annual increases of income, but the additional tax must not be too great to throttle enterprise or discourage saving. He thinks that existing capital 'is in the control of too few persons.'

Sir Edward Brabrook, who does not approve of the present abatements, exemptions, and allowances, writes: 'The question of ability to live on what is left after payment of taxation does not arise in a well-graduated tax.'

QUESTION 9.—A Sub Committee was appointed last year to inquire and report upon 'The Currency and the Gold Standard,' with Mr. Lavington and Mr. Robertson as Joint-Conveners, but our questionnaire was gradually extended, mainly by the suggestions of Mr. Lavington and Mr. Robertson, to cover most of the Sub-Committee's province. The original question ran thus: '*Can a paper currency (such as a "Bradbury"), which is controlled by the Government and bears no fixed relation to any stock of gold or silver, serve as a measure and standard of value or as a satisfactory medium of exchange?*'

Our members and correspondents seem to agree with Dr. Cannan's reply: 'It is quite a satisfactory medium of exchange, but a bad standard.'

Dr. Cannan also writes: 'There is no reason to suppose that the absolute limitation of the Bank of England's fiduciary issue to 18,000,000*l*., or whatever it has been, and the abolition of other fiduciary issues, has been of any use for the *standard*, and it certainly has not made the medium of exchange any better. Convertibility into free gold is all that is required to keep the paper at gold value; uniformity and cognoscibility make a single issue desirable, but that can be attained without making notes into gold *certificates*.'

Dr. Dalton replies: 'If not over-issued to such a point that public confidence in it disappears, it is an excellent medium of domestic exchange, and much more economical and convenient than gold coins. If intelligently controlled by the Government it might become quite a good standard of value.'

Mr. Ellinger thinks that a paper currency, 'if efficiently controlled, according to the requirements of production, and not according to the requirements of the Government, might be a satisfactory measure and standard of value, and a satisfactory medium of exchange for internal transactions, but the likelihood of such a control being efficient is so small, and the desirability of maintaining a gold standard for the purpose of international trade is so great, that, for this country at all events, the maintenance of a gold standard is essential.'

Mr. Robertson, who added sub-questions (1), (2), (3), and (4), replied as follows to the original question: 'Theoretically, an inconvertible paper currency, properly managed, would be a better standard than one tied to gold, the value of which cannot be expected to remain stable. But the inherent tendency, both of industry and commercial activity, to press for a continuous expansion of the instruments of payment is so strong that there is a good deal to be said for tethering both Governments and bankers down to gold, which, though far from a satisfactory standard, is better than no standard at all.

'Further, if other countries have a currency whose value is stable in terms of gold there is a strong argument for our doing the same, in order to facilitate international business. The U.S.A. have such a currency at present, and it is perhaps more likely that the European countries and the Dominions will eventually return to such a system than that they will make a good job of any other.

'There seems no reason why a paper currency, which is convertible into gold *only for the purpose of making payments abroad*, should not serve as a satisfactory medium of exchange within the country.'

Dr. Dalton agrees with Mr. Robertson's views, though he thinks the last sentence, 'an understatement.' He adds: 'To me it seems obvious that a paper currency *is* a satisfactory medium of domestic exchange, though it may be a bad standard of value.'

Mr. Shaw, with more scepticism, replies: 'It can, *if* it is honestly controlled, but it never is.' Mr. Lawrence agrees. Sir J. C. Stamp, Sir Drummond Fraser, Mr. Sykes, and Mr. Mason answer 'No,' Sir Josiah adding: 'Not as at present controlled. Not if we are thinking of the long run.' Mr. Sykes and Mr. Allen believe that the standard of value should be, as far as possible, independent of Government or other human control. Mr. Sykes adds: 'I am firmly convinced that, in spite of the admitted defects of gold as a standard, the balance of advantages in its favour, as compared with any standard dependent on Government control, is very great indeed. The question of the medium of exchange is of minor importance.'

Sir Drummond Fraser says that a paper currency should be issued by the Bank of England on a gold basis, as our Committee suggested in its 1915 Report. He adds: 'I believe in the cast-iron principles of the Bank Act. The mistake in issuing the currency notes was that the amount at first was unlimited. This enabled the Bank of England to manufacture bank credit *ad lib.* and without restraint.'

Question 9 (1).—*Do we want our price-level to remain stationary? or to be continually falling* (*as in* 1873-96), *or to be continually rising slightly* (*as in* 1896-1914)*?*

It is assumed in this question that prices in general may be raised or lowered by an increase or decrease of currency, and either by natural causes, such as the discovery of new or exhaustion of old goldfields, or by the action of the Government in expanding or contracting a paper currency. This is a commonplace of economics, and does not contradict the opinions given in the replies to Sub-questions '1,' '2,' and '3.'

With so much experience of the troubles caused by rising prices between 1914 and 1921, our members and correspondents agree in *not* wanting the price-level to rise. There is some difference of opinion as to whether it is desirable that the price-level should fall further. Dr. Cannan replies: 'Keep it steady'; Mr. Lawrence says: 'Stationary or slightly falling'; Mr. Mason says: 'As stationary as possible'; Sir J. C. Stamp prefers it to be 'continually falling, not to a pre-War level, but to a level where the superstructure of credit has a reasonable relation to a metallic basis.' Dr. Dalton thinks that 'if future price-movements were perfectly foreseen by all parties it would not matter what those movements were. But perfect, or even tolerably good, all-round foresight being unattainable, I think that, on the whole, a price-level slowly and steadily falling is most to be desired.' Mr. Hoare, placing more trust in Government control of paper money than the rest of us, wants the level to 'tend constantly downwards, paper money being always so increased as to make the drop in prices very slow, *i.e.* the drop should be due to increased output, and the drop should be so hindered by increased currency as to prevent that drop from being so pronounced as to check output.'

Mr. Ellinger also answers 'Stationary,' but he adds: 'I should like to see prices on the level corresponding to the average level which existed at the time the War Debt was created, and in so far as the general level of prices may be higher than such level I should like to see prices falling gradually as public debt is paid off, or converted to a lower rate of interest. It must be borne in mind, particularly as regards the three following clauses ("2," "3," and "4") that the level of prices in this country depends very largely on the general level of world prices, and I should be sorry to see the general level

of prices of our exports falling more rapidly than the general level of prices of our imports.'

Mr. Shaw replies : 'I cannot understand anyone but a speculator "wanting" prices to fluctuate.'

QUESTION 9 (2).—*In pursuit of our object, whatever it is, ought we to seek to maintain our currency at a parity with gold?*

This is one of the few questions on which we are able to reply with practical unanimity in favour of linking our currency with gold. Professor Cannan replies : 'Get it to par with gold first, and then join other gold countries in seeking either greater stability of gold or a substitute for gold.' Mr. Mason also wishes 'Gradual contraction of the paper currency until parity of exchange with gold standard countries has been re-established. A date then fixed for resumption of specie payments. Legislative enactments would probably be necessary to facilitate machinery of convertibility and regulation of currency issues between the Treasury and the Bank of England and the other banks throughout the country. All of the foregoing would be helped and stimulated by drastic economy in every department of the State.'

Mr. Pethick Lawrence replies : 'In default of an index-number standard (Professor Irving Fisher's compensated dollar), which I regard as theoretically best, I favour gold parity.'

Sir J. C. Stamp replies : 'With gold certainly, but I do not think an actual parity matters, so long as there is reasonable stability.'

Dr. Dalton replies : 'As an immediate policy, Yes. When this is achieved the next stage should be to seek to apply Professor Irving Fisher's or some other similar scheme.'

Mr. Gibson, who is reading a paper at the Edinburgh meeting on the subject, replies : 'I think the time is ripe for the introduction of a new standard of value, to be stabilised by the interest factor when an international agreed-upon index-number has fallen to pre-War level. The working man is surely entitled to a continuity of employment. In pre-War times the state of employment was dependent on variation of gold supplies. I suggest an international note-issuing bank (denominations of notes, say, 1,000*l.* and upwards), such notes to form the basis of bank cash reserves, and cover for internal paper issues. Bank and Government applicants for international notes would pay interest thereon, the rate of which would be subject to variation. Each country would have its own internal rates of interest, varied from time to time according to the course of the exchanges and domestic conditions. Gold in pre-War times functioned as the mercury of the barometer that compelled interest rates to be altered from time to time in accordance with the current economic atmosphere. The interest factor preserved the due proportion between the employment of labour for producing consumable goods and capital goods.'

Mr. Lavington replies : 'Against the advantages of re-establishing a gold standard must, of course, be set the evils resulting from (1) the raising the value of the Bradbury to that of the sovereign, or (2) the fixing of its value in terms of gold at something lower than the sovereign. I doubt if it is socially desirable to lower the level of prices, except in so far as this may be necessary to re-establish a gold standard. In correcting old injustices in this way we should also be creating new ones.'

Mr. Hirst and Mr. Allen maintain : 'That any monetary measure and standard of value should possess intrinsic value, so that its utility should not depend upon the pleasure of the Government. No doubt the fact that gold has been so generally adopted as a standard adds to its value.'

QUESTION 9 (3).—*If so, should it be at the old parity, or some other—say one corresponding to the present gold value of the Bradbury?*

Here some differences of opinion will be found. Professor Cannan replies : 'The old. The fixed-income people will have been a good deal robbed even then.'

Dr. Dalton, Mr. Ellinger, Mr. Lawrence, and Mr. Mason favour the old parity. Mr. Shaw replies : 'It depends on circumstances. A big change in the purchasing power of a currency upsets everything frightfully. A restoration may be as great a calamity as an acceptance of the change.' Mr. Allen agrees, and regrets that these considerations, which seem so obvious now,

should not have had more weight with the Chancellor of the Exchequer and his advisers in August 1914 and during the earlier months of War.

Mr. Hoare replies : 'I don't see that it matters theoretically, and therefore I should aim at the old parity.'

Sir J. C. Stamp replies : 'Depends on foreign countries to some extent. On the whole, Yes.'

QUESTION 9 (4).—*If the old parity, should an effort be made to restore it in the immediate future? If so, how?*

Professor Cannan replies : 'Yes, by quietly reducing the Bradbury issue without making any great definite profession about it.'

Dr. Dalton says : 'If "immediate future" means "within the next eighteen months, "Yes."' As to the method, he refers to his answer to question '13' below.

Mr. Ellinger fears that 'No heroic measures are possible to enable restoration in the immediate future. Exports should be increased to the uttermost. Economy of consumption should be practised in imported articles not forming the raw materials or semi-raw materials of our industries. Efforts should be made to draw gold from debtor countries, like Spain, who have an adverse exchange and a large accumulation of gold. At the present time Government securities in the hands of the public, in so far as they are used to obtain advances from banks, cause inflation of credit; the total amount of credit available should be measured by the total volume of commodities to be financed, and not by the total volume of commodities, *plus* a Government debt which represents no existing commodities. The gradual restoration of the credit position on these lines would lead to a reduction of the currency notes in circulation and to a more speedy restoration of the gold standard. The reduction of the Government debt, particularly of floating debt, would have the same effect.'

Dr. Dalton comments : 'But the value of commodities varies with the amount of credit.'

Mr. Lavington comments : 'I doubt if we should try to get gold from Spain. It is surely better that all surplus gold should go to the U.S.A., ultimately raising prices there and so helping to re-establish the U.K. on a gold standard. I do not think that there is any urgent need for more gold in this country. The urgent need, in my view, is for (1) increased production, (2) diminished consumption, and (3) diminished currency.' (Dr. Dalton agrees.)

Sir J. C. Stamp replies : 'Only very slowly.'

Mr. Shaw replies : 'Make a Bradbury exchangeable for a sovereign at the Bank of England or the Mint.' [5]

QUESTION 10.—*Would a Capital Levy or a Forced Loan offer any escape from the dangers of national insolvency? Or would private credit suffer more than public credit could hope to gain from any such method of reducing the War debt?*

This question is evidently controversial. Unfortunately, some heat has been imparted to the discussion by the discovery of large war profits and by the Board of Inland Revenue's estimate of 4,000,000,000*l.* of 'increased wealth' in private hands owing to the War. Sir John Anderson informed the Select Committee that 'the estimated aggregate increases of value appertaining to those individuals whose capital wealth had increased during the War, diminished by the fall in value of the capital of those whose capital wealth had decreased, amounted to about 4,000,000,000*l.*' (Cmd. 594, p. 17). As we said in our Interim Report at Cardiff last year : 'This statement was taken to mean that the nation as a whole, or owners of property as a body, were actually better off as a result of the War. The case for a levy on War wealth, like the case for the Excess Profits Duty, was based on the very natural sentiment that people ought not to "make money out of the War." According to these Memoranda the Board of Inland Revenue had taken an estimate of the amount of wealth in private hands before the War, which came to 11,000,000,000*l.* and compared it with a similar estimate "as at June 30, 1919." The second estimate showed a total of 15,000,000,000*l.*, leaving 4,000,000,000*l.* apparently liable to

[5] It is so now under the Currency Notes Act (IV. & V. Geo. V. ch. xiv.). But in practice the Bank's officials show a reluctance to pay out sovereigns, and as the sovereigns can neither be exported nor melted down, the Act is a dead letter.

the proposed levy. When an allowance is made for the depreciation of the currency, it is clear enough that these figures indicate a decline rather than an increase in the amount of real wealth in private hands.'

The Memoranda contained a warning that 'the increase in the exchange value of certain forms of wealth is merely the result of the general rise of prices'; but this qualification, which was not very lucidly expressed, has been overlooked. Part of the unrest among wage-earners is due to the belief, fostered by a misunderstanding of these Memoranda, that the War had enriched the propertied classes. We thought it necessary (at Cardiff) to declare our opinion that the reverse is the case; that the recent War, like other wars, has caused the destruction, not the creation, of wealth. But the working-classes are quite right in believing that *some sections* of the propertied classes have been greatly enriched by the War.

Had the demand for the conscription of wealth taken the form of demanding increased direct taxation of all non-combatants, it would have been better justified. As a matter of fact, it is almost impossible to postpone the financial sacrifices involved in a big war by postponing taxation. Unless the Government's expenditure can be defrayed by loans subscribed out of the genuine savings of its subjects, the alternative is a policy of credit and currency inflation, which acts as a concealed levy on many forms of pre-War capital and as a concealed income tax on all kinds of income. But whereas the ordinary income tax can be adjusted to the ability of the taxpayer, the concealed income tax falls mainly upon people with 'fixed incomes.' It was assumed by the Income Tax Committee of 1906 that the words 'permanent' and 'precarious' in their terms of reference might be translated by 'unearned' and 'earned.' Experience has shown that most 'earned' incomes may be raised as the purchasing power of money is lowered, either automatically, as in the case of Government employees and railwaymen, whose pay rises or falls with the changes in the Ministry of Labour's index number, or (with more friction) through strikes and lock-outs or amicable arrangements. There are other cases, such as those of clergymen and some schoolmasters, though not the teachers in State-supported schools, whose money incomes are not elastic.

On the other hand, the so-called 'unearned,' or 'permanent,' or 'fixed' incomes cannot, as a rule, be adjusted to alterations in the purchasing power of money. This applies to 'gilt-edged' or trustee securities, and explains very largely the present financial straits of our voluntary hospitals, which have found their income from property cut down by about a half. It applies to all debenture and preference stocks, and to certain classes of ordinary shares—*e.g.* those of gas, water, tramway, and railway companies—and at present to bank shares. Consequently, if the pound sterling loses half its purchasing power the receivers of fixed money incomes, whether from earnings or from investments, suffer the equivalent of a 10*s.* income tax, from which large sections of the wage-earning and salaried classes, whose incomes are adjusted to the Ministry of Labour's index number, escape. In some instances, especially those of industrial companies which can make and divide profits without statutory restrictions, the depreciation of the currency has transferred part of the debenture-holders' and preference-holders' property to the ordinary shareholders. Sometimes this fact has been recognised by the distribution of bonus shares to the ordinary shareholders.

On the whole, therefore, it appears that while certain favoured persons and small classes of capitalists acquired new wealth as a result of the War, the vast majority of the propertied classes are very much poorer. Moreover, a good deal of the profits which might have been assessed to a levy on War Wealth a year ago has vanished in the recent industrial depression.

Our members and correspondents differ irreconcilably in their answers to Question 10.

Mr. Ellinger and Mr. Gibson do not think that this country is in any danger of national insolvency. Mr. Ellinger says: 'National insolvency might take two forms: (1) That we could not pay our debt to America; (2) that we could not raise sufficient taxation to pay the interest on our internal debt. As regards (1), a capital levy or forced loan would not help. As regards (2), a capital levy would help. I think that it is very doubtful, if we had had a capital levy a year ago, whether the shock to private credit which would have

been caused by such a levy would have been as great as the shock which private credit has now sustained, and which might have been avoided had a capital levy been imposed; for in this event the inflation of 1920 would have probably been avoided, and it is this inflation which is largely responsible for the present condition. However, in view of the present state of private credit it would be disastrous at this time to superimpose on the present shock such further shock as might be caused by a capital levy.'

Mr. Gibson maintains that 'No country is nationally bankrupt until it cannot pay, within a reasonable time, the interest on its external debt by exports or other form of settlement. In 1914 we had 4,000,000,000*l.* invested abroad. Since that year we have sold about 1,000,000,000*l.* of these investments and created an external debt of 1,000,000,000*l.* in round figures. We have therefore a net balance abroad of about 2,000,000,000*l.*, not taking into account any indemnity payments yet to be received from former enemy States or loans to Allies. Last year we had a net trade balance in our favour, taking invisible exports into account, and a net favourable balance is likely to be maintained in future years. I see no present necessity for a forced loan. A capital levy directed solely against War-fortunes I support if practicable. It would accelerate the economic recovery of the country and tend to lower costs of production, after a small temporary disturbance to credit. Preferably it should take the form for several years of payment of 5 or 6 per cent. interest on amount assessed.'

Sir J. C. Stamp replies: 'It is a nice balance. It all depends upon the prevailing psychology in financial circles at the time when it is introduced. It is not enough to prove that mathematically or actuarially the effect should not follow. It is what people imagine that will rule the issue, and not what they ought to think.'

Dr. Dalton also does not take seriously 'the dangers of national insolvency.' He adds: 'But I favour a capital levy sufficiently productive of revenue to wipe out at least half the National Debt within the next few years. I am in general agreement with Prof. Pigou's argument on this subject, and I regard such a levy as specially desirable if a policy of deflation is adopted, as I think it should be. Unless the business world is successfully bamboozled into unreasonable panic, I believe that no appreciable shock to private credit need result from such a levy. (For a detailed scheme, with which I am in general agreement, though the proposed minimum exemption from the levy—5,000*l.*—is, perhaps, rather too high, see the Second Interim Report of the Joint Labour Committee on Taxation and the Cost of Living.) I am opposed to a forced loan. It appears to me to combine nearly all the disadvantages, and none of the advantages of taxation and a voluntary loan.'

Mr. Robertson replies: 'A capital levy would have great advantages: (*a*) in paying off a considerable amount of debt before the value of the pound sterling greatly increases; (*b*) by affording a smaller deterrent than a higher recurrent income tax to *future* enterprise and saving, since even if *levied* by instalments it would be *assessed* on present capital values; (*c*) in eliminating the standing menace of undue expansion of the currency caused by the continuous maturing of Treasury Bills; (*d*) in its social and political effects. I am not convinced that if payment were allowed to be made in selected securities (with perhaps preferential treatment of War loans), and to be spread over a number of years, the effect need be very damaging to private credit; but I do not feel that I have the requisite practical experience to speak with confidence on this point.'

On the other side, Mr. Shaw writes: 'A capital levy is utter nonsense economically; it is the delusion of "the practical business man," who thinks that because he can sell an income of 5*l.* a year for 100*l.* down, the whole income of the world could be sold for twenty times its figure. There is nothing to be got out of capital by way of taxation except the income it produces. As to a forced loan, why not a forced gift? All taxation of the interest on a loan to the State is repudiation. The War Debt is already repudiated to the extent of 11*s.* in the pound in the case of the very rich. How far the repudiation should go, and what form it should take, are matters of expediency.'

Mr. Lawrence, answering Questions 9 (iv.) and (10) together, replies: 'Yes. Capital levy, complete revision of peace treaties, free trade, limitations of paper currency, and many other steps. Had deflation been carried

out immediately after the War by such means I would have recommended some compensation to holders of stocks of commodities. I am doubtful whether such compensation should now be given, but in any case it should be confined to deflation specially brought about by Government action.'

Mr. Mason is opposed both to a capital levy and a forced loan at the present time; Mr. Hoare thinks that 'a forced loan would be far preferable to a capital levy.'

Sir Drummond Fraser writes: 'I am opposed to a capital levy or a forced loan, because both produce monetary inflation.'

Mr. Allen desires to point out that a levy on pre-War capital has, in effect, taken place. Each War loan acts as a levy on existing investment securities, just as an issue of bonus shares lowers the value of a company's existing shares. This fact was recognised by Mr. McKenna and his successors at the Exchequer when they gave 'conversion rights' to holders of earlier War loans. They may have foreseen, also, that people would not subscribe to such issues without some kind of guarantee against depreciation. If a holder of pre-War gilt-edged securities has been able to save enough money to buy War loans to the nominal value of his stocks in July, 1914, he is about where he was when war broke out; otherwise he is poorer. The case of hospitals must be well known to everybody. These institutions, like many educational, religious, and charitable bodies, are in serious financial difficulties because the value of their property has been cut down about 50 per cent., although they are supposed to be exempt from taxation; they escape income tax, it is true, but ordinary persons and business concerns have to pay tax on an income which in many cases has been cut down by one-half. Earned incomes, of course, pay income tax, but then they are elastic, and in most cases have been raised considerably from their pre-War rate. In some cases salaries are paid free of income tax, which seems to me an undesirable practice. To illustrate the change in the distribution of the national income produced by the War and the depreciation of the currency let me take a single familiar instance: Before the War railway shareholders and railway operatives drew about the same income from their undertaking—about 47,000,000*l*. or 48,000,000*l*. a year. The shareholders are still drawing their 47,000,000*l*., with perhaps another million for new capital; the railwaymen are drawing about 160,000,000*l*., though their wages are now falling with the decline of the 'cost of living.'

No doubt some individuals have made large fortunes out of the War; perhaps wars would not last so long if no one made money out of them. A new propertied class appears to have come into existence through the War. There was a good case for the taxation for these special fortunes, but by this time the opportunity has been lost. In my opinion we ought to have had at an early period of the War an excess income tax to balance or supplement the excess profits duty. One result of the War may be welcomed: it has brought about a much more equal distribution of the national income. But that fact is an argument against a levy on capital.

On grounds of equity the objection to a levy is that it throws a special burden on a particular class without any reference to the principle of ability to pay. It may be admitted that a person with 500*l*. a year from property has a greater taxable capacity than a similar person earning 500*l*. a year, but this difference is already recognised (*a*) by levying a higher rate of income tax on invested income, and (*b*) by imposing special and heavy taxation when property changes hands at death. To a much smaller extent there is a special kind of taxation in the form of stamp duties when property of most kinds passes from hand to hand.

Dr. Dalton comments: 'It is a question of verbal convenience whether you like to describe what has happened in the past as a "levy on pre-War capital." I have no objection to so describing it, provided that you recognise that the fall in prices, which is now taking place, and the fall in rates of interest, which may soon be anticipated, will be a "bounty to pre-war capital."'

QUESTION 11.—*Now that the Excess Profits Duty has been repealed, should some other form of special taxation of business profits be imposed? If so, what?*

On the whole our opinions are against the imposition of a special tax on business profits. Prof. Cannan, Sir Drummond Fraser, Mr. Mason, Mr. P. Lawrence, and Mr. Sykes all answer 'No,' and suggest as an alternative greater

economy in Government expenditure. Mr. Hoare also answers 'No.' On the other hand, Sir J. C. Stamp answers, 'Yes, as already indicated by me in the *Economic Journal.*' Mr. Robertson also is 'inclined to think that a special tax on business profits, if free from the particular defects of the E.P.D., and felt to be generally equitable as between different firms and different trades, is justifiable (*a*) on the grounds of equity—the rewards of business enterprise being on the whole disproportionate to those of other kinds of mental and bodily activity; (*b*) on the grounds of indirect effects—a well-devised profits tax, pressing evenly all round, would not materially impair efficiency or discourage emulation in enterprise. But I am not a business man!'

Mr. Ellinger, answering 11 and 12 together, writes: 'I hope the Chancellor is right, and that it will not be necessary to impose any tax in place of the "Excess Profits Duty." I do not like the French Tax on Turnover; nor do I like the American differentiation of spent and saved income. I think that the Canadian Tax on Sales is worth further examination, if it should prove necessary to replace the Excess Profits Duty.'

Dr. Dalton, expressing the views which we imagine to be held by the majority of economists, writes: 'I am not much enamoured of special taxes on business profits. If the difficulties of capital valuation can be overcome at a reasonable cost, there is something to be said for a tax progressive according to the rate of profits. If not, there is a little, but not much, to be said for the present Corporation Tax. But as a general proposition, I doubt the expediency of making "A," who derives a certain income from business, pay more taxation than "B," who derives an equal income from War Loan.'

Mr. Bernard Shaw writes: 'I see no objection to giving effect to a law of maximum profit; but the maximum must be determined first on grounds of national welfare.'

Mr. Allen writes: 'I am opposed entirely to the special taxation of business profits. Except in the case of very small businesses, where the taxation is negligible, business profits are already assessed up to the hilt; for the Revenue authorities treat as taxable a larger sum than any prudent firm of partners or board of directors would think it wise to distribute or treat as income. In the case of the professions, and of other earnings which are not made as the result of a regular business with clerks, ledgers, and so on, there is more possibility of income escaping taxation than there is in a regular business. Whether incomes earned in business are greater than those earned in a profession I cannot say, except, indeed, that big fortunes are almost entirely made in business. There is always a risk of error if we get away from the fact that "taxes are paid by taxpayers." Does it matter to the Chancellor of the Exchequer how a man gets his income? Is it not likely also that a special tax on business profits would in the long run tend to be passed on, like any other business expense, to the consumer? It would be difficult to say whether the E.P.D. was a direct or an indirect tax; I have little doubt that in many cases it was passed on, and that in most cases it led to extravagance or acted as a check on enterprise.'

QUESTION 12.—*Do you think that the French Tax on Turnover, the Canadian Tax on Sales, the American differentiation in taxing spent and saved income, or any other tax now being tried in a foreign country, would be a useful addition to the list of British Taxes?*

To offer a direct negative to so comprehensive a question appears to savour of national arrogance, and members of our Committee do not wish to say that nothing can be learnt from foreign and Colonial experience in taxation. Our general opinion is, however, that we have quite enough taxes in this country already, and that the main thing to be aimed at is a reduction in expenditure rather than the discovery of new revenue sources.

Dr. Cannan, Dr. Dalton, Sir Drummond Fraser, Mr. Hoare, Mr. Lawrence, Mr. Mason, and Mr. Sykes answer 'No.' Mr. Gibson and Sir J. C. Stamp also dislike the three taxes mentioned, but think the reference to *any* other tax used abroad too wide.

Mr. Gibson writes: 'Taxes on turnover or sales are difficult of equitable distribution and of collection. Presumably the percentage in the £ would have to vary with different trades, wholesale and retail. If such a tax is necessary, I prefer it to be collected at the spending source—a tax on income

actually spent each year, whether for goods or services by final consumer. Taxpayers to be assessed on previous year's expenditure. This tax would tend to increased saving, lower interest rates, and tend to increased production.'

Dr. Dalton also has 'no confidence whatever either in the practicability (at reasonable cost) or in the economic desirability of taxes on turnover, sales, &c.'

Mr. Allen writes: 'The Tax on Turnover has been strongly advocated by Lord Leverhulme and by some financial journals in London. Lord Leverhulme suggests a Tax on Turnover not exceeding $1\frac{1}{4}$ per cent.; "the Tax on Turnover would be a flat rate, not graded or varied for luxuries or necessities" (*The Organiser*, December 1920).

'Surely this idea of a Tax on Turnover is a survival from the Feudal System, and is as obsolete as chain mail and the common field. How could such a tax be applied at the same rate to the purchase of furs, champagne, and motor cars as to the purchase of boots, bread, and rail or tram tickets? Much business is done, especially in the City of London, and no doubt in our other large towns, on a tiny margin of profit. How would Lord Leverhulme apply his $1\frac{1}{4}$ per cent. to "day to day" loans at 4 per cent. per annum? How, too, would the tax be applied to purchases of Consols, War Loans, or other stocks bought to employ temporary balances? In this country we say that certain kinds of expenditure, *e.g.* on wines, spirits, beer, cigars, tobacco, motor cars, game-shooting, cinema and theatre tickets, indicate a taxable margin, and so we tax them. Most other kinds of expenditure, *e.g.* on clothes, food, schools, rent, doctors, depend largely on the size of a man's family and offer no indication of his capacity to pay taxes; if anything, they indicate the opposite.

'So far as I can ascertain, the Turnover Tax in France has proved a disappointment, and is said to have led to much dishonesty on the part of sellers. Evidently a tax on turnover is meant to be passed on to the consumer, and in most cases it would be. Probably it would fall most heavily on genuine business transactions or on the purchase of necessaries, and least heavily on the purchase of luxuries; therefore it would increase the cost of living; it is bound also to differentiate in favour of large concerns and multiple shops against the smaller concerns and the individual shopkeeper. It must involve, too, a prodigious extension of Government activity and constant interference by officials with trade and production of every kind. Experience since the Armistice shows how much injury can be done to trade and to the nation's prosperity by the interference of Government officials with trade.'

We should be glad to recommend some exemption of savings from income tax, seeing that the need of new capital is so great; but probably, as Dr. Dalton writes, the administrative difficulties are insuperable.

Dr. Dalton suggests certain changes in our tax system: 'I am in favour of a tax on the capital site values of land, but probably this would be most conveniently treated as a source of revenue for local authorities. I am in favour of changes in the Death Duties, as a result of which (*a*) the present legacy and succession duties should be amalgamated and steeply graduated according to the amounts received by inheritors; and (*b*) the present estate duty should, if administratively practicable, be replaced by a tax of the type suggested by Professor Rignano, of Milan. (See my review of Professor Rignano's 'Del Diritto Successorio' in the *Economic Journal*, March 1921, and also my 'Inequalities of Income,' Part IV., Chapters IX. and X.)

The mainstays of the British tax system should, in my opinion, be (*a*) Income Tax, (*b*) Inheritance Tax, (*c*) Taxes on Alcohol and Tobacco, with (*d*) Capital Levy as an exceptional and transitory measure.'

Mr. Bernard Shaw 'is strongly of opinion that the principle of exempting invested income—that is, income transformed into capital—from income tax should be made general; so that expenditure, not savings, should be taxed.'

QUESTION 13.—*Describe the process and results of deflation.*

Most of our members and correspondents have omitted to deal with this question, which evidently requires considerable reference to statistics. These statistics, however, are given and discussed in the report of our Sub-Committee on Currency and the Gold Standard.

Perhaps our collective views are fairly expressed by Sir J. C. Stamp, who replies: 'I think that these are sufficiently well known, without my being able to add very much. In any pronouncement on the subject it should be

made clear that we have most to fear from the burden of the debt becoming correspondingly much higher than if we maintain currencies at present level.' He gives as his own view : 'If we can spread the effects over about eight or ten years, we can absorb the loss on existing stocks in business by a modest reduction of the ordinary margin of profits, say one-fifth thereof.'

Mr. Gibson replies that deflation may take two forms, automatic and compulsory. A fall in prices will cause a reduction in bank loans; therefore, *per contra*, in deposits. Currency notes will return to the banks from circulation. Compulsory deflation will be caused by banks restricting new grants of credit and calling in part of existing loans. Increased taxation will also reduce the volume of deposits, and thereby the available purchasing power of the public. Banks will also sell part of their investments to the public, and will thereby reduce deposits. The funding of the Floating Debt will reduce bank holdings of Treasury Bills and money at call and short notice, and, *per contra*, deposits. Mr. Gibson continues :

'The world is at present passing through a severe transition period. It is in reality engaged in reaping the bountiful inflation harvest predicted by economists. This country will, in my opinion, come through with flying colours and make rapid economic recovery in the course of a few years. Its main danger lies at Westminster. If tariffs are imposed on any scale, their indirect effect on the costs of production will be such as to materially restrict the pre-War volume of our export trade. Germany will, in my opinion, set Russia on her feet again (though eventually the future economic alliance of these two countries will prove a standing menace to European peace). German indemnity payments to England in the course of two years or so will largely take the form of foodstuffs and raw materials from Russia, and therefore tend to reduce costs of production in the United Kingdom. The operation of the foreign exchanges will force this indirect form of payment from Germany. Russia in exchange will receive from Germany manufactured goods, transport material, and agricultural implements.

'It is to be hoped that more cordial relations will in future exist between capital and labour, brought about by an equitable co-partnership scheme in the products of industry. In addition, industrial peace demands ample reserves for distribution in times of trade depression, accumulated in times of prosperity. The future prosperity of the country, and a raising of the standard of living, is largely dependent on increased willingness to work and increased output by all classes.'

Sir Drummond Fraser, who is at present engaged in organising the International Credits Scheme (known as the ter Meulen bond scheme) for the restoration of international trading with the impoverished European countries, believes that the scheme, 'without monetary inflation, will provide a reservoir of credit which exporters can tap for productive enterprise. It has been accepted by the British Government as a "satisfactory security" for their guarantee for 85 per cent. of the exporter's risk. It has had the approval of the Second World Cotton Conference held in Liverpool and Manchester, and the International Chambers of Commerce held in London.' Sir Drummond reminds us that the British Association warmly supported his National War Bonds at the Manchester meeting in 1915.

Dr. Dalton is the only correspondent who goes fully into Question 13, and we print his reply in full.

'Attention may be concentrated on restoring the dollar exchange to pre-War parity. When this has been achieved, which implies that the British paper pound is again worth a gold pound, and when freedom to export and melt gold has been re-established, there will be no purpose in attempting further deflation. For this would only result in bringing in gold from America and Japan to replace paper currency destroyed. (Steps should be taken to prevent gold coins getting back into internal circulation. The ancient proposal to make paper convertible, not into gold sovereigns, but into gold bar for export, still seems the simplest and best way of doing this.)

'In order to restore the dollar exchange, it is necessary to lower the British price level relatively to the American price level. The amount of deflation required will therefore depend on American currency policy. It is possible to operate either through regulation of the currency note issue or through

regulation of the bank rate. (Both methods lead to the same result.) I recommend the former. Let the fiduciary issue of currency notes be subjected to a sliding maximum, in such a way that the maximum permissible shall steadily decrease in successive periods. For example, let the maximum permissible fiduciary issue be reduced at the rate of 2,000,000*l*. a week. Let this policy be announced beforehand, and let it be further announced that the policy will be continued until the dollar exchange first touches par, and that whenever, during the six months following this happy event, it again falls below par the policy will be resumed until it is up to par again. The effect of the announcement of this policy in advance would enlist the forces of speculation on the side of the pound sterling. A sharp speculative rise in sterling, without subsequent relapse, might be expected to follow immediately. Unless the Americans were deflating simultaneously, the depreciation of sterling would almost certainly run off within six months, and possibly more quickly. Gold could then be "decontrolled," *i.e.* freedom to export or melt gold could be restored, and steps taken to fix a limit to the fiduciary issue, which could remain unchanged for several years. The period of deflation would probably be marked by falling prices and, with a view to reducing unemployment, it would be well to attempt to secure, during this period, the general adoption of sliding scales of wages based on cost of living. The lag of retail behind wholesale prices might be deliberately lessened by a temporary rise in the bank rate. But this would probably come about in any case as a result of the joint stock banks drawing heavily on their balances (or the Government seeking further overdrafts on Ways and Means) at the Bank of England, in the new situation, in which new currency notes beyond the shrinking limit fixed would no longer be obtainable. It is undeniable that the period of deflation would involve various temporary inconveniences, but these, in my opinion, would be a small price to pay for the permanent advantages which would result. An alternative principle for securing a reduction in the maximum permissible fiduciary issue would be to shorten the period of one year, which is the unit of time adopted by the Government, on the recommendation of the Cunliffe Committee, in their hitherto abortive scheme for restoring the gold standard. At present the maximum permissible fiduciary issue during any calendar year is the maximum actual fiduciary issue during the preceding calendar year. This plan is leading to preposterous procrastination. For the actual issue having been allowed to expand practically up to the legal maximum at the end of 1920, no appreciable reduction of the legal maximum *can* now take place before the beginning of 1923 at the earliest, and there is no guarantee that it *will* take place even then. *Sic itur ad infinitum!* But reduce the unit of time in this scheme from a calendar year to a quarter, or, better still, to a month or even a week, and there will be more chance, though no certainty, that the dollar exchange will be restored within the lifetime of our grandchildren. Personally, I prefer the scheme indicated in the earlier part of this answer, but to those who may think it too drastic I commend this milder alternative.

'P.S.—The trade depression has presented the Government with an excellent opportunity of reducing the legal maximum *pari passu* with the actual contraction in the currency note issue. But this opportunity has been thrown away, and there is no legal barrier to an equivalent expansion in the currency note issue in the near future. There may be a barrier of policy, but, in view of recent reductions in the bank rate and of the great political influence of inflationist "big business," I doubt it.'

'Mr. Gibson contends that the course of the London-New York exchange in future will largely be dependent on economic and financial conditions on the Continent of Europe, for this important exchange is the great trunk line of settlement between Europe and North America. He disagrees with Dr. Dalton's opinion that Great Britain by her own action can restore this exchange to parity.'

APPENDIX.

International Credits.

By Sir Drummond Drummond Fraser, *K.B.E.*, *M.Com.*

The International Credits Scheme is the scheme of Mr. ter Meulen, unanimously adopted by the International Financial Conference at Brussels in September, 1920, of which I was appointed Organiser in March, 1921, by means of which necessitous nations, with approved credits, may be enabled to finance essential imports.

An International Commission of bankers and business experts is to be appointed by the League of Nations, who will have power to determine the gold value of the assets offered by the Governments of these countries. The Governments will then issue bonds to the gold value of their pledged assets, which will be specifically secured by the revenue from these assets. These assigned assets will be so administered by the participating Governments, or by the International Commission, that the bond-holders will be secured against default or loss.

Guarantee.—The bonds are issued by the Governments—when the condition of their internal currency justifies the issue—against revenue-producing assets. They are lent by the Governments to their importers, who offer them as a guarantee that they will pay the exporter by the proceeds of the goods manufactured by them.

Negotiability.—For ordinary commercial transactions banks in lending countries will find the necessary accommodation, because the bonds are a 'satisfactory security' (the British Government will guarantee 85 per cent. of the risk to manufacturers). When the guarantee is used for reconstructive purposes and a number of years is required for the development of productive enterprises, Credit Associations will be established to attract investors' money on the bond principle in order to finance the purchase of goods on a long-time credit in place of the short-time credit afforded by banks. (The British Government will in this case guarantee 70 per cent. of the capital supplied by the banks, financial groups, &c.)

Realisation.—In the event of the necessity to realise the guarantee, because the importer has failed to carry out his obligation to the exporter, the exporter, in the first instance, must offer the bond to the issuing Government in exchange for his debt. Should the Government not purchase the bond, he is at liberty to sell it on the open market, or otherwise dispose of it.

It is intended that the accumulation of the sinking fund shall be sufficient to purchase the bonds of defaulters. The object is to create a new capital and credit by international co-operation.

On Certain of the More Complex Stress Distributions in Engineering Materials.

—*Report of Committee* (Prof. E. G. Coker, *Chairman*; Profs. L. N. G. Filon *and* A. Robertson, *Secretaries*; Prof. A. Barr, Dr. C. Chree, Dr. Gilbert Cook, Prof. W. E. Dalby, Sir J. A. Ewing, Mr. A. R. Fulton, Mr. J. J. Guest, Dr. B. P. Haigh, Profs. Sir J. B. Henderson, C. E. Inglis, F. C. Lea, A. E. H. Love, *and* W. Mason, Sir J. E. Petavel, Dr. F. Rogers, Dr. W. A. Scoble, Mr. R. V. Southwell, Dr. T. E. Stanton, Mr. C. E. Stromeyer, Mr. J. S. Wilson.

Introduction.

The Committee submit as their Report the following contributions embodying: (1) A review of recent advances in the special domains of (*a*) Stress Concentrations, (*b*) the Theory of Plane Stress in perforated plates; (2) special researches carried out by members of the Committee during the past year; (3) an account by Prof. B. P. Haigh of his theory of failure of elasticity.

I. Stress Concentrations due to Notches and like Discontinuities. By Prof. E. G. Coker, F.R.S., and Dr. Paul Heymans.

II. The Distribution of Stress in a Flanged Pipe. By Prof. Gilbert Cook, D.Sc.

III. On Stresses in Multiply-connected Plates. By Prof. L. N. G. Filon, F.R.S.

IV. Stress Concentrations in Theory and Practice. By A. R. Griffith, M.Eng.

V. The Strain Energy Function and the Elastic Limit. By Prof. B. P. Haigh, D.Sc., M.B.E.

VI. Alternating Combined Stress Experiments. By W. Mason, D.Sc., and W. J. Delaney, B.Eng.

VII. On some Problems relating to the Design of High-speed Discs. By R. V. Southwell, M.A.

VIII. On the Stability of a Rotating Shaft, subjected simultaneously to End Thrust and Twist. By R. V. Southwell, M.A., and Barbara S. Gough.

IX. The Stresses in Cylinders and Pipes with Eccentric Bore. By G. B. Jeffery, M.A., D.Sc.

The Committee ask for re-appointment, with a grant of 100*l*.

I.

Stress Concentrations due to Notches and Like Discontinuities.

By Prof. E. G. Coker, *F.R.S.*, and Dr. Paul Heymans,
University College, London.

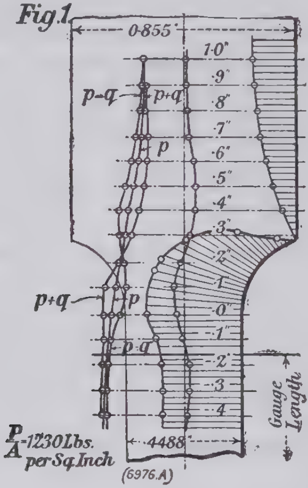
It is well known that discontinuities in loaded members cause, in general, severe stresses in their neighbourhood, and therefore do not allow full economy of material to be realised in cases where such discontinuities are inevitable in practical construction. In the majority of such cases it is difficult and sometimes impossible to calculate the maximum stresses produced, and experience in the behaviour of similar members is then usually relied on, in the absence of an accurate knowledge of the stress distribution which exists. The stress distribution caused by discontinuities has also become of importance in recent years in the testing of materials, and a considerable field of inquiry has developed on the behaviour of materials subjected to impulsive loads, which produce stresses at definitely shaped discontinuities sufficient to cause rupture at the chosen section, and in this way a classification of materials is obtained which has proved of great value in engineering practice, although with our present knowledge such testing operations can only be regarded as of an empirical nature.

In the present report a brief account will be attempted of some experimental investigations which have been made on discontinuities by photo-elastic methods described in a previous report (1), whereby stress distributions have been determined sufficiently completely to allow of a fairly accurate value being assigned to the maximum stresses experienced under given loads. In all these cases the member, a flat bar, has been subjected to plane stress, and the results are, therefore, only applicable strictly to this kind of stress except in some cases which will be noted.

An interesting case of a discontinuity of a simple kind arises where it is necessary to alter the cross section of a member in a definite manner, as for example in a flat tension member or a cylindrical rod. This case occurs in a variety of instances in practice, and the salient fact in connection with this type of discontinuity is the increase of stress which occurs near the join of the smaller to the larger section when the contours are formed by circular arcs, which are tangential to the boundaries of the smaller section and intersect the larger at an angle.

The maximum stresses reached are shown experimentally not to occur exactly at the joins (2), but slightly beyond, at places where a tangent to the contour makes a small angle to the axis of the specimen. Thus, for example, in a case

Fig. 1.



where the smaller breadth of a flat bar is .4488 in. and the larger 0.855 in. connected by arcs of 0.3 in. radius, the maximum stress is found at a point of the curved contour where a tangent line makes an angle of about 8° to the line of pull, and the stress intensity is nearly 20 per cent. greater than the uniform stress in the smaller section.

The distribution in this member at the contour is shown in the accompanying fig. 1, from which it will be observed that a maximum stress of 1,470 lb. per sq. in. is reached when the uniform stress in the smaller section is 1,230 lb. per sq. in. As might be expected, this maximum stress increases with decrease of radius at the re-entrant angle, and in a very nearly similar contour, but with a re-entrant angle of $\frac{1}{16}$ in. radius, an increase of stress of 57.5 per cent. was observed.

The type of contour shown in fig. 1 is probably more frequently met with in British practice than any other. It is, for example, adopted by the Engineering Standards Committee as a standard form of end (with a radius of one inch for the connecting arc) for tension test members of boiler-plates and like material.

As will be observed from the contour stresses, the distribution in the region of the discontinuity is extremely variable and in the interior of the plate is of a complex nature.

Along the central line, for example, there is a gradual change from high to low tensional stress p in the direction of pull, accompanied by a variable cross stress q, which latter has a maximum tensional value very near to the section where the width of the member begins to increase, but it soon changes sign and becomes a small cross compression stress, and ultimately vanishes when the stress in the larger section becomes a uniform tension. A map of the stress in the direction of the line of pull for this case is drawn to a distorted scale both horizontally and vertically (fig. 2), in order to show the distributions at sections one-tenth of an inch apart, and from this it will be observed that the greatest variations of stress occur at the contours, and in general within the region of complex stress the maximum stresses occur at the sides in the smaller section and along the axial line in the larger section. The type of stress distribution shown here does not, however, appear to persist beyond the elastic limit in ductile material, and the variations of stress shown in the figure probably tend to equalise, and hence fracture does not necessarily begin to take place at the point on the contour where the maximum stress is indicated. In brittle materials, however, where little change occurs in the type of stress distribution, it is found that fracture occurs very frequently at this cross section. This latter result, which is often ascribed, and generally erroneously so, to imperfect centering of the specimen in the testing machine, is therefore more probably explained by the facts of the distribution observed.

Another fact which emerges from the experimental observations is the penetration of complex stress into the parallel part of the reduced section, and it is easy to show for this case, where the model is one of three-tenths scale of an Engineering Standards Committee test bar, that complex stress occurs for a distance of .185 in. within the parallel part, so that in a specimen of this size and contour, and a parallel part 0.37 in. long, there is no pure tension stress at any cross section between the enlarged ends except possibly the central one. In a geometrically similar test bar with connecting arcs of one inch radius it would appear, therefore, that a total length of 1.23 in. of the parallel part is in a state of complex stress, and since it is found experimentally that complex stress distribution at the ends does not vary to an appreciable extent as the length of the parallel part increases beyond this, it may be inferred that, within the elastic limit, a sufficiently short tension test member under load may be in a state of complex stress, which has little or no resemblance to pure tensional stress at any part of it, and it is therefore likely to give fallacious results for practical applications.

In the example cited above the limiting length of the parallel part for which there is no pure tension stress across the sections between the shoulders is 1.23 in., and for such a case complex stress occurs at all sections except possibly the central one.

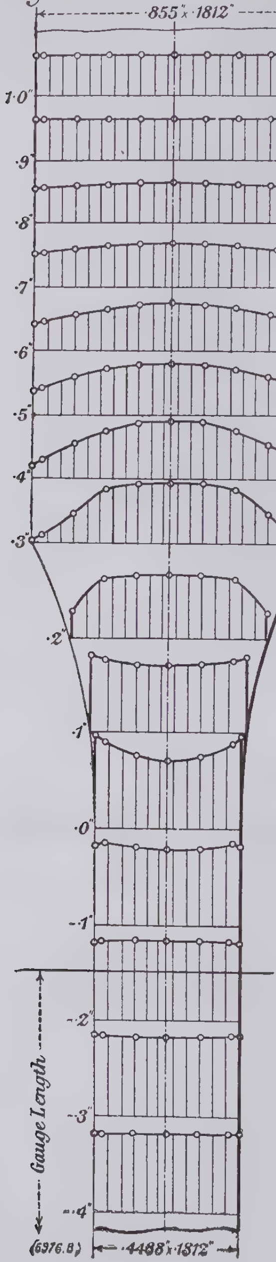
The danger of accepting tension tests on short specimens as representing the true behaviour of the material in pure tension is clear, especially as it is not difficult to show (2) that in turned specimens of the same contour as a flat bar specimen the complex stress in the former extends further into the gauge length than in the latter.

In another standard form widely used in Continental practice the enlarged ends are gradually tapered towards the gauge length and have straight line contours, which join the parallel part at a very obtuse angle. At this junction photo-elastic observations show that there is a small amount of stress concentration probably not more than 10 per cent. greater than the stress intensity in the gauge length and, moreover, very localised in extent.

In still another form, used by Professor Dalby, mainly to avoid contact stresses and local indentations of extensometer screws, the extremities of the gauge length are defined by two thin collars turned on the specimen with connecting arcs to the main body of 0.04 in. radius. In this case the stress concentration is very local, and is rather more than 30 per cent. (3) of the mean average stress, and is symmetrically disposed with reference to the collar. It is, moreover, practically independent of the radius of the fillet within wide limits, and in that respect differs from the stress at a re-entrant angle, where the radius of the fillet is the main factor which determines the stress concentration.

Observations of stress distribution by photo-elastic means indicate, in general, that the stress concentrations described above tend to equalise more or

Fig. 2.



less in the plastic stage. Thus in a nitro-cellulose model, of the form used by the Engineering Standards Committee, it is observed that the varied colour effects, which mark the penetration of complex stress within the gauge length, tend to recede towards the enlarged ends as the material passes into the plastic state, and in general at fracture the line of greatest colour variation is concentrated near to the section where the curved contours meet the parallel part of the gauge length, so that it seems possible in ductile material that nearly the whole of the parallel part of the gauge length ultimately comes into a state of uniform stress.

Discontinuities in Materials Subjected to Impact Tests.

The practical necessity of ascertaining the ability of materials to withstand suddenly applied loads has led to the introduction of shock tests of various kinds, and these are generally arranged in such a manner that fracture of the specimen shall occur, generally, with a single application of a load applied by a falling weight or swinging pendulum. Such tests are essentially different

Fig. 3a. STRESS DISTRIBUTION AT THE CONTOUR OF A VEE SHAPED NOTCH.

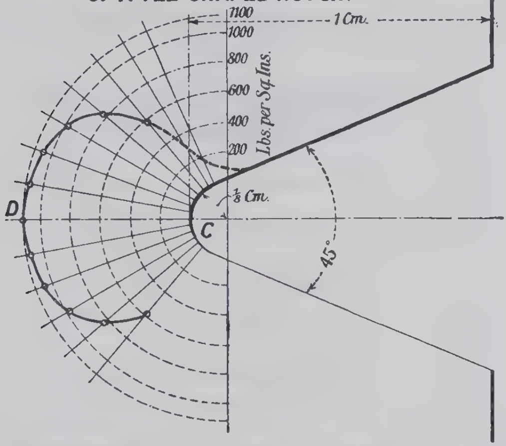
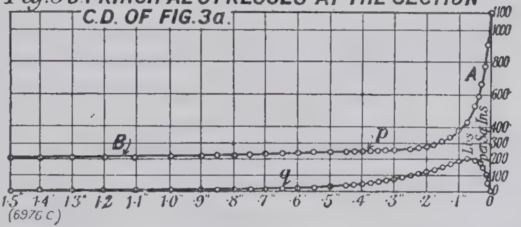


Fig. 3b. PRINCIPAL STRESSES AT THE SECTION C.D. OF FIG. 3a.



from static tests of materials, and in general classify them in a different manner, for it is quite possible to have materials which give similar results under static loading and yet behave very differently under impact tests.

Although there cannot be any doubt of the importance of impact tests in affording an element of value in determining the properties of a material to resist stress, yet there is a considerable difference of opinion as to the exact nature of the information afforded, and this is hardly surprising when the number of variables is considered which enter into the problem.

The various machines designed for impact testing differ greatly in the manner and swiftness with which the load is applied, in the rigidity of the frame and

clamping devices for the test specimen, while the form of the piece also varies very much especially as regards the notch cut in it to fix the place of failure. These and other circumstances tend to explain why it is so difficult, at present, to correlate the results obtained in a complete and satisfactory manner.

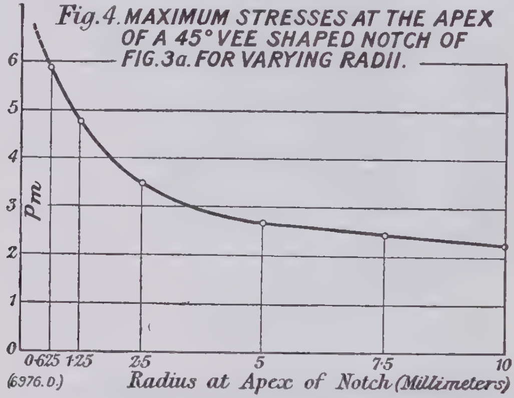
An important element for consideration is the stress distribution produced under load in various standard types of discontinuity, and it is proposed to examine some simple cases here, and discuss their bearing on the problem.

It is evident from other cases of discontinuity already described that, within the elastic limit, the contour of the notch is of chief importance as regards stress concentration, and that whether the notch be subjected to bending or tensional stress, the salient facts will be much the same at the contour. There is, however, a considerable difference in the experimental difficulties, and very little to be gained in a preliminary inquiry by examining a notch under the former type of stress. In all these experiments, therefore, notches are cut of considerable size in a very wide plate of transparent material, and this plate is subjected to uniform tensional stress in a testing machine in order to find the stress at the notch contour and across the principal section through the line of symmetry.

To obtain symmetrical conditions about the line of pull a second notch is cut in the plate at the opposite edge.

In some standard forms used in impact testing the notch is of V form with sides inclined at 45°, and with a definite radius of curvature at the apex.

Fig. 4. MAXIMUM STRESSES AT THE APEX OF A 45° VEE SHAPED NOTCH OF FIG. 3a. FOR VARYING RADII.



Such a notch is shown on the accompanying fig. 3A, one centimetre deep with a radius of $\frac{1}{8}$ cm. at the apex and cut in a plate 13 cm. wide. Under load there is no great difficulty in measuring the distribution of stress at any point of a plate of this size, and when this is carried out along the minimum cross section perpendicular to the line of pull, a measure of the general accuracy of the observations is obtained, since the value $\int p.dx$ along this line should be equal to the load registered by the testing machine.

The values of p and the cross stress q in this case are shown in the accompanying diagram (fig. 3B), from which it appears that the stress rises to a maximum of 1,100 lb. per sq. in. accompanied by a cross stress q, which is very small except near the ends, where a maximum value of about 200 pounds per square inch is reached at a distance of between 0.3 to 0.4 of a centimetre from the edge of the notch. The mean stress as given by the testing machine is 230 lb. per sq. in., while the integrated value of the curve of distribution corresponds with 240 lb. per sq. in., or about 4.8 per cent. in excess. Assuming the correctness of the testing mahcine reading, the stress concentration at the notch is 4.78 times the average stress at the cross section, and for an angular distance of about 30° on each side of this, the stress at the contour does not fall below 4.40 times of this value, but beyond this the stress falls rapidly to zero along the notch contour. If, however, the radius of the

notch is decreased to $\frac{1}{16}$ cm., the maximum stress rises to nearly six times the mean stress, as fig. 4 shows, while if the radius is very large, say 10 mm., the maximum stress is approximately 2.2 times the mean stress.

It seems evident from these experiments and others described below that an impact test on a notched specimen affords a valuable means of discriminating between ductile and brittle material, for whatever be the radius of curvature at the apex of the notch in a ductile material, it acts primarily as an indicator of the place where fracture is to commence, but once plastic stress begins, its influence in maintaining the high stress concentration observed here must recede in importance, although it is quite possible its form may be a factor in the final result. In hard and brittle materials, however, the radius at the bottom of the notch is apparently a dominant factor since the stress distributions

Fig. 5a. STRESS DISTRIBUTION AT THE CONTOUR OF A NOTCH OF THE CHARPY TYPE.

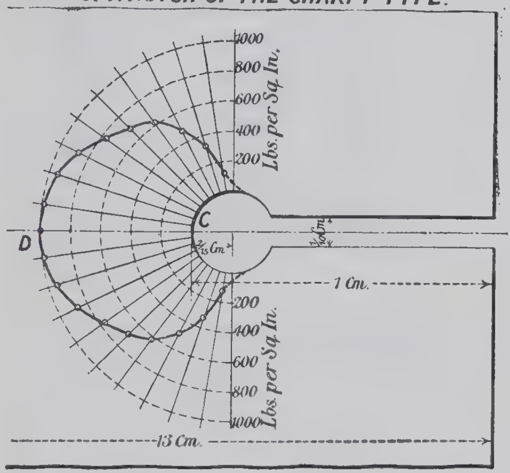
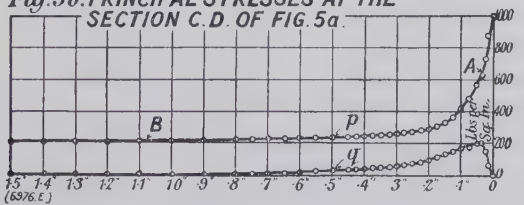


Fig. 5b. PRINCIPAL STRESSES AT THE SECTION C.D. OF FIG. 5a.



observed here can suffer little change in type throughout, and when due allowance is taken of static breaking stress in tension, such materials must have a comparatively low impact value on account of the maintenance of the relatively high stress concentration.

As a discriminating test of quality an impact test may possibly be too severe, and good material may be rejected by it, since the radius of curvature of the notch is a factor of variable importance according as the material is or is not ductile. In very hard materials there seems good evidence for bringing this factor into account in grading materials by impact tests.

This is more particularly emphasised when tests under different conditions of notch form are considered, as, for example, in one of the Charpy type, in which all the conditions of experiment are exactly the same as before, with a notch 1 cm. deep rounded off at the apex by an arc of $\frac{2}{15}$ cm. radius, (fig. 5A). Measuring the general accuracy of the experimental values by a comparison of the average stress intensity of 282 lb. per sq. in. recorded by the testing machine with the integrated stress intensity curve AB of fig. 5B,

we find in this case a value of 289 lb. per sq. in., or 2.48 per cent. in excess, with a maximum stress of 1,000 lb. per sq. in. at the notch contour, and a concentration stress factor of 3.54, very much less in fact than is afforded by the standard notch of the form used in Izod machines and described above.

This difference in concentration of stress is even more noticeable when the notch is of rectangular cross section (fig. 6A) with angles rounded off to a radius of 0.2 cm. for a notch 1 cm. deep. In this case the mean stress of 230 lb. per sq. in. recorded by the testing machine agrees remarkably well

Fig. 6a. STRESS DISTRIBUTION AT THE CONTOUR OF A STANDARD RECTANGULAR NOTCH WITH ROUNDED CORNERS.

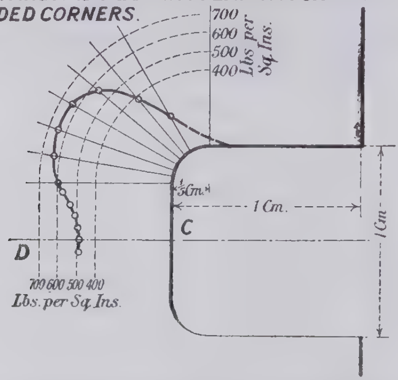
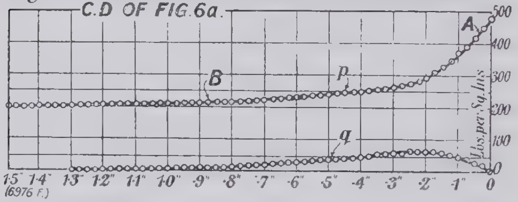


Fig. 6b. PRINCIPAL STRESSES AT THE SECTION C.D OF FIG. 6a.



with the mean value of 229 lb. per sq. in. afforded by the curve AB of stress distribution across the principal cross section (fig. 6B), and we now find the maximum stress at points in the contour where a tangent line makes a slight inclination to the line of pull. Its value is 640 lb. per sq. in., and gives the low stress concentration factor of 2.78.

These results seem to show that the form of the notch in an impact test not only exercises a very considerable influence on the results, but any one type of notch may cause that influence to be exerted in a variable manner according to the material tested.

As a test it is undoubtedly complex and even more difficult to analyse in the beam form, and its simplification, if that were possible, would be a great advantage.

It might, for example, be worth while to examine the possibilities of an impact tension test specimen formed by drilling out a very large hole centrally placed in a flat tension member, since this form of discontinuity has been shown (4) to cause an approximately linear stress distribution across the minimum section with a maximum value at the inner contour, and a minimum at the outer contour, which is nearly but not quite zero. In hard materials, therefore, where the elastic stress distribution probably remains practically unchanged up to fracture, the maximum stress is very approximately twice the mean value, while in ductile materials photo-elastic indications appear to show that the stress distribution becomes fairly uniform at fracture.

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(4) 'Photo-Elastic and Strain Measurements of the Effects of Circular Holes on the Distribution of Stress in Tension Members.' By Prof. E. G. Coker, K. C. Chakko, and Y. Satake. *Trans. The Inst. of Engrs. and Shipbuilders in Scotland.* Vol. lxiii. Part i.

II.

The Distribution of Stress in a Flanged Pipe.

By GILBERT COOK, *D.Sc.*

The problem of the elastic deformation of a thin cylindrical shell subjected to internal pressure when certain boundary conditions restricting the displacement at the ends are to be satisfied has been investigated theoretically by Love,[1] and as applied to the case of a short boiler, in more detail by Nicolson.[2] The particular types of end constraint considered were those in which the ends of the tube remain circular and suffer no radial displacement. In a discussion of certain experiments on short boilers[3] it has been shown that the stress

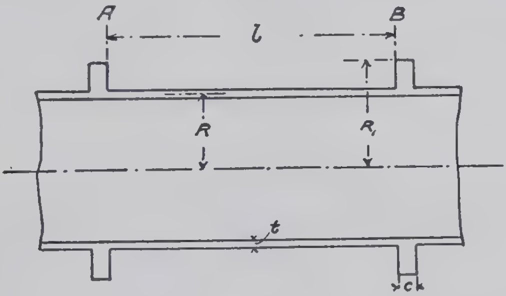

FIG. 7.

distribution in the vicinity of the end constraints is of a somewhat peculiar nature, inasmuch as over a certain region the radial deformation and the corresponding circumferential stress are greater than that which would obtain in an infinitely long tube, but little experimental evidence of this fact appears to have been forthcoming. A third type of constraint which possesses considerable practical interest is that produced by a flange or collar at any point in the length of the tube. At this point the inclination of the generators of the cylinder to the axis is, by symmetry, zero. The radial displacement is not zero, but is determinate, and the constraint is of a type which may be more accurately and conveniently reproduced experimentally than those referred to above.

The general equation of equilibrium for any type of end constraint may be very simply obtained as follows:

Let t be the thickness of the tube (fig. 7), assumed small in comparison with R, the mean radius. Let z be the radial displacement, q the circumferential

[1] *Mathematical Theory of Elasticity.* Third Ed. Art. 340.
[2] Nicolson, 'The Strength of Short Flat-ended Cylindrical Boilers.' *Trans. N.E. Coast Inst. Engineers and Shipbuilders.* Vol. vii., p. 205.
[3] *Engineering.* Vol. 51 (1891), p. 468.

stress, and f the longitudinal stress (tensile) in the shell at any point distant x measured along the axis from some fixed point 0. Let P be the internal pressure.

Then the circumferential strain $\frac{z}{R}=\frac{q}{E}-\frac{f}{mE}$, where E is Young's Modulus, and $\frac{1}{m}$ Poisson's Ratio.

So that

$$q=\frac{Ez}{R}+\frac{f}{m} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Consider the equilibrium of a longitudinal strip of the shell subtending an angle $\delta\phi$ at the axis. This may be considered to act as a beam supported in some arbitrary manner at the points of constraint, and to sustain a load in the plane of bending which will be the resultant in that plane of the internal pressure and the circumferential stress. The internal pressure P will produce a uniform load intensity equal to $PR\,\delta\phi$ per unit length, while the component, in the plane of bending, of the load due to the circumferential stress q will be equal to $qt\delta\,\phi$ The component of the longitudinal stress will be negligible. The resultant intensity of load per unit length in the plane of bending is therefore

$$w=PR\delta\phi-qt\delta\phi$$

The equation connecting load and displacement in a beam is

$$\beta\frac{d^4z}{dx^4}=w$$

where β is the flexural stiffness, which for a thin plate is equal to

$$E\frac{m^2}{m^2-1}\,.\,I^{[4]}$$

$$=E\frac{m^2}{m^2-1}\,.\,R\delta\phi\,.\,\frac{t^3}{12}$$

So that we have $\quad E\frac{m^2}{m^2-1}\,\frac{t^3}{12}\,R\delta\phi\,\frac{d^4z}{dx^4}=PR\delta\phi=qt\delta\phi$

Inserting the value of q from equation (1), we obtain the equation of equilibrium

$$E\frac{m^2}{m^2-1}\,\frac{t^3}{12}\,\frac{d^4z}{dx^4}+\frac{Et}{R^2}z=P-\frac{ft}{mR} \quad . \quad . \quad . \quad . \quad (2)$$

If it be assumed that the longitudinal stress is that due to the internal pressure when the ends of the tube are closed,

$$f=\frac{PR}{2t}$$

and the equation becomes $\quad E\frac{m^2}{m^2-1}\,.\,\frac{t^3}{12}\,.\,\frac{d^4z}{dx^4}+\frac{Et}{R^2}z=P' \quad . \quad . \quad . \quad . \quad (3)$

where $\quad P'=P\,.\,\frac{2m-1}{2m}$

It reduces to the form $\quad \frac{d^4z}{dx^4}+4n^4z=b$

where $\quad n^4=\frac{3}{t^2R^2}\,.\,\frac{m^2-1}{m^2}$

$$b=\frac{12}{Et^3}\,.\,\frac{m^2-1}{m^2}\,.\,P'$$

[4] This statement involves the assumption that the circumferential stress in the tube has no effect on the flexural stiffness. A more rigorous, but less simple, analysis leading to equation (2) is given by Nicolson, *loc. cit.*, pp. 205-214.

and $$\frac{b}{4n^4}=\frac{P'R^2}{Et}$$

The solution of this equation is

$$z=\cos nx\,(A\cosh nx+B\sinh nx)+\sin nx\,(C\cosh nx+D\sinh nx)+\frac{P'R^2}{Et} \quad . \quad (4)$$

Consider a length AB ($=l$) of the pipe between successive flanges, it being assumed for the present purpose that the flanges are spaced equidistantly along the pipe, and let the origin for x be at A (fig. 7).

It is evident, from symmetry, that

$$\frac{dz}{dx}=o \text{ for } x=o \text{ and } x=l$$

and also $$z=z_0 \text{ for } x=o \text{ and } x=l$$

where z_0 is the radial displacement at the flange, as yet undetermined. These four conditions enable the four arbitrary constants A, B, C, and D to be expressed in terms of known quantities and z_0, and equation (4) becomes

$$z=\frac{P^1R^2}{Et}-\left(\frac{P^1R^2}{Et}-z_0\right)\left(\cosh nx\cos nx-H\sinh nx\cos nx+H\cosh nx\sin nx\right.$$
$$\left.-L\sinh nx\sin nx\right) \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where $$H=\frac{\cosh nl-\cos nl}{\sinh nl+\sin nl}$$

$$L=\frac{\sinh nl-\sin nl}{\sinh nl+\sin nl}$$

When the distance l between successive flanges is large, H and L both approximate to a value 1 and the equation giving the radial displacement becomes

$$z=z_1-(z_1-z_0)\,(\cosh nx-\sinh nx)\,(\cos nx+\sin nx)$$

or $$z=z_1-(z_1-z_0)\,e^{-nx}(\sin nx+\cos nx) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where $z_1=\frac{P'R^2}{Et}$, the radial displacement in a uniform tube without flanges.

It remains to determine z_0, the radial displacement at the flange. For this purpose it will be assumed that the flange and the part of the tube beneath it form a homogeneous ring of longitudinal thickness c and external radius R_1. The forces acting on the ring tending to increase its diameter will be those due to (1) the internal pressure acting on its inner surface, and (2) the reactions between the ring and the adjacent tube. The latter will consist of a shearing force acting radially outwards on the flange over the whole circumference of the tube. Denoting this shearing force per unit length measured along the circumference of the pipe by F, the resultant outward force per unit length of the circumference will be

$$Pc+2F$$

and the equivalent pressure per unit area of the internal surface of the flange, tending to increase the diameter, will be

$$\frac{Pc+2F}{c}$$

$$=P+\frac{2F}{c}$$

It will be assumed that the longitudinal stress in the flange is confined to, and uniformly distributed over, a ring of thickness equal to the thickness of the tube. The flange may then be regarded as a compound cylinder under internal pressure $P+\frac{2F}{c}$ the inner portion whose thickness is t having a longitudinal stress equal to that in the tube due to the pressure P. Observing that at the common surface the radial stress and radial displacement have equal values in

the two portions, the radial displacement at radius R can be shown, by the ordinary theory of the stress in a thick cylinder, to be approximately (*i.e.* neglecting squares of $\frac{t}{R}$)

$$Z_0 = \left\{ P.\frac{2m-1}{2m}+\frac{2F}{c} \right\} \frac{R(R-t)}{Em} \cdot \frac{R_1^2(m+1)+R^2(m-1)}{R_1^2(R+t)-R^2(R-t)}$$

$$= \left\{ P.\frac{2m-1}{2m}+\frac{2F}{c} \right\}. \text{ J, say} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Now the shearing force at any point of a beam is equal to

$$\beta\frac{d^3z}{dx^3}$$

where β is, as before, the flexural stiffness. At any point of a longitudinal strip (of unit width) of the tube wall under consideration, it will be given by

$$E\frac{m^2}{m^2-1} \cdot \frac{t^3}{12} \cdot \frac{d^3z}{dx^3} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Performing the required differentiation on the expression for z given by equation (6), and putting $x=0$, the expression (8) becomes

$$-\frac{1}{3}\frac{Em^2t^3}{m^2-1} \cdot n^3(z_1-z_0)$$

which, from a consideration of the signs, will be the shearing force at the end of the strip acting *inwards* on the flange. The value of F, acting outwards, will therefore be

$$F=\frac{E}{3} \cdot \frac{m^2}{m^2-1} \cdot t^3n^3(z_1-z_0)$$

The substitution of this value of F in equation (7) enables z_0 to be determined.

Thus

$$z_0=\frac{P \cdot \frac{2m-1}{2m}+\frac{2}{3}\frac{m^2}{m^2-1} \cdot \frac{E}{c}n^3t^3z_1}{\frac{1}{J}+\frac{2}{3}\frac{m^2}{m^2-1} \cdot \frac{E}{c}n^3t^3}$$

and this value of z_0 substituted in equation (6) will enable the radial displacement at any point to be determined, and from this the circumferential stress by equation (1).

It will be seen from equation (6) that the expression for the radial displacement contains a periodic function of the distance from the flange, and that the longitudinal profile consists of a series of waves whose amplitude rapidly diminishes with the distance from the flange.

Thus, if the equation be written in the form

$$z=z_1-(z_1-z_0)\phi(x)$$

$\phi(x)$ reaches a negative maximum when $x=\frac{\pi}{n}, \frac{3\pi}{n}, \frac{5\pi}{n} \ldots$

For steel, taking $\frac{1}{m}=0{\cdot}295$, these values become

$$x=2{\cdot}44\sqrt{tR},\ 7{\cdot}32\sqrt{tR},\ 12{\cdot}20\sqrt{tR} \ldots$$

and $$\phi(x)=-{\cdot}0432,\ -{\cdot}0019,\ -{\cdot}00008 \ldots$$

It would appear therefore that the constraint imposed by a flange produces an appreciable increase in radial displacement over a region extending from

$$x=\frac{3\pi}{4n} \text{ to } x=\frac{7\pi}{4n}$$

i.e. $$x=1{\cdot}83\sqrt{tR} \text{ to } x=4{\cdot}27\sqrt{tR}$$

In the limiting case in which displacement at the flange is entirely prevented the increase in stress may thus amount to 4.3 per cent. of the normal stress. The elastic deformation at the flange will reduce this figure, but it is sufficiently great to be measurable experimentally.

A tube of mild steel was accordingly machined accurately to an internal diameter of 1 in. and an external diameter of 1.05 in., a collar being left on the tube having an external diameter of 1.5 in. and a thickness of .038 in. Connections were provided at the ends for attachment to a pump and gauge, and the tube was free to take up the longitudinal stress produced by the internal pressure applied. The increase in diameter was measured by an instrument specially designed for the purpose, and similar in principle to that previously used in another connection[5] by the author and Professor Robertson. An outline sketch of the instrument is shown in fig. 8. A light frame A is held on the cylinder by means of two flat springs B so that the steel point P_1 is set at one end of the diameter whose extension is to be measured. In contact with the cylinder at the other end of the diameter is a second steel point P_2 carried by a level L, which is free to turn about the knife edge D, and is held in position by the light spring S_1 (consisting of a rubber thread). Between V's in the outer end of the lever L and a vertical bar E supported as shown in the sketch from the frame A, is held a small diamond-shaped rocking piece R possessing two knife edges, one edge resting in the V's, and the other against

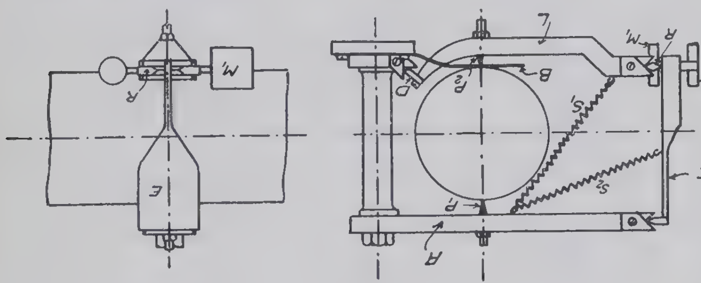

FIG. 8.

the flat surface of the vertical bar E. The rocking piece carries on the continuation of its axis a mirror M_1 and a counterpoise. The angular rotation of this mirror is proportional to the change in diameter between the steel points P_1P_2, and is measured by means of a telescope and scale. A mirror M_2 (not shown in the right-hand view) fixed to the vertical bar E serves to detect any displacement of the instrument as a whole. In these experiments the scale was placed at a distance of 15 feet from the mirrors, and by using a telescope having a linear magnification of about 25, and a fine cross wire, the scale could easily be read to 0.1 mm. Successive readings of the scale for the same pressure range differed by not more than 0.1 to 0.2 mm. on a total reading (for parts of the tube remote from the flange) of about 40 mm. The lever ratio being 2.85, and the rocking piece being 0.25 in. broad, diametrical extensions could be measured to an accuracy of the order of 10^{-6} in. To secure this degree of accuracy it was found necessary to mount the apparatus on a massive pedestal supported by felt pads to damp out small tremors, and to maintain a constant temperature. It was not possible to take accurate measurements within half an hour of handling the instrument.

The pressure gauge used was a standard steel-tube test gauge made by the Budenberg Gauge Co.

In the test the increase in diameter was measured at various distances from the flange when the pressure was raised from 100 to 900 lb. per sq. in., and the results are tabulated below, together with the values calculated by the theory described above. For the latter purpose an accurate determination

[5] *Engineering*, December 15, 1911.

of the elastic constants for the material was made. Young's Modulus (for longitudinal stress) was found from the longitudinal extension in a cylindrical bar, and Poisson's Ratio from a measurement of the lateral contraction of the same bar by the instrument used on the tubes, axial loading shackles being employed to secure uniformity of stress. The values thus found were

$$E=29{\cdot}88\times10^{6}\text{ lb. per sq. in.}$$

$$\frac{1}{m}=0{\cdot}295.$$

The results are also plotted in fig. 9, and it will be seen that the experimental results follow closely the theoretical curve. The small discrepancy which appears at points remote from the flange is entirely accounted for by the fact

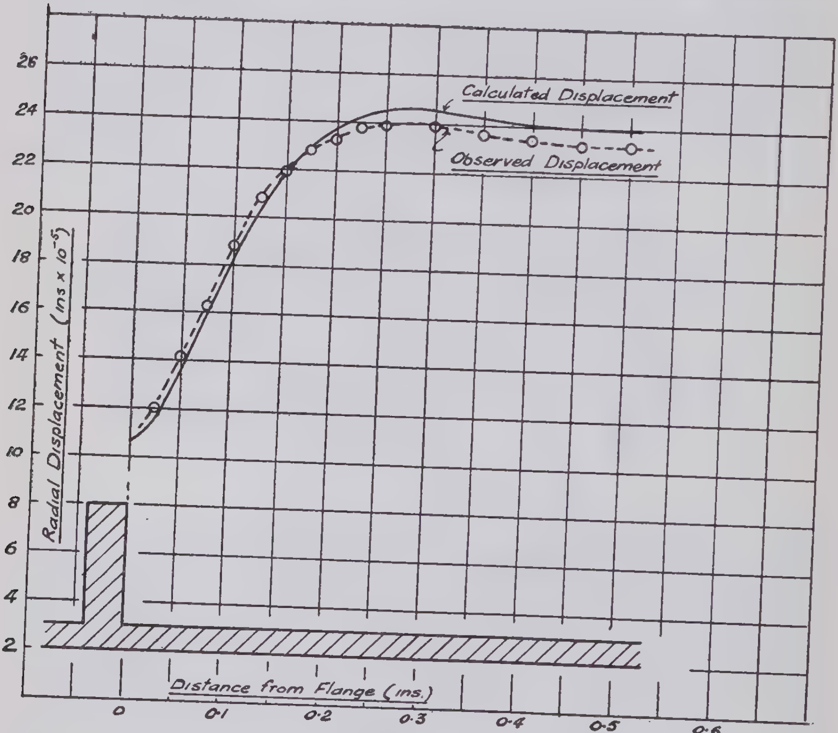

FIG. 9.

that the actual extensions were measured on the outside of a tube of appreciable thickness, whereas the theory assumes uniform stress across the walls. Thus, the radial extension at the outside of a plain tube is given by

$$\frac{PR'}{E}\left(2-\frac{1}{m}\right)\frac{R_0^2}{R'^2-R_0^2}$$

where R' and R_0 are the external and internal radii respectively, giving, with the present data, a value 23.95×10^{-5}, almost identical with that observed in the test. The discrepancy near the flange is probably due to errors in the assumptions involved in the calculation of the displacement of the flange. The point of maximum displacement is situated at a distance of about .28 in. (as nearly as can be measured) from the flange, the calculated distance being .276 in.

The actual increase at this point is 4.5 per cent. of the difference between the displacement remote from the flange and at the flange, which approaches very closely the calculated value of 4.3 per cent.

Tests have also been carried out on tubes of greater thickness and with broader flanges, the results obtained being very similar to those described above, but, as would be expected, the agreement of theory and experiment is not so close.

The results of this investigation have an important bearing on the strength of steel pipes reinforced by steel bands, such as are occasionally used in pipe lines for hydro-electric installations. Experimental work in this connection is at present in progress.

Distance from Flange (ins.)	Radial Displacement, for Pressure 800 lb. per sq. in.	
	Calculated (ins. $\times 10^{-5}$)	Observed (ins. $\times 10^{-5}$)
0	10·60	—
·025	11·53	12·05
·05	13·60	14·20
·075	15·94	16·30
·100	18·16	18·80
·125	20·31	20·80
·150	21·92	21·95
·175	22·95	22·80
·200	23·78	23·30
·225	24·22	23·80
·250	24·46	23·85
·30	24·52	23·90
·35	24·32	23·70
·40	24·10	23·50
·45	24·06	23·35
·50	24·02	23·35

III.

On Stresses in Multiply-connected Plates.

By Professor L. N. G. Filon, *M.A., D.Sc., F.R.S., University College, London.*

1. It is a well-known theorem, due to Airy, that in a problem of plane strain, the axes of x and y being in the plane of the strain, the stresses can be expressed in terms of a single function E by the equations

$$\widehat{xx}=\frac{\delta^2 E}{\delta y^2},\quad \widehat{xy}=-\frac{\delta^2 E}{\delta x\,\delta y},\quad \widehat{yy}=\frac{\delta^2 E}{\delta x^2}, \qquad (1)$$

$\widehat{ns}$ denoting the stress, parallel to s, across a face perpendicular to n.

This result can be extended to the case where the stress $\widehat{zz}$ normal to the faces of a thick plate vanishes throughout. In this case, P, Q, S denoting the *mean* stresses taken across the faces of the plate

$$P=\frac{\delta^2 E}{\delta y^2},\quad Q=\frac{\delta^2 E}{\delta x^2},\quad S=-\frac{\delta^2 E}{\delta x\delta y} \qquad (2)$$

In either case the function E, which is called Airy's stress function, satisfies the equation

$$\nabla^4 E=0 \qquad (3)$$

If λ, μ are the elastic constants of Lamé, we have, in the first case, the stresses $\widehat{xx}$, $\widehat{xy}$, $\widehat{yy}$ given in terms of the displacements u, v by the equations

$$\left.\begin{aligned}\widehat{xx} &= \lambda\left(\frac{\delta u}{\delta x}+\frac{\delta v}{\delta y}\right)+2\mu\frac{\delta u}{\delta x}\\ \widehat{yy} &= \lambda\left(\frac{\delta u}{\delta x}+\frac{\delta v}{\delta y}\right)+2\mu\frac{\delta v}{\delta y}\\ \widehat{xy} &= \mu\left(\frac{\delta u}{\delta y}+\frac{\delta v}{\delta x}\right)\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

In the second case P, Q, S are given in terms of the mean displacements U, V by precisely similar equations, λ being replaced by λ', where $\lambda' = 2\lambda\mu/(\lambda+2\mu)$. The two problems are thus analytically the same, and in what follows we shall confine ourselves to the second one, which is known as that of generalised plane stress.

2. The conditions at a boundary, where n is the direction of the outwards normal whose direction cosines are l, m, are given by

$$\left.\begin{aligned}X &= lP + mS\\ Y &= lS + mQ\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

X and Y being the mean stresses across the boundary parallel to the axes.

Equations (5) can be written in the form

$$X = \frac{\delta}{\delta s}\left(\frac{\delta E}{\delta y}\right) \quad Y = -\frac{\delta}{\delta s}\left(\frac{\delta E}{\delta x}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

ds being an element of the boundary.

Since the differential equation (3) satisfied by E and the boundary conditions (6) for E do not involve the elastic constants, it would at first sight appear that the solution for E, for any given set of boundary stresses, cannot involve the elastic constants and that the stress-distribution in a plate, under given applied boundary stresses in the plane of the plate, is independent of the material of the plate, provided only it be elastic.

This proposition, if true, is of great practical importance, because it shows that if we can find the stress distribution in a plate by any method whatever. for example by the exploration of a plate of xylonite by means of polarised light, the results, so far as the stresses are concerned, can be transferred immediately to a plate of any other material, such as steel, provided the applied stress-system is the same.

It was first shown by J. H. Michell (1) that the theorem in question does not hold when the area of the plate is *multiply connected*, that is, when it contains one or more *holes*, so that closed circuits can be drawn inside the plate, which cannot be made to shrink into a point by continuous deformation without passing outside the material of the plate.

The problem was discussed by A. Timpe (2) in connection with a ring boundary bounded by concentric circles, and Timpe was the first to point out that the failure of the theorem was due to the existence of types of strain in such a multiply-connected plate, created by removing a thin strip or wedge of material, thus cutting a thin channel or gap, with straight edges, from the hole to the outer boundary and then closing up the gap and cementing its faces together. We then obtain a stressed ring, although no forces are applied to the circular boundaries.

More recently Prof. Vito Volterra (3) has given a general theory of such types of strain, to which he has given the name of *distorsions*, and for which Prof. A. E. H. Love (4) has proposed the English term *dislocations*. Prof. Volterra's account is not restricted to two dimensions, and he has incorporated in it various scattered results obtained by Weingarten. Cesaro, and Tedone. He does not, however, discuss the question whether the solution is independent of the elastic constants, as this question does not really arise in the three-dimensional case. So far as I have been able to find out, Michell's has been the latest statement on this particular subject.

3. This result of Michell's necessarily throws doubt upon the general applicability of investigations with xylonite models to ordinary engineering structures, since the ratio of the elastic constants is different in xylonite and in the usual engineering materials. M. Mesnager (5), in answering a similar objection to his use of glass models,

appears, however, unaware that the unique determination of the stress function, independently of the elastic constants, is subject to exceptions.

The following sections give a short account of the theory of such dislocations, in a plane, on lines somewhat more direct than those followed by Michell and Volterra. One important result obtained by this method of attack is to show that the corrections to the stresses under a given system of loads, when we pass from one material to another, can be obtained directly from exploration of the stresses in one kind of material only, by a subsidiary experiment with a dislocated plate, provided Poisson's ratio is known for both materials; this correction, for any given shape of plate and applied force system, can thus be determined experimentally by optical means, even when a mathematical solution is not available.

It seemed also of interest to determine, for a shape of plate which allows of mathematical solution, an upper limit to the divergences to be expected in the stresses, when we pass from one material to another. This has been done here for a ring-shaped plate; the final results are simple in form and give indications which should be valuable to the engineer. In particular they show that if the width of the ring be moderate in comparison with its radius, the corrections, in practice, will be very small, at any rate at all points where the stresses are of any magnitude.

4. In what follows we shall have to consider functions of which the derived functions of a certain order are one-valued, whilst the functions themselves and their derived functions of lower order are many-valued.

Let ϕ be a function of x, y, and let its differential coefficients be defined in a non-ambiguous manner all over the area contained by a closed circuit APBQA (fig. 10),

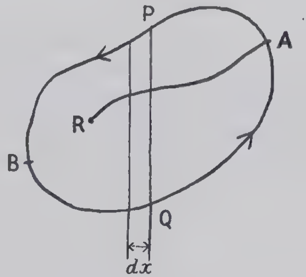

FIG. 10.

which can be made to shrink to a point without passing outside the material. Such a circuit is said to be *reducible*.

Then if $(\phi)_1$ be the value of ϕ selected at A at the beginning of the circuit and $(\phi)_2$ the value reached at A after describing the circuit

$$(\phi)_2-(\phi)_1=\int_A^A \frac{\delta\phi}{\delta s}\,ds \text{ taken round the circuit.}$$

$$=\int_A^A\left(\frac{\delta\phi}{\delta x}\,.\,dx+\frac{\delta\phi}{\delta y}\,dy\right).$$

But since $\frac{\delta\phi}{\delta x}$, $\frac{\delta\phi}{\delta y}$ have been defined in a non-ambiguous manner, then, in the figure

$$\left(\frac{\delta\phi}{\delta x}\right)_P dx_P+\left(\frac{\delta\phi}{\delta x}\right)_Q dx_Q = dx\left[\left(\frac{\delta\phi}{\delta x}\right)_Q-\left(\frac{\delta\phi}{\delta x}\right)_P\right],$$

(bearing in mind the sense of describing the contour)

$$=-dx\int_Q^P \frac{\delta}{\delta y}\left(\frac{\delta\phi}{\delta x}\right)dy$$

where dx, dy are now positive throughout.

Thus

$$\int_{A}^{A} \frac{\delta\phi}{\delta x}\, dx = -\int \frac{\delta}{\delta y}\left(\frac{\delta\phi}{\delta x}\right) dx dy$$

taken over the whole area of the circuit.

Similarly

$$\int_{A}^{A} \frac{\delta\phi}{\delta y}\, dy = +\int \frac{\delta}{\delta x}\left(\frac{\delta\phi}{\delta y}\right) dx dy.$$

Thus

$$(\phi)_2 - (\phi)_1 = \int\left\{\frac{\delta}{\delta x}\left(\frac{\delta\phi}{\delta y}\right) - \frac{\delta}{\delta y}\left(\frac{\delta\phi}{\delta x}\right)\right\} dx dy$$
$$= 0,$$

if inside the circuit $\frac{\delta^2\phi}{\delta x \delta y}$ exists and is defined in an unambiguous manner.

Now it is clear that if

$$\int_{APB} \cdot \frac{\delta\phi}{\delta s} \cdot ds + \int_{BQA} \frac{\delta\phi}{\delta s}\, ds = 0,$$

then, reversing the sign of the second integration

$$\int_{APB} \frac{\delta\phi}{\delta s}\, ds = \int_{AQB} \frac{\delta\phi}{\delta s}\, ds,$$

or

$$(\phi_B - \phi_A) \text{ along APB} = (\phi_B - \phi_A) \text{ along AQB},$$

that is we obtain the same value of ϕ at B if we build it up by integration along any two *reconcilable paths*, starting from a definite selected value at a point A.

The value of ϕ thus obtained at any point which can be reached from A by a continuous path is thus defined in an unambiguous manner, provided the *connectivity* of the path, *i.e.*, the manner in which it winds amongst the holes in the material, is specified.

Now, if A is a point of a reducible circuit, all paths AR from A to a point inside or on the circuit, are clearly reconcilable. We thus get a function ϕ unambiguously defined in the same manner that $\frac{\delta\phi}{\delta x}$ and $\frac{\delta\phi}{\delta y}$ were unambiguously defined.

It follows that we can in this way construct the function ϕ from its first differential coefficients, provided $\frac{\delta\phi}{\delta x}$, $\frac{\delta\phi}{\delta y}$ and $\frac{\delta^2\phi}{\delta x \delta y}$ exist and can be unambiguously defined in the region considered.

If we continue our function ϕ, obtained in this way, round an irreducible circuit (enclosing say a hole in the plate), then on reaching again the same point A we get, in general, a value ϕ_2 different from ϕ_1. We get, however, the same value ϕ_2 for any other irreducible circuit reconcilable with the first one, so that ϕ_2 is a definite function of the co-ordinates x, y of A determined by the connectivity of the irreducible circuit.

We shall call $\phi_2 - \phi_1$ the cyclic function of ϕ for this particular connectivity and denote it by $\mathrm{Cy}(\phi)$; it will usually be convenient to omit the indication of the circuit typical of the connectivity: this may be specified by a suffix denoting the circuit in question, where required.

It will follow directly from the definition that

$$\mathrm{Cy}\left(\frac{\delta\phi}{\delta x}\right) = \frac{\delta}{\delta x}\,\mathrm{Cy}(\phi),$$

so that Cy and $\frac{\delta}{\delta x}\left(\text{or } \frac{\delta}{\delta y}\right)$ are commutative symbols of operation.

5. In what follows the *stresses* and *strains* will be restricted to be *essentially* one-valued, that is, no restriction has to be placed on the functions which represent them in order to make them one-valued over a given region and this quite irrespectively of multiple connection of the space considered.

It is easily shown that the displacements are obtained in terms of the stress function as follows

$$\left.\begin{aligned}2\mu U &= -\frac{\delta E}{\delta x} + (1-\sigma)\frac{\delta\psi}{\delta y}\\ 2\mu V &= -\frac{\delta E}{\delta y} + (1-\sigma)\frac{\delta\psi}{\delta x}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where

$$\nabla^2\psi = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$\frac{\delta^2\psi}{\delta x\delta y} = \nabla^2 E \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and $\sigma = \frac{1}{2}\lambda'(\lambda'+\mu)$, so that $(1-\sigma) = (1+\eta)^{-1}$, where η is Poisson's Ratio.

We note first of all that $\frac{\delta^2 E}{\delta x^2}$, $\frac{\delta^2 E}{\delta y^2}$, $\frac{\delta^2 E}{\delta x\delta y}$ all exist and are one-valued, and that the same holds good of the third differential coefficients. From this, by the theorem of § 4, it follows that $\frac{\delta E}{\delta x}$, $\frac{\delta E}{\delta y}$ are acyclic for a reducible circuit and have a definite cyclic function for an irreducible circuit. By repeated application of the theorem the same result holds good for E.

The cyclic functions for $\frac{\delta E}{\delta x}$ and $\frac{\delta E}{\delta y}$ are very readily found from equations (6).

Taking a circuit enclosing one hole and one only, we have, integrating equations (6) along this circuit

$Cy\left(\frac{\delta E}{\delta x}\right) = -\int Y ds = -$Total force resultant on the circuit parallel to $y =$ Force resultant on boundary of enclosed hole parallel to $y = Y_0$, say.

Similarly $Cy\left(\frac{\delta E}{\delta y}\right) = -X_0$, where $X_0 =$ Force resultant parallel to x acting on the boundary of enclosed hole. This result is due to Michell.

In a similar manner it can be shown that

$$Cy(E) = Y_0 x - X_0 y - M_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

where M_0 is the couple resultant at the origin of the forces acting on the inner boundary of the enclosed hole. The value of Cy(E) at any point is thus the moment about that point of the forces applied to the boundary of the hole about which the typical circuit is taken.

We have next to consider the cyclic functions of $\frac{\delta\psi}{\delta y}$ and $\frac{\delta\psi}{\delta x}$. First of all we note that since $\nabla^2 E$ is essentially one-valued, so is $\frac{\delta^2\psi}{\delta x\delta y}$ and therefore also $\frac{\delta^3\psi}{\delta x^2\delta y}$ and $\frac{\delta^3 \psi}{\delta x\delta y^2}$. And since $\nabla^2\psi = 0 \therefore \frac{\delta^3\psi}{\delta x^3} + \frac{\delta^3\psi}{\delta x\delta y^2} = 0$, so that, $\frac{\delta^3\psi}{\delta x\delta y^2}$ being essentially one-valued, so is $\frac{\delta^3\psi}{\delta x^3}$, and, similarly, so is $\frac{\delta^3\psi}{\delta y^3}$.

Thus the third differential coefficients of ψ are essentially one-valued, and accordingly so will the fourth differential coefficients be. Hence by the theorem of §4 the second differential coefficients have cyclic functions (which involves being acyclic for reducible circuits and being unambiguously defined for reconcilable paths). The first differential coefficients and ψ itself will therefore have cyclic functions for any typical irreducible circuit.

Applying Cy to equations (8) and (9) and remembering that $\nabla^2 E$ is acyclic, we have

$$\nabla^2 Cy(\psi) = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

$$\frac{\delta^2 Cy(\psi)}{\delta x\delta y} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

These two equations require that

$$C_j(\psi) = \tfrac{1}{2}A(x^2-y^2) + Bx + Cy + D \quad . \quad . \quad . \quad . \quad (13)$$

where A, B, C, D are constants. Thus

$$Cy\left(\frac{\delta\psi}{\delta x}\right) = Ax + B$$

$$Cy\left(\frac{\delta\psi}{\delta y}\right) = -Ay + C.$$

6. Referring now to equations (7) and applying the operation Cy, we have

$$\left.\begin{aligned} 2\mu Cy(U) &= -Y_0 + (1-\sigma)[C - Ay] \\ 2\mu Cy(V) &= \quad X_0 + (1-\sigma)[Ax + B] \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (14)$$

Thus Cy(U), Cy(V) are equivalent to a rigid body displacement consisting of a (small) translation of components $(-Y_0 + 1 - \sigma C)/2\mu$ parallel to x and $(X_0 + (1-\sigma)B)/2\mu$ parallel to y and a small rotation $(1-\sigma)A/2\mu$ about the origin.

This is Weingarten's theorem (6). So soon as it is given that U and V have definite cyclic functions the result is obvious. For since U_1, V_1 and U_2V_2 are both solutions of the equations of elasticity, $U_2 - U_1$, $V_2 - V_1$, that is Cy(U) and Cy(V) are also solutions. But the stresses obtained from these must be zero. since the stresses are essentially acyclic. Thus Cy(U) and Cy(V), by a well-known result, are necessarily a rigid body displacement.

We can now introduce Timpe and Volterra's interpretation of Cy(U) and Cy(V). Reduce the multiplicity of connection by drawing a barrier CDE (fig. 11) from the

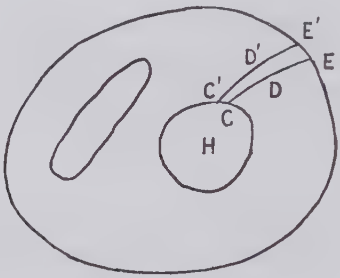

FIG. 11.

boundary of any inner hole H to the outermost boundary of the plate.

This cut need not be straight.

Now give the cut a very small rigid-body displacement so that it takes up the position C′D′E′. Cut the plate so that CDE is one boundary of the gap and C′D′E′ the other. If necessary, wedges of material may have to be cemented on to effect this. Now cement the two boundaries together, so that the plate once more forms a whole. Then, in consequence of this *dislocation*, strain is introduced, corresponding to displacements having for cyclic function with respect to the hole H the rigid body displacement which transformed CDE into C′D′E′. With regard to any other hole the displacements are still acyclic.

In general there will be a cyclic function for each hole, each such function involving three constants, so that if there are n holes, so that the multiplicity of connection of the plate is $n+1$, these are $3n$ cyclic constants. By cutting n channels of appropriate shape and recementing we obtain a plate under internal strain and stress, in which the displacements are affected by the correct cyclic functions. If now external forces are applied to the boundary of the cemented plate, these introduce a strain which involves only acyclic displacements, and so leaves the cyclic functions unaltered.

We thus obtain a representation of the most general type of strain of such a plate as a combination of an external force system with a set of dislocations.

7. Let us now consider in what way the elastic constants are introduced into the solution.

Imagine a plate of the same size and shape as the one considered, but made of some ideal material whose elastic constants have fixed numerical values. Then, if the given forces are applied to this ideal plate, there will necessarily on physical grounds be a solution which will lead to acyclic displacements U_0, V_0. Let E_0 be the value of E and ψ_0 the value of ψ corresponding to this solution.
Then, as before,

$$\left.\begin{aligned} 2\mu_0 U_0 &= -\frac{\delta E_0}{\delta x} + (1-\sigma_0)\frac{\delta\psi_0}{\delta y} \\ 2\mu_0 V_0 &= -\frac{\delta E_0}{\delta y} + (1-\sigma_0)\frac{\delta\psi_0}{\delta x} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (15)$$

u_0, σ_0 referring to the 'ideal' material, so that these are fixed numbers.

Now, if the plate is simply connected, every circuit drawn on it is reducible, hence E_0 and ψ_0 are necessarily acyclic.

If we now consider the same plate, made of any actual material, and write down the displacements

$$\left.\begin{aligned} 2\mu U &= -\frac{\delta E_0}{\delta x} + (1-\sigma)\frac{\delta\psi_0}{\delta y} \\ 2\mu V &= -\frac{\delta E_0}{\delta y} + (1-\sigma)\frac{\delta\psi_0}{\delta x} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (16)$$

These displacements will clearly lead to the same stresses as before—since the stresses depend on E only, and E is here E_0. Also since E_0 and ψ_0 are both acyclic so are U and V. We have therefore the solution required.

Thus, for any material, $E = E_0$, and so is independent of the elastic constants.

We have now to inquire how this is modified when the plate is multiply connected, there being no *dislocations*. This requires that U and V shall be acyclic.

First consider the case where the forces applied to each boundary separately reduce to a couple. This requires (§5) that $\frac{\delta E}{\delta x}$, $\frac{\delta E}{\delta y}$ are acyclic. Then, considering the plate of ideal material

$$(1-\sigma_0)\,\mathrm{Cy}\left(\frac{\delta\psi_0}{\delta y}\right) = 2\mu_0\,\mathrm{Cy}\,(U_0) + \mathrm{Cy}\left(\frac{\delta E_0}{\delta x}\right) = 0,$$

$$(1-\sigma_0)\,\mathrm{Cy}\left(\frac{\delta\psi_0}{\delta x}\right) = 2\mu_0\,\mathrm{Cy}\,(V_0) + \mathrm{Cy}\left(\frac{\delta E_0}{\delta y}\right) = 0.$$

Hence $\frac{\delta\psi_0}{\delta y}$ and $\frac{\delta\psi_0}{\delta x}$ are again acyclic, equations (16), therefore, lead to acyclic values of U and V for the actual plate whatever σ may be, and we can take $E = E_0$ and thus again the stresses are independent of the elastic constants.

If, however, the forces applied to each boundary do not reduce separately to a couple, then $\frac{\delta E_0}{\delta x}$ and $\frac{\delta E_0}{\delta y}$ are cyclic, and in order to make the displacements U_0, V_0 acyclic, it is necessary that

$$\left.\begin{aligned} \mathrm{Cy}\left(\frac{\delta E_0}{\delta x}\right) &= (1-\sigma_0)\,\mathrm{Cy}\left(\frac{\delta\psi_0}{\delta y}\right) \\ \mathrm{Cy}\left(\frac{\delta E_0}{\delta y}\right) &= (1-\sigma_0)\,\mathrm{Cy}\left(\frac{\delta\psi_0}{\delta x}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (17)$$

Hence, for the *actual material*, when $\sigma \neq \sigma_0$

$$\mathrm{Cy}\left(\frac{\delta E_0}{\delta x}\right) \neq (1-\sigma)\mathrm{Cy}\left(\frac{\delta\psi_0}{\delta y}\right),$$

$$\mathrm{Cy}\left(\frac{\delta E_0}{\delta y}\right) \neq (1-\sigma)\,\mathrm{Cy}\left(\frac{\delta\psi_0}{\delta x}\right).$$

Thus we *cannot*, in this case, take $E = E_0$ and $\psi = \psi_0$ and the function E (and therefore also the stresses) must involve the elastic constants.

8. Let us now return to our plate of ideal material and introduce in it a simple dislocation with respect to any given hole corresponding to a unit relative translation of the faces of the cut parallel to x. (The unit, of course, must in this case be a small length, so that the squares of the strains introduced are negligible.) No forces are to be applied to any boundary.

Corresponding to this dislocation of the ideal plate, there will be an internal strain and stress defined by the stress function E_1, and let ψ_1 be the corresponding value of ψ. Owing to the absence of external forces, E_1 and its differential coefficients are acyclic throughout. The corresponding displacements U_1, V_1 are cyclic for a circuit enclosing the hole considered. Also

$$\mathrm{Cy}(U_1) = 1, \qquad \mathrm{Cy}(V_1) = 0.$$

Hence, applying Cy to (15).

$$2\mu_0 = \overline{1-\sigma_0}\,\mathrm{Cy}\left(\frac{\delta\psi_1}{\delta y}\right) \qquad 0 = \overline{1-\sigma_0}\,\mathrm{Cy}\left(\frac{\delta\psi_1}{\delta x}\right) \quad . \quad . \quad . \quad . \quad (18)$$

Similarly if E_2, ψ_2 belong to a simple dislocation with respect to the same hole corresponding to a unit relative translation of the faces of the cut parallel to y

$$0 = \overline{1-\sigma_0}\,\mathrm{Cy}\left(\frac{\delta\psi_2}{\delta y}\right) \qquad 2\mu_0 = \overline{1-\sigma_0}\,\mathrm{Cy}\left(\frac{\delta\psi_2}{\delta x}\right) \quad . \quad . \quad . \quad . \quad (19)$$

In like manner, unit translational dislocations with reference to the other holes will lead to stress functions $E_3, E_4, \ldots E_{2n-1}, E_{2n}$ and corresponding ψ functions $\psi_3, \psi_4, \ldots \psi_{2n-1}, \psi_{2n}$.

Build up a solution

$$E = E_0 + \alpha_1 E_1 + \alpha_2 E_2 + \alpha_3 E_3 + \alpha_4 E_4 + \ \ldots \ + \alpha_{2n-1} E_{2n-1} + \alpha_{2n} E_{2n}\,;$$

$$\therefore \psi = \psi_0 + \alpha_1\psi_1 + \alpha_2\psi_2 + \alpha_3\psi_3 + \alpha_4\psi_4 + \ \ldots \ + \alpha_{2n-1}\psi_{2n-1} + \alpha_{2n}\psi_{2n},$$

where $\alpha_1 \ldots \alpha_{2n}$ are constants to be determined.

If this solution is to lead to acyclic values for the displacements U, V, we must have, for *every* irreducible circuit

$$\left.\begin{aligned}\mathrm{Cy}\left(\frac{\delta E}{\delta x}\right) &= (1-\sigma)\,\mathrm{Cy}\left(\frac{\delta\psi}{\delta y}\right)\\ \mathrm{Cy}\left(\frac{\delta E}{\delta y}\right) &= (1-\sigma)\,\mathrm{Cy}\left(\frac{\delta\psi}{\delta x}\right)\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

If we take Cy to refer to an irreducible circuit enclosing the hole to which solutions 1 and 2 refer, but none of the others, then $\mathrm{Cy}(E_1) = \mathrm{Cy}(E_2) = \mathrm{Cy}(E_3) = \mathrm{Cy}(E_4) = \ldots = \mathrm{Cy}(E_{2n} = 0$, and also, since the displacements U_3, V_3, say, are acyclic except for a circuit enclosing that hole to which solutions 3 and 4 refer,

$$\mathrm{Cy}\left(\frac{\delta\psi_3}{\delta y}\right) = \mathrm{Cy}\left(\frac{\delta\psi_3}{\delta x}\right) = \ \ldots \ = 0.$$

Equations (20) then become, on substitution,

$$\mathrm{Cy}\left(\frac{\delta E_0}{\delta x}\right) = (1-\sigma)\,\mathrm{Cy}\left(\frac{\delta\psi_0}{\delta y}\right) + \alpha_1(1-\sigma)\,\mathrm{Cy}\left(\frac{\delta\psi_1}{\delta y}\right) + \alpha_2(1-\sigma)\,\mathrm{Cy}\left(\frac{\delta\psi_2}{\delta y}\right)$$

$$\mathrm{Cy}\left(\frac{\delta E_0}{\delta y}\right) = (1-\sigma)\,\mathrm{Cy}\left(\frac{\delta\psi_0}{\delta x}\right) + \alpha_1(1-\sigma)\,\mathrm{Cy}\left(\frac{\delta\psi_1}{\delta x}\right) + \alpha_2(1-\sigma)\,\mathrm{Cy}\left(\frac{\delta\psi_2}{\delta x}\right)$$

whence, using (18) and (19),

$$\left.\begin{aligned}\frac{1}{1-\sigma}\,\mathrm{Cy}\left(\frac{\delta E_0}{\delta x}\right) - \mathrm{Cy}\left(\frac{\delta\psi_0}{\delta y}\right) &= \frac{2\mu_0\alpha_1}{1-\sigma_0}\\ \frac{1}{1-\sigma}\,\mathrm{Cy}\left(\frac{\delta E_0}{\delta y}\right) - \mathrm{Cy}\left(\frac{\delta\psi_0}{\delta x}\right) &= \frac{2\mu_0\alpha_2}{1-\sigma}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (21)$$

Since the cyclic functions on the left-hand side are known to reduce to mere constants, equations (21) determine α_1 and α_2.

Similarly, taking irreducible circuits about the other holes, we obtain α_3, α_4, etc., and we have finally an acyclic solution which holds good for the actual plate, but

this solution is at once seen to involve the ratio of the elastic constants, since σ enters into the values of α_1, α_2 determined by equations (21).

These values can be simply expressed in terms of the force resultants X_0, Y_0 applied to the corresponding hole. For using (17) we have

$$\left.\begin{aligned} 2\mu_0\alpha_1 &= \left(\frac{1-\sigma_0}{1-\sigma}-1\right) \mathrm{Cy}\left(\frac{\delta E_0}{\delta x}\right) = \frac{\eta-\eta_0}{1+\eta_0} Y_0 \\ 2\mu_0\alpha_2 &= \left(\frac{1-\sigma_0}{1-\sigma}-1\right) \mathrm{Cy}\left(\frac{\delta E_0}{\delta y}\right) = -\frac{\eta-\eta_0}{1+\eta_0} X_0 \end{aligned}\right\} \quad . \quad . \quad (22)$$

9. These results are going to enable us, not only to obtain an upper limit to the divergences in the stresses in two circular rings of different materials under the same applied forces, owing to the divergence in the ratio of the elastic constants, but, what is even more important to correct definitely results obtained on a multiply-connected xylonite model and make them applicable to a material like steel, even when the mathematical solution cannot be attained.

Let us take this latter application first.

Suppose that our 'ideal' material is now taken to be xylonite, and that in a plate of xylonite the stresses P, Q, S have been measured by the optical and transverse contraction methods described by Professor Coker [(7) (8) and (9)], so that we may consider P_0, Q_0, S_0 as known (the suffixes having the same meaning as before). Suppose further that by a separate experiment with a xylonite model suitably cut and re-cemented, the stresses P_1, Q_1, S_1 ; P_2, Q_2, S_2 ; etc., corresponding to specified unit dislocations, have been similarly explored. As the only dislocations considered are translational, the experiment is very easily carried out in the majority of cases by cutting a straight channel perpendicular to the direction of translation required and bringing the edges of the cut together.

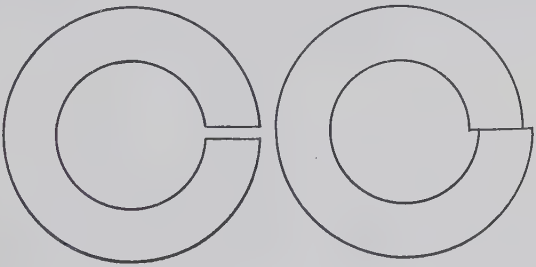

FIG. 12.

Now let the 'actual' material be any ordinary engineering material, *e.g.* steel, then the stresses in this material, due to the same force system already applied to the celluloid, are given by

$$\left.\begin{aligned} P &= P_0 + \alpha_1 P_1 + \alpha_2 P_2 + \ \ldots \\ Q &= Q_0 + \alpha_1 Q_1 + \alpha_2 Q_2 + \ \ldots \\ S &= S_0 + \alpha_1 S_1 + \alpha_2 S_2 + \ \ldots \end{aligned}\right\} \quad . \quad . \quad . \quad (23)$$

where α_1, α_2, α_3 . . . are given by equations (22) and similar equations.

Now the total force resultants (per unit thickness) applied to the various boundaries are, in most problems to which this method can be applied, directly known, and the values of η, η_0, μ_0 for the materials concerned can be found without difficulty by direct experiment when they are not already known. Thus the α's are calculated without difficulty and the stresses in the steel plate at all points can be deduced. Accordingly the exploration of a xylonite model can, even in this case, be made to give complete information about the stresses in a plate of any material, even though the mathematical solution is beyond our present powers of analysis.

10. We will now apply this method to find the magnitude of the corrections which have to be applied to the stress-system observed in a xylonite ring, the radii of the inner and outer boundaries being a and b. In order to do so it is necessary first of all to work out the stresses for the two elementary translational dislocations represented in fig. 12.

We can further simplify by taking the axis of y parallel to the force resultant on the inner boundary. We have then $X_0 = 0$, and therefore $\alpha_2 = 0$, so that only the dislocation due to translation parallel to x need be considered (fig. 12, right-hand).

The stress function for this dislocation has been given by Timpe (*loc. cit.*, p. 31). It can also be deduced from Volterra's general results. In this case

$$E_1 = K \sin\theta \left[\frac{a^2b^2}{2r} - \frac{r^3}{2} + (a^2+b^2) r \log r\right]$$

leading to the following stresses, r, θ being polar co-ordinates

$$\widehat{rr}_1 = \frac{K \sin\theta}{r^3}(b^2-r^2)(r^2-a^2),$$

$$\widehat{r\theta}_1 = -\frac{K\cos\theta}{r^3}(b^2-r^2)(r^2-a^2),$$

$$\widehat{\theta\theta}_1 = \frac{K\sin\theta}{r^3}\{a^2b^2 - 3r^4 + r^2(a^2+b^2)\}.$$

We find also

$$\psi_1 = 2K\{\tfrac{1}{3}x^3 - xy^2 + (a^2+b^2)(x\log r - y\theta - x)\},$$

whence

$$Cy(\psi_1) = -4\pi K(a^2+b^2)y,$$

and therefore

$$Cy\left(\frac{\delta\psi_1}{\delta y}\right) = -4\pi K(a^2+b^2),$$

so that equation (18) gives

$$2\mu_0/(1-\sigma_0) = -4\pi K(a^2+b^2),$$

or

$$K = -\frac{2\mu_0(1+\eta_0)}{4\pi(a^2+b^2)},$$

and, using (22)

$$\alpha_1 K = -\frac{(\eta-\eta_0)Y_0}{4\pi(a^2+b^2)}.$$

The *corrective terms* to the stresses, which we may call for shortness $\Delta\widehat{rr}$, $\Delta\widehat{r\theta}$, $\Delta\widehat{\theta\theta}$, are then

$$\left.\begin{aligned}\Delta\widehat{rr} = \alpha_1\widehat{rr}_1 &= -\frac{(\eta-\eta_0)Y_0}{4\pi(a^2+b^2)}\frac{(b^2-r^2)(r^2-a^2)}{r^3}\sin\theta\\ \Delta\widehat{r\theta} = \alpha_1\widehat{r\theta}_1 &= +\frac{(\eta-\eta_0)Y_0}{4\pi(a^2+b^2)}\frac{(b^2-r^2)(r^2-a^2)}{r^3}\cos\theta\\ \Delta\widehat{\theta\theta} = \alpha_1\widehat{\theta\theta}_1 &= -\frac{(\eta-\eta_0)Y_0}{4\pi(a^2+b^2)}\left\{\frac{a^2b^2-3r^4+r^2(a^2+b)^2}{r^3}\right\}\sin\theta\end{aligned}\right\} \quad . \qquad (24)$$

Note carefully that these corrective terms depend only upon the total force resultants applied to the hole and *not in any way upon the manner in which these force resultants are distributed*; and this remark holds good, not merely for the circular ring, but for the general case of a plate of any shape.

11. We have now to consider where these corrective stresses are greatest.

It is clear from equations (24) that for each stress the maximum occurs for the same value of r on each radius vector. These maxima will themselves reach their greatest values: (1) in the case of $\Delta\widehat{rr}$ and $\Delta\widehat{\theta\theta}$ when $\theta = \pm\pi/2$, that is along the diameter parallel to the resultant applied force; (2) in the case of $\Delta\widehat{r\theta}$ when $\theta = 0$ or π, that is along the diameter perpendicular to the resultant applied force.

Consider now the maximum corrective stress $\Delta\widehat{\theta\theta}$. It will occur when $\phi = a^2b^2/r^3 - 3r + (a^2+b^2)/r$ has its greatest numerical value. Now $d\phi/dr = -3a^2b^2/r^4 - 3 - (a^2+b^2)/r^2$

and is essentially negative. Thus the greatest *algebraic* value of ϕ occurs at the inner radius $r=a$ when $\phi=2(b^2-a^2)/a$ and the least algebraic value at the outer radius $r=b$ where $\phi=(a^2-b^2)/b$. We thus see that ϕ changes sign between the two and that the greatest *numerical* value is at $r=a$, leading to:

$$\Delta\widehat{\theta\theta}_{\text{max.}}=(\eta-\eta_0)\frac{Y_0}{2\pi a}\left(\frac{b^2-a^2}{b^2+a^2}\right),$$

from which it appears at once that this corrective stress becomes small if b/a is near unity, that is, if the thickness of the ring is small in comparison with its radius. If we take $\eta_0=0.2$, $\eta=0.3$ and b/a as large as 1.25, we get $\Delta\widehat{\theta\theta}_{\text{max.}}=(0.022)Y_0/2\pi a$, *i e.* about 2% of a stress equal to the total load distributed uniformly over the internal circumference. This last stress will in general itself be small compared with the bigger existing stresses, so that, in this case, the correction, at any rate near critical or dangerous points, will be entirely negligible.

Coming now to $\Delta\widehat{rr}$ and $\Delta\widehat{r\theta}$, we have to make $\phi=(b^2-r^2)(r^2-a^2)/r^3$ a maximum. Here we have clearly no change of sign inside the ring and a positive maximum occurs between $r=a$ and $r=b$.

The value of r corresponding to this maximum is readily found from:

$$r^2=\frac{\sqrt{(a^2+b^2)^2+12a^2b^2}-a^2-b^2}{2}$$

This, however, leads to a somewhat awkward algebraic expression for the maximum stress corrections, involving lengthy radicals, and since we are really chiefly concerned with finding an upper limit for the stress corrections, we notice that we necessarily increase ϕ, if in the denominator we replace r^3 by its least possible value a^3. The greatest value of (b^2-r^2) (r^2-a^2) is then well known to occur when $r^2=(a^2+b^2)/2$, so that the maximum value of ϕ is certainly less than $\frac{1}{4}\frac{(b^2-a^2)^2}{a^3}$, leading to

$$\Delta\widehat{rr}_{\text{max.}}=\Delta\widehat{r\theta}_{\text{max.}}<(\eta-\eta_0)\frac{Y_0}{2\pi a}\frac{(b^2-a^2)^2}{8a^2\ (a^2+b^2)}$$

Since this contains the square of b^2-a^2 it leads in general to an even smaller correction than for the stress $\widehat{\theta\theta}$. Taking the same values of η, η_0 and b/a as before, we find

$$\Delta\widehat{rr}_{\text{max.}}=\Delta r\theta_{\text{max.}}<(0{\cdot}00154)Y_0/2\pi a$$

which will usually be entirely negligible.

The investigation of the case of the circular ring therefore indicates that the correction due to variation in the ratio of the elastic constants is usually very small. The more general theorems previously obtained show how, even when this correction cannot be neglected, or computed mathematically, it can nevertheless be allowed for if a suitable experimental exploration is undertaken. This justifies the use of xylonite models for the exploration of stress, even when such models are multiply connected.

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IV.

Stress Concentrations in Theory and Practice.

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1. Introduction.

It has long been realised, as a result both of experience and theoretical considerations, that sharp re-entrant corners, rapid changes of section and the like, are, in general, undesirable in members subjected to considerable stresses, by reason of the local weakening which they cause. At the same time, very few attempts have been made to take account of these stress concentrations in design, partly because of the inherent difficulty of the subject, but also very largely because calculations based on the mathematical theory of elasticity have in some cases appeared to overrate greatly the weakening effect which is produced.

During recent years, however, the greater stringency of the working conditions imposed on machines and structures has so largely increased the number of failures arising from stress concentration as to render imperative a better understanding of the mechanism of such failures, and particularly of the causes which limit the validity of the mathematical methods of attack.

In the present paper it is proposed to review briefly the existing state of knowledge of this subject, and to discuss some of the problems which still await solution.

2. Methods of Estimating Stresses.

The following methods are at present available for estimating the magnitude of stress concentrations :—

I. Direct Mathematical Calculation.
II. The Photo-elastic Method.
III. The Soap-film Method.
IV. The Thermal Method.

So far as stress concentrations are concerned, the most important numerical results which have been obtained by method I. are those relating to the stresses in a plate containing an elliptic hole of any eccentricity (1) (and, approximately, those in a plate from whose edge springs a small semi-elliptic crack or groove); and in some cases the stresses in bent and twisted cylindrical shafts, the boundaries of whose cross-sections include re-entrant portions. These results may further be extended to find approximately the stress concentrations due to small semi-elliptic holes or cracks, and also scratches or grooves, formed in the surface of members which are not cylindrical, but in which the general stress distribution has been found by other methods.

In a further application, the general two-dimensional solution in elliptic coordinates (1) may be employed to find the stresses in a flat plate having the form of a hyperbolic cylinder of any eccentricity, when it is subjected to a tensile load directed along the imaginary axis of the hyperbola.

Method II. (2) may be used to find directly the stresses in a flat plate of any shape, to which given edge tractions are applied. As with the previous method, concentrations due to small cylindrical holes or cracks, cut normally into the surface of a member, may be found, but in this case there is no restriction on the shape of the hole. Further, the effect of cutting any scratch or groove, in a surface along which the principal stresses are parallel and perpendicular to the direction of the scratch or groove, may be determined.

There is another possible application of this method which, so far as the author is aware, has not yet been used. If a thin plate, initially flat, be bent by couples applied at its edge, the equations satisfied by the principal curvatures of its surface are mathematically identical with those satisfied by the principal stresses in a plate, of the same shape, subjected to appropriate edge tractions. Hence, if a photo-elastic experiment be performed in which the applied edge-tractions represent, on some convenient scale, the prescribed conditions of curvature at the boundary of the bent plate, then the measured stresses at any point in the stretched plate will represent, on the same scale, the curvatures at the corresponding point of the bent plate. Hence the stresses in the latter may be found.

Turning now to III. (3), it is possible by this method to find the shearing stresses in a bent or twisted cylindrical beam or shaft, having a cross-section of any given shape. The weakening effect of any small groove, formed in the surface of a member which is not cylindrical, may be found in those cases where the stress in the neighbourhood is a shearing stress acting on planes respectively parallel and perpendicular to the groove.

The soap-film solutions for small surface grooves are complementary to those obtained by the photo-elastic method, so that a combined method may be used to find the effect of such a groove in any case whatever.

The thermal method depends on the fact that there is a sudden generation of heat in soft metals such as mild steel when the stress reaches the yield point. The resulting rise of temperature may be detected by means of a thermo-couple applied to the surface of the material. It follows that advantage may be taken of this phenomenon to determine the magnitude of stress concentrations, and that, unlike the other available methods, there is no restriction to two-dimensional cases. So far, however, very few attempts have been made to develop this method.

In this branch of the subject progress is most urgently needed in the estimation of stresses in solids of revolution. On the purely mathematical side the most promising line of attack seems to be the investigation of solutions applicable to ellipsoids and hyperboloids of revolution, and it may be regarded as probable that an important advance in this direction will shortly be made. As regards solids of revolution in general, what is required is some method of the type of the soap-film and photo-elastic methods.

3. Examples of Stress Concentrations.

3. *Examples of Stress Concentrations.*—It is not proposed to attempt here anything like a comprehensive summary of the detailed work which has been performed by the foregoing methods. Nevertheless, it is desirable for the purposes of the present paper to illustrate by means of examples the general nature of the results obtained.

In what follows it will be convenient to use the term 'concentration factor' to denote the ratio of the true calculated stress to the stress which would be calculated if the concentration of stress were to be neglected.

A very simple example is that of a small round hole, such as an oil hole, drilled normally into the surface of, say, a twisted or bent circular shaft. In the twisted shaft the tensile stress concentration factor is 4, while the shear stress factor is 2. In the bent shaft the tensile and shear factors are both 3. These results are practically independent of the ratio of the size of the hole to the size of the shaft, unless the former is so large as to reduce materially the cross-section of the shaft.

Similar concentration factors are found in cases where holes are drilled in members of other shapes.

As another illustration we may take a small groove of semi-elliptic cross-section, cut in the surface of a twisted shaft. If ρ is the radius of curvature at the bottom of the groove and a is the depth, the shear stress concentration factor is

$$1+\sqrt{\frac{a}{\rho}}$$

This result is true whatever the angle between the direction of the groove and the axis of the shaft. The tensile stress factor, on the other hand, varies from

$$1+\sqrt{\frac{a}{\rho}}$$

if the groove is parallel or perpendicular to the axis, to

$$1+2\sqrt{\frac{a}{\rho}}$$

if the groove runs round the shaft in the form of a 45° spiral.

It may be remarked here that work on grooves of other shapes has shown that the stress concentration depends mainly on the depth and the radius of curvature at the bottom, provided the groove is not very shallow. For instance, a 60° Vee groove with a rounded corner gave factors only a few per cent. less than those calculated from the foregoing formulæ. It appears, therefore, that the latter may be used even when the groove departs considerably from the elliptic form.

It will be seen from the above results that stress concentrations may theoretically be quite large in cases likely to occur in practice. Thus, if the depth of a groove is sixteen times the radius of curvature, the concentration factor may vary from 5 to 9, while in the extreme case of a surface crack even these values may be greatly exceeded.

The theoretical stresses are lower in the case of a continuously grooved surface such as a screwed portion than in an isolated groove. For example, in a particular screwed rod the tensile stress concentration factor was estimated to be 3.7, while for an isolated groove of the same shape the factor was found to be 4.9. Possibly this fact partly explains why it is found to be of value to turn down the plain portion of a screwed bolt to the root diameter; if this is not done the stress concentration in the end groove is considerably greater than in the rest of the screwed part.

As in the case of the circular holes, the above results are substantially independent of the size of the grooves unless these are so large that a material amount of metal is removed in their formation.

A striking consequence of this fact is that severe stress concentrations should arise from the scratches and other surface defects left on machine parts by the ordinary processes of manufacture. As will be seen later, the non-fulfilment of this prediction is one of the outstanding discrepancies between the theoretical and experimental aspects of our subject.

4. Practical Limitations of the Foregoing Methods.

In practice it is not infrequently found that the application of the foregoing results, on the basis of the usual criteria of failure, gives a value for the weakening due to stress concentration which is misleading or entirely wrong. In other cases the agreement is quite good enough for practical requirements. It is

therefore important to find out in what way the assumptions made in applying the theory are defective.

Methods I. and III., above, are subject to the usual assumptions of the mathematical theory of elasticity, namely, that the material is homogeneous and isotropic (or possesses some particular kind of æolotropy), that the stresses are proportional to the strains, and that the strains are so small that their squares may be neglected. As regards II., it has been shown that this method measures the stresses actually existing in the xylonite plates which are usually employed in the experimental work. Its use is therefore subject only to the assumption that the stress-strain relations of the material considered are of the same form as those of xylonite.

In the first place, it is to be noted that method I. gives results which are in substantial agreement with those of method II. in those cases where a comparison is possible. We may therefore conclude that the assumption of linear stress-strain relations involves no important error provided the departure from linearity is of the same order as that of xylonite under the conditions of photo-elastic tests. Similarly we note that the assumption of infinitesimal strains is not material if the strains are comparable with those in xylonite under the above-mentioned conditions. Since xylonite exhibits, in these two respects, noticeable departures from the ideal assumptions, within the ranges of stress common in photo-elastic tests, it may be concluded that the effects of these assumptions are of little importance in practice except where relatively large strains occur, as in cleavage slipping and viscous flow.

Further consideration of the causes of departure from theory may most conveniently be made with reference to the type of fracture which occurs. For this purpose fractures may be classified as elastic or brittle, if rupture occurs without material inelastic deformation; plastic, if cleavage slipping occurs; viscous, if the type of fracture is mainly or largely determined by viscous flow; and fatigue fractures. The viscous type will not be further discussed here.

In the case of a member subjected to a steady load such that the fracture, when it occurs, is of the plastic type, it has long been known that stress concentrations have little or no weakening effect. The reason for this is also well known, namely, that an amount of plastic flow, at the point of high stress, which is small compared with the amount of flow at fracture is adequate to annul the stress concentration. In some experiments described in a recent paper (4), the author showed that this flow occurs at approximately the estimated load even in the case of extremely small surface scratches having a depth of the order of 10^{-4} inch.

The two practical cases in which weakening due to stress concentration is of real importance are those of elastic fracture under a load steadily applied, and fatigue failure under a periodically varying load.

Much work has been done during the past few years which bears, directly or indirectly, on the question of stress concentrations in these two cases, and it may now be said with considerable confidence that one reason for the frequent discrepancy between the estimated concentration factor and the observed weakening is, in both instances, the existence of a scale effect. The general result is that stress concentrations are practically unimportant if the linear dimensions of the region of high stress are sufficiently small, no matter what the estimated value of the concentration factor may be, provided that it does not exceed a certain high upper limit. As we have seen, this scale effect is not predicted by the purely theoretical methods of attack.

As regards elastic fractures, the scale effect appears, so far as existing knowledge goes, to be the only important cause of observed departures from theory. In the case of fatigue fractures, on the other hand, it may be regarded as probable that redistribution of stress due to cleavage slipping often occurs under ranges of stress within the fatigue limits of the material. Under these circumstances an additional factor is introduced into the problem.

The existing evidence on the above points is discussed in the three succeeding sections.

5. Scale Effect: (a) Elastic Fractures.

With certain exceptions, combined stress tests which result in elastic fractures, and which do not involve stress concentrations, may be regarded as satisfying the law that rupture occurs when the greatest (positive) tensile stress reaches a particular value. The exceptional cases are those in which the greatest principal compression is numerically much larger than the greatest principal tension.

Confining ourselves in the present discussion to cases where the greatest compression is not predominant, it appears that we may reasonably infer that the weakening effect of, say, a scratch should be obtainable at once from the tensile stress concentration factor. It is found, however, that this prediction is by no means verified if the scratches are small.

A familiar illustration is met with in the operation of glass-cutting, where local weakening is produced by means of a scratch, the object being to determine the direction of fracture when the glass is subsequently broken. It is found that the weakening effect is insufficient unless the depth of the scratch exceeds a certain value, notwithstanding the fact that the estimated concentration factor due to the shallower scratches would appear to be quite adequate for the purpose in view.

As an example of the same phenomenon in elastic fractures of crystalline metals, some experiments on hardened cast-steel may be cited.

The theory states that if surface scratches be made on a tensile test piece there should be no stress concentration in the case of scratches whose direction is parallel to that of the tensile stress, while if they are perpendicular to the direction of the stress the concentration factor should be a maximum for the particular shape of scratch. Experiments were therefore undertaken,[1] at the author's request, to find the effect of the orientation of the surface scratches on the tensile breaking load of dead hard cast-steel test-pieces. The scratches were made with No. 0 carborundum cloth, and their depth was of the order of 10^{-4} inch. The shapes of scratches made by this means were examined micrographically, and it was deduced that the concentration factor due to the perpendicular (circumferential) scratches should be 3 to 4 at least. In the tensile tests, however, there was no systematic difference between the breaking loads of the axially and circumferentially scratched specimens. Here, then, is a case of a severe stress concentration which appears to have been completely annulled by reason of the scale effect.

In another experiment, conducted to determine the effect of larger grooves on the same material, two strips 0.025 inch thick and 0.5 inch wide were broken by bending. One of the strips had a serrated edge, the depth of the serrations being 0.025 inch. It was found that the breaking couple of the plain strip was 2.48 times that of the serrated one. The concentration factor due to the serrations was estimated to be 2.9, so that in this case the greater part of the theoretical weakening was developed.

A theory of scale effect in relation to elastic fractures was advanced by the author in the paper already mentioned (4). It was shown that, on the usual assumptions of elastic theory, the accepted strengths of materials, under tests which result in elastic fracture, are incompatible with strengths which are deducible on theoretical grounds from other physical properties of the materials, the theoretical strengths being by far the greater. A detailed investigation performed on a certain kind of glass indicated that this discrepancy could only be explained by supposing that the material contained minute flaws which gave rise to very severe stress concentrations. It was found that in this glass the necessary order of magnitude of the flaws was about 10^{-4} inch. In the case of vitreous materials a process was discovered whereby these flaws could be temporarily eliminated, and the substances then possessed tensile strengths agreeing approximately with the theoretical values. Thus for the particular kind of glass which was chiefly used the tensile strength was normally about 25,000 lb. per sq. in. After treatment values as high as 900,000 lb. per sq. in. were recorded, so that the concentration factor due to the flaws was about 36.

These results suggest immediately an explanation of the scale effect. If a

[1] By Mr. W. D. Douglas, of the Royal Aircraft Establishment.

small surface scratch be formed whose depth is of the same order as the dimensions of the flaws already existing, but which gives rise to a less severe stress concentration, the strength of the piece must clearly be unaffected, since whether the scratch be present or not, rupture must be occasioned by the stress concentration due to the flaws. If, however, a groove be made, of such a size that the region of high stress is large compared with the size of the flaws, there must be a double magnification of stress, arising firstly from the concentration of stress at the bottom of the groove, and secondly from the flaws within that region of high stress.

Since the above-mentioned paper was published, it has been found possible to prepare pure vitreous silica in a stable form in which a relatively large proportion (at least one-third) of the theoretical tenacity is retained. According to the theory, this implies that the flaws must be reduced almost to molecular dimensions, whence it follows that the scale effect should be practically non-existent.

This conclusion has received ample support from a number of experiments, the results of which are sufficiently illustrated below.

Two thin circular rods of the material were touched together as lightly as possible, whereby a minute injury, invisible under a magnification of 250 diameters, was inflicted on the surface of each. One of the rods was broken by flexure, with the abrasion on the tension side. The degree of flexure at rupture was only about one-eighth of that required to break an uninjured rod of the same diameter. The other rod was broken with the injury on the compression side, and in this case no weakening could be detected. This is in agreement with the hypothesis of rupture under a specific tensile stress, since no concentration of tensile stress should arise from a surface defect on the compression side of a bent beam. Further experiments were performed in which rods were lightly touched with other solid bodies, both hard and soft, and with the finger tip. In all cases a notable weakening resulted. Even if a rod was left exposed to bombardment by the dust particles in the atmosphere there was a slow but perfectly definite weakening.

In the case of brittle substances, then, it appears that stress concentrations due to minute flaws and surface defects, far from being negligible, constitute the controlling factor which determines the magnitude of the technically available tenacity of these materials, a result which may be regarded as a remarkable vindication of the elastic theory of stress concentrations.

6. Scale Effect: (b) Fatigue Fractures.

The general nature of available experimental results in this branch of the subject is the same as in the case of elastic fractures, that is to say, fine scratches appear to have little or no effect, while sufficiently large grooves often give rise to nearly the full theoretical weakening. The evidence is, however, less complete, and much more work is required to place our knowledge of scale effect in fatigue phenomena on a satisfactory basis.

On the theoretical side the main difficulty arises from the lack of a well-established theory of fatigue phenomena in general. Beilby's original work (5) on the production of an amorphous phase in metals by overstrain showed that large internal stresses might be set up as a result of cleavage slipping, by reason of the difference in density between the amorphous and crystalline phases. This result suggested as a possible theory of fatigue that the internal stresses set up by repeated cleavage slipping, when combined with the stress system externally applied, might ultimately be adequate to initiate a brittle fracture of the material. Beilby considered that the amorphous phase was developed between the slipped surfaces, so that on the theory derived from his work fatigue fracture should always be an ultimate effect of repeated slipping. A similar conclusion may be reached regarding the theory of Ewing and Humfrey (6), according to which fatigue cracks are initiated by the attrition of the slipping cleavage surfaces.

A difficulty in connection with both these theories is that, as the present author has shown (see §4 above) cleavage slipping can occur at the corners of very small scratches at loads very much below those necessary to cause it to take place in the remainder of the material. According to the foregoing theories, therefore, the presence of such scratches on a test-piece should greatly reduce

its fatigue strength, a conclusion which is not borne out by the results of tests.

Another view was put forward by the author (4), according to which it was considered that the slipped surfaces suffered no permanent change, and that the amorphous phase was generated entirely at the inter-crystalline boundaries. On this theory, no phase change, and therefore no fatigue cracking, can result from cleavage slipping within a crystal unless the slipping extends at least as far as the junction with neighbouring crystals. Hence this theory, unlike the other two, does predict an effect similar to the observed scale effect, and to that extent is more satisfactory. It does not seem likely, however, that the author's theory will prove entirely adequate in its present form, as many new and so far unexplained facts have come to light since the publication of the paper in which it was first advanced. For instance, some data now available suggest on the one hand that fatigue fractures can sometimes occur without cleavage slipping, and on the other that repeated cleavage slipping in certain materials does not necessarily lead to fatigue failure even though it extend over a number of crystals.

On the experimental side the position of this branch of our subject appears to be even less satisfactory than on the theoretical side. So far as the author is aware, very few published results exist which are sufficiently complete for a close comparison with theory; essential data, such as the shape of the scratches or the size of the crystals, being omitted.

7. Fatigue Fractures: Effect of Stress Redistribution.

It may be regarded as proved that, in many metals, no important plastic flow occurs, within the fatigue limits of stress, under an applied load alternating between numerically equal positive and negative values. Under these conditions the only uncertainty in applying the theory of stress concentrations is that due to the scale effect.

The case is otherwise, however, in the technically commoner examples of fatigue failure in which the alternating load is superposed on a relatively large steady load. Fatigue under such loads is, in general, preceded by plastic flow, and in dealing with failure at a point of local high stress, such flow involves a redistribution of stress in the neighbourhood. The nature of this redistribution cannot at present be found by calculation. It follows that while the range of stress at the fail point is calculable the mean stress is not; there is therefore no basis on which the ordinary data regarding the fatigue strength of the material can be employed to find the conditions of rupture at a point of stress concentration under such kinds of load.

In the case of some metals, some recent experiments suggest that plastic flow may be met with even under a pure alternating stress.

It may be remarked that the effect discussed above, though a serious obstacle to theoretical work, is probably of great value in practice, since the redistribution of stress must decrease the mean stress, and therefore the weakening effect of the concentration.

An additional complication accrues from the fact that if appreciable plastic flow occurs at the fail point the material there must be in a cold-worked condition, whence increased resistance to fatigue is to be anticipated.

8. Stress Concentration in Mechanical Testing.

It is customary, in the usual mechanical tests, including fatigue tests, to make the working portion of the test-piece of smaller section than the portions which are gripped, in order to localise the fracture and other effects of the test. The junction of the two parts is rounded off so as to secure a gradual change of section. Now, at such a change of section there must, in general, be a stress concentration, so that the actual maximum stress must be greater than the estimated working stress in the specimen; moreover, it is not usually possible at present to calculate the magnitude of the concentration factor. It appears, then, that if the difference between the estimated stress and the true maximum stress is appreciable the ostensible results of the tests may be materially erroneous, especially in the case of fatigue tests. It is evidently necessary in any important test work to reduce this unknown factor to negligible dimensions. Partly on theoretical grounds and partly as the result of experience with test

pieces of various forms, the author has reached the conclusion that this object may be attained in most cases by making the radius of the fillet at the change of section at least five times the working diameter (or corresponding dimension) of the test-piece.

The only other type of stress concentration which is likely to modify the results of mechanical tests is that due to surface scratches. As has been seen, advantage may be taken of the scale effect to render this factor unimportant, by making the scratches sufficiently fine. Scratches having a depth of the order of 10^{-4} inch, such as are produced by No. 0 emery cloth, or by grinding with a wheel of about No. 80 grit, would appear to be satisfactory in all ordinary cases.

9. Allowance for Stress Concentrations in Design.

The author has often been asked by designers what measures can be taken to guard against the occurrence of dangerous stress concentrations in practice. It is clear from what has already been said that nothing like a complete answer to this question is possible at present. All that can be done is to lay down approximate rules based not only on calculable cases and such experimental facts as have so far been collected, but also very largely on personal judgment. The subjoined recommendations, which embody advice which the author has given from time to time in the past, may, perhaps, be found useful pending the development of more rational methods. It may be remarked that they have not infrequently been successful in overcoming difficulties encountered in practice. They are intended to apply mainly to members possessing some ductility and subjected to periodically varying loads.

In the first place, the radius of the fillet in a re-entrant corner should be at least a quarter of a certain dimension which may be called the ruling dimension. For grooves, such as screw-threads and keyways, the ruling dimension is the depth of the groove. Alternatively, in a continuously grooved surface, such as occurs in a gear wheel, the root thickness of the teeth or ridges may be used if this is less than the depth. For sudden bends, as in crankshafts, the ruling dimension may be taken to be the radius of the shaft (or the corresponding dimension). At a change of section on a shaft, the smaller radius or the difference between the radii, whichever is the less, may be taken.

The above values of the radius of fillet should be regarded as minima and should be exceeded wherever possible.

In addition, an allowance should be made for the weakening effect of the stress concentration. It is impossible at present to give any very definite idea of the magnitude of this factor, as it appears to vary so greatly with the nature of the material and the kind of load. With the worst materials and pure alternating loads it may be necessary to allow the full theoretical concentration factor, which with the above values of the radius of fillet will usually be about 2 to 6. In more favourable cases, *e.g.*, where the load fluctuates but does not change sign, much lower values may be used. Probably a factor of 2 will be found satisfactory in the majority of such cases.

As has been seen above, plastic flow can be of great value in reducing the effect of local stress intensifications. Hence two qualities are desirable in the material of a member liable to break at a point of stress concentration. In the first place, the fatigue limits should be as wide as possible, and in the second the range of stress within which plastic flow is absent should be as small as possible.

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V.

The Strain Energy-Function and the Elastic Limit.

By Professor B. P. Haigh, *D.Sc., M.B.E., Royal Naval College, Greenwich.*

In a contribution to the report of this Committee, for 1919, the author analysed published data, with the object of comparing, for ductile metals, the different elastic limits under simple and complex stresses. Experimental values of the ratios between the elastic limits for different kinds of stress were compared with the corresponding values predicted by applying three alternative hypotheses, due to Lamé and Rankine, to Darwin, Tresca and Guest, and to Saint Venant; and with values deduced from a noval hypothesis, viz. that the relation between the elastic limits is governed by the strain energy per unit volume which, at the elastic limit, reaches a definite limiting value independent of the simple or complex nature of the applied stress.

Diagrams were plotted in which the marked points represented experimental determinations of the ratios between the elastic limits. On comparing the loci of these experimental points with the graphs representing the alternative hypotheses, it was observed that the strain-energy graph was in fair agreement with experiment throughout the field of investigation, and that the graphs representing other hypotheses were in agreement in narrower fields, *e.g.* in the case of Saint Venant's hypothesis, when the ratio between the principal stresses was less than $\pm$ 0.30.

The diagram shown in fig. 13 illustrates the method of comparison adopted for two-dimensional stresses; the axes OX and OY representing the principal stresses on the material and the lengths OA and OB the elastic limits in simple tension. The co-ordinates of points in the quadrants represent the principal stresses for complex combinations. The strain-energy hypothesis is represented by an ellipse whose eccentricity varies slightly for different values of Poisson's Ratio σ. Other hypotheses are represented by figures composed of intersecting straight lines. The diagram includes also a fifth hypothesis, published in 1916 by Dr. Albert Becker,[1] viz. that the elastic limit is determined by a dual condition—limiting maximum shear stress and limiting maximum strain. This is represented by a ten-sided figure which, for combinations approximating to shear stress, nearly coincides with the ellipse for the strain-energy hypothesis. Where two nearly equal like stresses are combined, as in turbine discs, Dr. Becker's hypothesis appears to overestimate the elastic limit, although not so greatly as does Saint Venant's.

In the earlier report it was explained that the strain-energy hypothesis represents not merely an arbitrary assumption, roughly endorsed by published data, but an attempt to apply established thermodynamic principles to the now generally accepted theory that the production of non-elastic strain is associated with a change in the state of the metal, from the crystalline to the 'vitreous' or 'amorphous' phase. The earlier report, however, was chiefly an analysis of published data, without any attempt at explanation. In what follows, the object is to trace the theoretical bearing of thermodynamic principles on the relation between the elastic limits under different kinds of stress.

Physical Theory of Non-elastic Strain.

The current theory for permanent strain rests on two main experimental observations: (1) that permanent strain is the cumulative result of numerous gliding movements in individual grains of metal, the reality of these movements being demonstrated by the slip-bands observed when polished faces of a block strained beyond the elastic limit are examined under the microscope; and

[1] A. J. Becker, *Bulletin* 85, University of Illinois.

(2) the development of permanent strain is associated with a change of physical state, from the crystalline to the undercooled liquid or 'vitreous' state. In the developed theory the change from the elastic crystalline state to the vitreous is held to be essential for the explanation of the gliding movement.

DIAGRAM FOR ELASTIC-LIMIT : TWO PRINCIPAL-STRESSES.

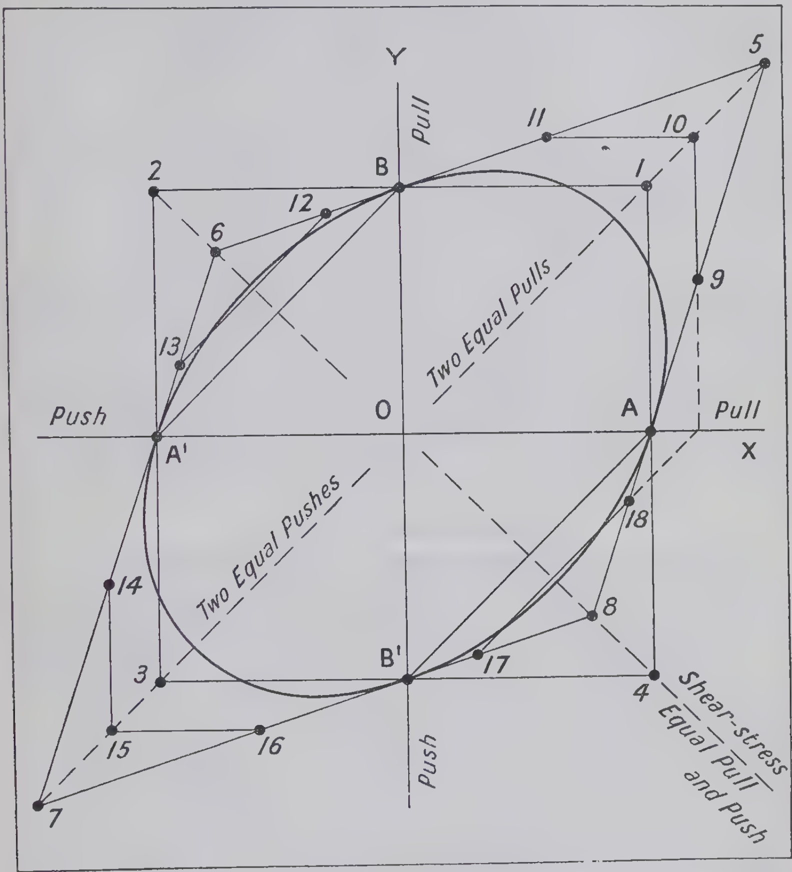


FIG. 13.

Strain-energy hypothesis is represented by ellipse (Equation $X^2+Y^2-2\sigma.X.Y=\overline{OA}^2$ Poisson's ratio $\sigma=\frac{1}{3}$).

Rankine hypothesis for max. principal-stress, figure A1B2A′3B′4.
St. Venant hypothesis for max. principal-strain, figure A5B6A′7B′8.
Guest's hypothesis for max. shear-stress, figure A1BA′3B′.
Becker hypothesis for max. shear-stress and strain, figure A9, 10, 11, B12, 13, A′.

Before this theory was advanced by Beilby, and developed by Ewing, Rosenhain, and others, there was no satisfactory explanation of permanent strain. The difficulty is clearly defined in Professor James Thomson's statement, written in 1861 [2] : 'I have not any conception of continuous crystalline structure admitting of what may be called ductile bending—that is, bending

[2] James Thomson, *Proc. Roy. Soc.*, 1861.

beyond the limits of elasticity . . . and still remaining of the nature of one continuous crystal. What . . . may be the nature of the change of molecular arrangement induced by bending them I cannot say; but I suppose that, in their yielding, their crystalline structure is materially altered, and rendered discontinuous where, before, it was continuous.'

Where Thomson used the word 'discontinuous,' current theory substitutes 'vitreous' or 'amorphous,' with many associated ideas and more or less satisfactory definition. The physical and chemical properties of vitreous metals are still in some measure uncertain, because the vitreous phase tends to recrystallise; but it is known that the physical characteristics include great hardness, the limited mobility of a highly viscous fluid, and, in the case of iron, low magnetic permeability. The change is not merely a matter of pulverising. Without entering on controversial matter that has collected round the definition of an 'allotropic' change, it may be accepted that the change that occurs when a ductile metal is strained is purely physical in the sense that it involves only a rearrangement of the molecules without change of internal molecular structure.

If this theory be accepted as an explanation of permanent strain, it is evident that the change from the crystalline to the vitreous state must precede the gliding action; the essential condition for gliding movement cannot be merely a consequence. Without denying the probability that further quantities of metal may suffer the same change during the gliding movement, it is clear that the initial change of state must be a direct result of the preceding elastic stage of straining. It follows that, in the course of any alternative process by which the metal can be strained to its different elastic limits by the application of different stresses, the metal that suffers the change must absorb—as heat or as mechanical work—such quantities of work as enable it to change from the stable crystalline state to the vitreous phase, which, at ordinary temperatures, is known to be 'metastable,' *i.e.* unstable but restrained from change by 'internal viscosity.'

Thermodynamic Principles.

When any physical or chemical change is produced mechanically, by forces exerted by external bodies acting on a mass of the substance in question (*e.g.* by a piston acting on vapour enclosed in a compressor cylinder), the work done by the forces during the change is ordinarily greater than the quantity that can be regained by allowing the change to occur in the reverse direction at the same temperature. The difference between the two quantities is converted to heat by the action of friction or other 'irreversible' effects. Under ideal frictionless conditions the two quantities of work may be equal, and the process of change is then said to be 'reversible.'

A well-known application of the second law of thermodynamics states that when a given change can be produced by different reversible processes, carried out at the same temperature, the quantity of work that must be done by the forces acting on the substance must be definitely constant, *i.e.* independent of how the forces act. If one and the same change of physical state can be caused by stresses of different kinds, pull or shear or combinations of these, the quantities of work done by these forces on unit mass of substance suffering the change must always be the same, provided that the action is reversible and is carried out at the same temperature. If the action be not perfectly reversible, somewhat more work may have to be done; but the quantity will approximate to the ideal constant value if the action is nearly reversible.

It is clear that this thermodynamic law affords a theoretical relation between the elastic limits of a ductile metal. To develop such a relation, however, it is necessary to equate expressions for the work done by different forces in changing unit mass of metal from the crystalline to the vitreous state in reversible and isothermal processes.

Prof. James Thomson's Work on 'Regelation.'

In 1849,[3] in applying thermodynamic principles to problems involving change from the crystalline to the fluid state, Professor James Thomson showed that the melting-point of ice should be lowered by the application of fluid pressure;

[3] James Thomson. *Trans. Roy. Soc., Edinburgh*, 1849.

and in the following year the theorem was verified in experiments carried out by Lord Kelvin, then Professor William Thomson.

Considering the Carnot cycle of operations in an ice-water heat engine operating in a small range of temperature near the freezing-point, Thomson showed that the work done in a cycle using a difference of pressure dp, acting through the small change of volume $(V-v)$ that occurs when ice changes to water, could be equated to the work done by an ideal heat engine receiving the latent heat, L, of the water produced and working through a range of temperature equal to the corresponding depression of the freezing-point. Thus :

$$dp\,(V-v) = L.dT/T$$

whence

$$dT/dp = (T/L)\,(V-v)$$

where dT/dp is the rate of fall of the freezing-point with rise of pressure.

In establishing this expression, Thomson considered only the influence of fluid-pressure, acting equally in all directions; but in a later paper, of 1861,[4] the application of thermodynamic principles was greatly widened. He then states : '. . . I soon positively formed the opinion that *any stresses whatever, tending to change the form of a piece of ice in ice-cold water* (whether these stresses be of the nature of pressures or tensions—that is, pulls or pushes—and whether they be in one direction alone, or in more directions than one), *must impart to the ice a tendency to melt away and to give out its cold, which will tend to generate, from the surrounding water, an equivalent quantity of ice free from the applied stresses.*' The italics are Thomson's, and appear noteworthy in relation to the straining of metals. It would seem that the action of gliding may be not unlike the action pictured by Thomson.

It does not appear that Thomson proceeded to apply to metals the wider principle enunciated in 1861; and it has often been pointed out that the regelation theorem of 1849 does not in itself suffice as an explanation of the change of state that occurs in metals. Unlike water, most metals show a slight expansion when they change from the crystalline to the fluid form.

The Thermodynamic Reversibility of the Stages of Strain.

In applying the second law of thermodynamics to establish any relation between different forces that can effect a specified change, it is essential first to define ideal conditions such that the process of change may be reversible and isothermal. No real process being strictly reversible under working conditions, it is inevitable that some tax is made on the imagination.

The process of permanent strain may be regarded as comprising two distinct stages : (*a*) the initial stage, in which the metal is strained elastically until the first molecule leaves the 'continuous lattice' of the crystal to enter the 'discontinuous assembly' of the vitreous phase; and (*b*) the subsequent stage, in which, in consequence of the changed physical properties of the metal, gliding movements are produced by any shear-stress that is applied.

It is not suggested that the second stage approximates to a reversible process. Usually pictured as viscous gliding, but probably more complex in action and possibly akin to the process of melting and recrystallisation pictured by Thomson's description of the action in the case of ice, the action certainly results in large quantities of work being converted to heat, raising the temperature of the metal. Such an irreversible action affords no basis for an application of the second law of thermodynamics.

It is important to note, however, that this irreversible gliding movement is only a consequence of the preceding change of state, and comparable with other consequences, such as the recognised changes in thermal and electrical conductivity, magnetic permeability, and chemical activity. The motion that produces permanent strain is possible only when the change of state has been produced by an earlier cause.

The initial stage, on the other hand, exhibits the characteristics of a reversible process, although, admittedly, experimental reversal may be difficult of demonstration. Considerations bearing on this point may be summarised as follows : (1) Although the work done in straining the metal up to and beyond

[4] James Thomson, *Proc. Roy. Soc., London*, 1861.

the elastic limit cannot be wholly regained by releasing the applied stresses, there is evidence to show that part of the difference may be regained by other methods. For example, since the electrolytic potentials of the strained and unstrained metals are different, a quantity of work may be regained by using electrodes of the two in a suitable cell. When a given mass of vitreous metal is replaced by an equivalent mass of crystalline, a quantity of electrical work is obtained, and any deficiency—such as would doubtless be found in experiment—may fairly be regarded as the equivalent of heat generated during the gliding motion of the second stage of straining. (2) While the great internal viscosity of the vitreous metal prevents recrystallisation at ordinary temperatures, the process may be accelerated by raising the temperature (above 400° C. in the case of steel). (3) General experience in the widest fields of research indicates that every change of physical or chemical state is inherently reversible, actually or by means of appropriate imaginary methods of operation. (4) In straining the metal elastically, up to the elastic limits at which the change occurs under different complex stresses, quantities of work are done on the metal in strictly reversible manners; and these quantities may be expressed in terms of the applied stresses and the elastic constants of the metal.

Expression for the work done in the Reversible Stage of Strain.

The reversible process now considered is the initial stage of strain, commencing when the crystalline metal—initially free from strain—is subjected to a gradually increasing stress, and terminating when the first molecules are projected out of the 'crystalline lattice' into the 'vitreous assembly.' It is evident that the process can be carried out isothermally, thus completing the conditions required for the application of thermodynamic principles. We have to attempt the problem of obtaining, in terms of the applied stresses, an expression for the work done on unit mass of metal suffering the change; or, alternatively, an expression proportional to this quantity.

If the whole mass suffered the change, and were isotropic, and if the change involved no alteration of volume other than the elastic compression or expansion, the work done by the applied forces would be identical with the elastic strain-energy at the elastic limit; and would be given by the expression

$$W=\frac{1}{2E}(X^2+Y^2+Z^2) \quad - \quad \frac{\sigma}{E}(YZ+ZX+XY)$$

Part of this quantity of work, however, would be stored in the vitreous metal produced, in virtue of its elastic compression or expansion under the applied stresses. Thus the nett work corresponding to the change of state would be

$$W'=W-\frac{1}{2K}\left(\frac{X+Y+Z}{3}\right)^2$$

where K denotes the (unknown) modulus of compressibility of the vitreous metal.

On the assumption that the change results in a change of volume, in ratio α as measured free from stress, a further term may be introduced—analogous to the expression used in Thomson's 'regelation' theorem : Thus

$$W''=W'-\alpha\left(\frac{X+Y+Z}{3}\right)$$

The relation between the elastic limits would then be expressed by an equation stating that the quantity W'' is constant, *i.e.* independent of the ratios between the stresses X, Y, and Z.

The problem is appreciably complicated by the non-isotropic character of actual crystalline metal; and by the fact that only a small proportion of the total mass actually suffers the change of state. On the probable assumption that the increase of volume α is only slight, the relative importance of the terms introduced for W' and W'' largely disappears; or on the not improbable hypothesis that the change involves a reduction of volume, the two terms may wholly disappear, leaving the strain-energy W as the constant governing factor. On this latter hypothesis we may picture the change of state as accompanied

by the formation of numerous small cavities along the gliding surfaces, so that the action may be not inconsistent with the reduction of density so commonly recorded in experiment. That such cavities are formed on the gliding surfaces may be inferred also from many phenomena associated with mechanical fatigue.

It is highly probable that the work done on the small masses that suffer the change is drawn in part from the strain-energy of surrounding crystalline masses that suffer no change of state. Were this not so, the gliding surfaces would doubtless spread more widely than they appear to do. The smaller the grain size, the smaller the volume from which a given change can draw strain-energy, and the greater the stresses required to attain the elastic limit.

Few metals being free from internal strain, quantities of work must be stored in individual grains before external stresses are applied. These quantities doubtless contribute to the total constant energy required to effect the change; but at present they lie beyond the possibility of estimation.

The microstructure of most ordinary metals being highly complex, the distributions of strain-energy between individual grains, or parts of grains, must be exceedingly irregular, particularly when—as in the case of Ferrite and Cementite—the elastic constants differ widely. Even in pure metals and solid solutions, the non-isotropic properties of the grains must result in irregular distributions of energy, so that some grains suffer permanent strain before others. As a general rule, however, we may assume that the ratio between the maximum and mean intensities of strain-energy will be nearly constant unless the grain size is abnormal, so that the mean value may be expected to follow the law governing the maximum. The ratio between the elastic limit and the yield-point may be closely related to this irregular distribution of energy.

In view of these considerations, and in spite of the numerous uncertainties of detail, it seemed to the author probable that the strain-energy per unit volume might attain, at the elastic limit, a nearly constant limiting value independent of the simple or complex nature of the applied stresses; or if the strain-energy were found to vary appreciably and definitely, that its changes might afford a serviceable clue to the investigation of the more complex phenomena of strain. On plotting the graphs to represent the hypothetical values of the elastic limit given by the equation

$$(X^2+Y^2+Z^2)-2\,\tau\,(YZ+ZX+XY)=F^2$$

where F signifies the elastic limit in simple tension, it was found that the experimental loci were very close to the hypothetical, not only for comparatively pure metals, but also for those of more complex structure, such as Pearlitic steels. Judging from the results obtained in a variety of applications, the above relation gives, in the opinion of the author, a very reliable measure of the relation between the elastic limits.

It is not suggested that the non-ductile fracture of a brittle material, or even the ultimate strength of a ductile metal, is governed by the thermodynamic considerations that have been set forth. Even the yield-points are only approximately concerned, and the stricter application is limited to the relation between the elastic limits.

VI.

Alternating Combined Stress Experiments.

By W. Mason, *D.Sc., and* W. J. Delaney, *B.Eng.*

In 1914-15 some tests on the dead mild steel furnished by the British Association Committee on Complex Stress Distribution were published.[1] These tests included alternating torsion and alternating bending, but these straining actions were each applied separately. The experiments now described have been made during the past ten months upon the same batch of steel; but in these later tests the alternating bending and torsion were simultaneously applied. The

[1] 'Alternating Stress Experiments,' *Proc. Inst. Mech. Eng.*, January-May, 1917.

TABLE I. (MASON AND DELANEY.)

SUMMARY OF ALTERNATING STRESS TESTS. COMBINED BENDING AND TWISTING.

Specimen		Bending		Twisting		Bending and Twisting	—
Number	Conditions of Application of Stress	Range of Stress due to Bending $=\pm p_1$*	‘Range of Strain’ due to Bending	Range of Stress due to Torque $=\pm q_1$*	‘Range of Strain’ due to Torque	Range of Maximum Shear Stress	Cycles
		tons per sq. in.	cm.†	tons per sq. in.	cm.†	tons per sq. in.	
B31 (solid)	$p_1=2q_1$ (approx.) Phase difference=90°	Various ranges from ±8·00 to ±11·00 ±12·40 ±13·28	Various ranges from 5·75 to 8·90 9·60 to 9·70 10·50	Various ranges from ±4·00 to ±5·96 ±6·45 ±6·94	Various ranges from 7·95 to 12·30 13·40 15·00 to 15·90	Various ranges from ±4·00 to ±5·96 ±6·45 ±6·94	0 to 358,170 358,170 to 656,620 656,620 to 800,000 (fracture)
A13 (solid)	$p_1=2q_1$ (approx.) Phase difference=90°	Various ranges up to ±12·00 ±12·46 ±12·92	Various ranges up to 7·75 8·20 8·60 to 8·85	Various ranges up to ±5·90 ±6·13 ±6·36	Various ranges up to 8·45 8·85 9·25 to 9·35	Various ranges up to ±6·00 ±6·23 ±6·48	0 to 384,500 384,500 to 622,500 622,500 to 950,000 (fracture)
A6 (hollow)	$p_1=2q_1$ (approx.) Phase difference=90°	Various ranges—see Table III. ±11·50	to 6·05	±5·70	to 6·75	±5·75	0 to 911,900 911,900 to 1,584,900 (fracture)
B24 (hollow)	Phase difference=90°	Various ranges and with various ratios of p_1 to q_1 See remarks on page 331					A large number of short runs Total number of cycles 897,000 (fracture)
A9 (solid)	Constant range of q_1 Increasing range of p_1 Phase difference=0°	Various ranges up to ±8·90 ±9·40 ±9·92 ±10·33 ±10·75 ±11·16	Various ranges up to 5·33 5·68 to 5·80 6·15 to 6·27 6·58 to 6·80 7·23 to 7·35 7·80 to 8·02	Constant range of ±4·50 ±4·50 ±4·50 ±4·50 ±4·50 ±4·50	Various ranges up to 6·75 6·80 to 6·88 6·90 to 7·00 7·10 to 7·18 7·23 to 7·35 7·45 to 7·47	Various ranges up to ±6·34 ±6·50 ±6·70 ±6·85 ±7·00 ±7·20	0 to 2,395,000 2,395,000 to 3,578,000 3,578,000 to 4,590,000 4,590,000 to 5,743,000 5,743,000 to 6,629,000 6,629,000 to 7,539,000 (fracture)
B17 (solid)	Constant range of p_1 " " q_1 Phase difference=0°	Constant $=\pm 9{\cdot}75$	from 6·00 to 6·90	Constant $=\pm 4{\cdot}87$	from 6·90 to 7·70	Constant $=\pm 6{\cdot}90$	0 to 4,800,000 (fracture)

*p_1=Maximum value of stress due to bending (assuming perfect elasticity)$=\frac{Mr}{I}$, where I=Moment of Inertia of section about a diameter, and ±M=Range of Bending moment.

*q_1=Maximum value of torsional stress (assuming perfect elasticity)$=\frac{Tr}{J}$, where J=Polar Moment of Inertia of section, and ±T=Range of Torque.

† Length of Scale passing fixed line of Collimation

For Bending—Angle between tangents at ends of specimen$=\frac{\text{‘Range of Strain’ (above)}}{660}$

‘ Range of Strain ’ (above)

TABLE II.

COMPARISON OF TESTS IN SIMULTANEOUS BENDING AND TWISTING WITH BENDING TESTS AND WITH TORSION TESTS. (*Solid Specimens.*)

Torsion only. A7*			Bending only. A15*			Combined Bending and Twisting. Phase angle = 90°. A13				
Range of Stress	Range of Strain	Cycles at each Range of Stress	Range of Stress	Range of Strain	Cycles at each Range of Stress	Range of Stress due to Bending	Range of Strain due to Bending	Range of Stress due to Torque	Range of Strain due to Torque	Cycles at each Combined Range
tons per sq. in.	cm.		tons per sq. in.	cm.		tons per sq. in.	cm.	tons per sq. in.	cm.	
±5·00	7·01	—	±10·00	6·12	—	±10·00	6·20	±5·00	6·90	—
±5·60	7·84	46,000	±11·54	to 7·30	130,000	±11·50	to 7·00	±5·67	to 8·00	242,000
±5·85	to 8·26	31,000	±12·00	to 7·72	76,000	±12·00	to 7·75	±5·90	to 8·45	50,000
±6·10	to 8·82	38,000	±12·50	to 8·40	294,000	±12·45	to 8·20	±6·13	to 8·85	238,000
±6·35	to 9·56	368,000	±13·00	to 9·20	307,000	±12·95	8·60 to 8·85	±6·35	9·25 to 9·35	50,000 (fracture)
±6·65	to 10·96	2,215,000	±13·25	to 9·64	247,000	—	—	—	—	—
±6·90	to 12·24	1,981,000	±13·50	to 9·94	282,000	—	—	—	—	—
±7·15	to 13·55	1,873,000	±13·75	to 10·33	270,000 (fracture)	—	—	—	—	—
±7·40	to 14·67	1,492,000	—	—	—	—	—	—	—	—
±7·65	—	35,000 (fracture)	—	—	—	—	—	—	—	—

* For details of these tests see 'Alternating Stress Experiments,' *Proc. Inst. Mech. Eng.*, January-May, 1917.

TABLE III.

COMPARISON OF TESTS IN SIMULTANEOUS BENDING AND TWISTING WITH BENDING TESTS AND WITH TORSION TESTS. (*Hollow Specimens.*)

Torsion only						Bending only			Combined Bending and Twisting. Phase angle = 90°.				
B22*			B27*			A16*			A6				
Range of Stress	Range of Strain	Cycles at each Range of Stress	Range of Stress	Range of Strain	Cycles at each Range of Stress	Range of Stress	Range of Strain	Cycles at each Range of Stress	Range of Stress due to Bending	Range of Strain due to Bending	Range of Stress due to Torque	Range of Strain due to Torque	Cycles at each Combined Range
tons per sq. in.	cm.		tons per sq. in.	cm.		tons per sq. in.	cm.		tons per sq. in.	cm.	tons per sq. in.	cm.	
±5·00	5·90	—	±5·00	6·03	—	±10·00	5·20	—	±10·00	4·90	+5·00	5·55	—
±5·50	6·45 to 6·52	35,000	—	—		±11·00	5·67 to 5·72	37,000	±11·00	5·50 to 5·65	+5·50	6·15 to 6·30	827,000
±5·62	to 6·90	229,000	±5·60	to 9·06	2,284,000	—	—	—	±11·00	5·60 to 5·55	±5·70	6·70 to 6·65	41,000
—	—	—	—	—	—	±11·50	to 6·10	46,000	±11·50	6·00 to 6·05	±5·70	6·60 to 6·75	673,000 (fracture)
—	—	—	±5·85	to 10·15	1,052,000	—	—	—	—	—	—	—	—
±6·25	to 10·72	2,335,000 (fracture)	±6·10	to 11·36	1,037,000 (fracture)	—	—	—	—	—	—	—	—
—	—	—	—	—	—	±12·50	to 7·80	350,400	—	—	—	—	—
—	—		—	—	—	±12·75	to 8·18	416,400 (fracture)	—	—	—	—	—

* For details of these tests see 'Alternating Stress Experiments,' *Proc. Inst. Mech. Eng.*, January-May, 1917.

main feature of all these experiments was measurement of the ranges of cyclic bending and twisting strain.

The machine used was the one described in the paper above referred to, and the frequency of the cycles of stress was, as previously, 200 per minute. The apparatus for measurement of the strains is also substantially the same, and the ranges of strain simultaneously measured are the angle of twist, and the angular deflection due to bending between two sections about 4 in. apart. The bending moment was uniform over this length, which included $2\frac{1}{2}$ in. turned parallel between shoulders. The mirrors for measuring these strains were fixed to the squared shoulders of the specimen.

All the conditions were the same as for the experiments previously made in bending and twisting separately; the specimens were cut from the same two bars A and B, the number of specimen denoting the position of the specimen in the uncut bar. These conditions allow a systematic comparison to be made between these tests and those done previously—a procedure of obvious advantage in saving of time and labour, especially since simultaneous measurement of the two sets of cyclic strains entails considerable patience.

Phase Angles of Bending and Twisting in Combined Cycles.

A set of six specimens (Tables I. and IV.) was tested, four of which were solid and two hollow. These were of similar shape and dimensions, and the surfaces were finished in the same manner as previously. It will be seen that in four of these the phase angle (*i.e.* the angle between the crank giving the simple harmonic alternating bending moment and the crank which applied the S.H. alternating torque) was 90°; and in two of them the phase angle of them was zero. In the case of Specimens A6, A13, and B31, the maximum direct stress calculated from the bending moment was approximately twice the maximum shear stress calculated from the torque. In calculating the stresses, the usual simple formulæ for bending and twisting were used.

TABLE IV.

Preliminary Dead-weight Tests and Diameters of Specimens.

Specimen		Preliminary Dead-weight Test						
Number	Diameter	Range of Bending Moment $=\pm M$	Range of Stress due to Bending $=\pm p_1=\pm\frac{Mr}{I}$*	Range of Measured Strain due to Bending	Range of Torque $=\pm T$	Range of Stress due to Torque $=\pm q_1=\pm\frac{Tr}{J}$†	Range of Measured Strain due to Torque	Remarks
	inch	lb.-in.	tons per sq. in.	cm. on scale	lb.-in.	tons per sq. in.	cm. on scale	
B31	0·3125 solid	± 54	± 8·00	5·70	± 54	±4·00	7·90	See previous test p. 141 'Alternating Stress Experiments,' *Proc. Inst. Mech. Eng.* Jan.-May, 1917
A13	0·4375 solid	±183	±10·00	6·60	±183	±5·00	7·20	Accidentally bent and afterwards straightened
A6	hollow, see below	±172	±10·00	4·90	±172	±5·00	5·60	—
B24	hollow, see below	±149	± 8·54	4·20	±149	±4·27	4·90	—
A9	0·4376 solid	±183	±10·00	5·90	±183	±5·00	7·20	—
B17	0·4371 solid	±179	± 9·75	5·95	±179	±4·87	6·80	—

* I=Moment of Inertia of Section about a diameter, $(\text{inch})^4$.
† J=Polar Moment of Inertia of Section, $(\text{inch})^4$.

Number of Specimen	External Diameter (inch)	Internal Diameter (inch)	Thickness of Wall			Remarks
			Max. (inch)	Min. (inch)	Mean (inch)	
A6	0·5760	0·5050	0·0360	0·0355	0·0357	—
B24	0·5770	0·5050	0·0380	0·0340	0·0360	Bore eccentric

With phase angle 90°, the stress p due to bending and the shear stress q due to twisting may at any instant t be represented thus—

$$p = p_1 \cos 2\pi\frac{t}{T} \qquad q = q_1 \cos\left(\frac{\pi}{2} + 2\pi\frac{t}{T}\right)$$

where p_1 is the maximum direct stress due to the cycle of bending,
" q_1 " " shear " " " twisting,
and T is the period of either cycle.

The principal stresses at the time t will be—

$$\tfrac{1}{2}\left\{p_1 \cos 2\pi\frac{t}{T} \pm \sqrt{\left(p_1^2 \cos^2 2\pi\frac{t}{T} + 4q_1^2 \sin^2 2\pi\frac{t}{T}\right)}\right\}$$

Now if $p_1 = 2q_1 = 2a$ (say), as in the three tests just mentioned, the principal stresses are

$$a \cos 2\pi\frac{t}{T} \pm a$$

and the maximum shear stress due to the continued action is thus always equal to 'a.'

The regions at which the maximum shear stress 'a' always exists are, of course, at the diametrically opposite skins of the specimen in the plane of the bending moment; the planes of the shear 'a' are at right angles to the skin and rotate uniformly, half a revolution made in the time T of one cycle. On other planes at 45° to a principal stress (the principal planes also make one half revolution in time T), the maximum shear stresses of the cycle vary between 'a' and $\frac{'a'}{2}$ according to the position of the 45° plane with respect to the plane of the principal stresses. Comparing this state of stress with the stress-system induced by an alternating bending or an alternating twisting separately applied, the number of planes exposed to the maximum shear stress 'a' is indefinitely increased, and correspondingly the gliding planes of a much larger number of crystals are subjected to alternating shear stress of maximum value 'a.'

In the tests of Nos. A9 and B17 the phase angle was zero. The range of twisting moment on A9 was constant throughout, while the bending moment was increased by stages. B17 was subjected to constant ranges of both bending and twisting until fracture occurred. Both tests were continuous, the machine being run throughout without stopping.

Discussion of Tables II. and III.

Tables II. and III. have been prepared in order that a comparison may be made conveniently between the stresses and strains (i) in the 90° phase combined stress tests, and in (ii) tests wherein the bending and twisting were applied separately. Hollow and solid specimens are put in separate Tables in view of a previous result of one of the authors, viz. that a solid specimen under alternating bending or twisting is apparently considerably stronger than a hollow one.[2] The result of the comparison of stresses may be summarised thus—

If P be the maximum direct stress of bending cycles separately applied,
if Q be the maximum shear stress of torsion cycles separately applied,
and if p_1, q_1, a, have the meaning previously assigned,
then for the ranges of stress which produced fracture, it was found that $P > p_1$ and $Q > q_1$.

The maximum shear stress due to bending separately applied being $\frac{1}{2}P$, the relations between the maximum shear stresses are therefore $\frac{1}{2}P > a$ and $Q > a$.

It should be observed that these results are got by comparing only two specimens tested with combined alternating stress at phase angle 90°. A consideration of B31 also supports these conclusions if the smaller number of cycles endured by it is considered. It may be remarked, also, in view of the small

[2] 'Alternating Stress Experiments,' *Proc. Inst. Mech. Eng.*, January-May, 1917, p. 151. The stresses are calculated (see Table I.) from formulæ which assume that stresses are proportional to strains, which gives a calculated maximum stress greater than the actual, more especially for solid specimens.

number of tests made, that measurement of the cyclic strains furnishes a check on the accuracy of the applied stresses as well as on the normality of the specimen. The columns headed 'Preliminary Deadweight Test' in Table IV. are inserted so that this and other comparisons may be made.

It is probable that the differences of shear stress

$$\tfrac{1}{2}P-a \qquad \text{and} \qquad Q-a$$

would have been rather larger if the number of cycles at the stresses giving fracture in the combined tests had been as large as the number of cycles in these separate tests. It would appear, then, that there is strong *prima-facie* evidence for believing that for mild steel in combined tests with phase angle 90° and with $p_1=2q_1$ the limiting range of stress is less than the limiting ranges for separate tests by about 10 per cent.

With regard to the cyclic strains (phase difference 90°) it will be observed that the percentage increases of the component bending and twisting strains measured in the combined tests are practically the same as those measured for the *same stresses* in the separate tests; in other words, the total (*i.e.* elastic + non-elastic) strains pertaining to the bending and twisting applied simultaneously are practically the same as if cycles of the same ranges of stress had been applied separately. But the ranges of stress causing fracture in combined tests are about 10 per cent. less than the ranges that would have produced fracture had the bending and twisting been applied separately; thus the component ranges of strain at stress ranges approaching *fracture* are, as seen in Tables II. and III., much less in combined than in separate tests. Comparing solid specimens, the percentage decrease of total strain comes out to be 36 per cent. for the torsional strain, and 20 per cent. for the bending strain; and comparing hollow specimens, the respective figures are approximately the same, viz. 34 and 24. To obtain these figures, A6 and A13 were compared respectively with A14, A16, B22, B27,[3] and A7, A11, A15.[3]

Remarks on Component Strains in Tests with Phase Angle 90°.

An interesting point observed in the tests with phase difference 90° was the following. After a run under a pair of simultaneous stresses, during which there was cyclic non-elastic strain, an increase of one of the pair of stresses, say the bending stress, caused a slight contraction in the range of torsional strain, and *vice versa*. The test of Specimen B24 was specially directed towards the study of this effect. To obtain an enlarged effect, one of the simultaneous stresses was made zero and a run of several thousand cycles given. Then, without stopping or alteration of speed, the other stress was reimposed.

For example, in course of test of B24,

(*a*) With range of direct stress due to bending $= \pm 13{\cdot}4$ tons per sq. in. and range of torque zero, the range of bending strain increased from 7·35 to 7·58 cm.[4] during 40,000 cycles. On adding torque to give $\pm 5{\cdot}30$ tons per sq. in. of shear stress and retaining the same range of bending moment, the bending strain decreased to, and remained at, 7·33 cm. during 12,000 cycles.

(*b*) Again, with range of shear stress due to torsion $= \pm 6{\cdot}70$ tons/$\square''$ and range of bending moment zero, the range of torsional strain increased from 10·9 to 13·5 cm. during a run of 108,000 cycles. On adding bending moment to give $\pm 11{\cdot}95$ tons per sq. in. and retaining the same torque, the range of torsional strain decreased to, and remained at, 11·80 cm. during 25,000 cycles.

This effect is not due to friction occasioned by the extra loading on the parts of the machine when the additional stress is put on. In case (*b*) the effect of friction would be the reverse of that observed; and while in (*a*) the effect of friction would be similar to that observed, the magnitude of the friction (of ball bearings) would not be sufficient to account for the decrease. Moreover,

[3] See 'Alternating Stress Experiments' (*Proc. Inst. Mech. Eng.*, January-May, 1917) for record of these tests.

[4] On image of scale reflected from mirrors.

with phase difference 90°, the maximum of bending moment coincides with zero torque, and *vice versa*; so that friction must have diminished to zero at the time when the effect is measured.

But another circumstance must be considered. Suppose the specimen to be simultaneously stressed, and consider the instant when the torque is a maximum and the bending moment zero. There still remains the bending due to hysteresis; and application of the torque to the bent specimen would give a reduced torsional stressing and straining at the parts deflected away from the axis of the specimen by this hysteresis bending. But a rough calculation shows that the observed effect is too large to admit of this explanation, the hysteresis bending (though this was not measured) being evidently too small. The authors cannot venture to attempt an explanation of this phenomenon. Since the ranges of strains in combined tests agree substantially in magnitude with those under the same ranges of stress applied separately, it might appear that the work done in a combined cycle would be equal to the sum of that done in the same cycles applied separately. But this cannot be assumed, since the width and shape of the corresponding hysteresis curves may not be similar. The authors have not as yet attempted to measure the width of the hysteresis diagram; and until this is done it seems too early to attempt any explanation.

Remarks on Component Strains with Phase Angle Zero.

Turning now to the two tests with the bending and twisting in phase with each other—in B17 the range of direct stress due to bending was twice the range of shear stress due to twisting; while in A9 the ratio between these respectively was increased during the test, and during the last stage, at the end of which fracture occurred, the ratio was approximately 2·5. Comparison of the ranges of shear stress with those of A1, A7, and A11, A15, shows that the average values of the induced maximum shear stresses producing fracture are almost exactly the same for both simultaneous and for separate applications of bending and twisting. The material in this as in other respects already found, conforms approximately to an extension of Guest's Law to alternating stresses.[5] [6] These shear stresses are thus about 10 per cent. greater than that of the average of the shear producing fracture when the phase difference is 90° and $p_1 = 2q_1$.

The component cyclic strains (elastic + non-elastic) induced towards the end of the last stage of the combined tests (phase angle zero) were much below those in the final stages of comparable tests in separate bending and twisting. Thus, comparing A9 with A7, the reduction in range of total torsional (including both elastic and non-elastic) cyclic strain is 50 per cent.; and comparing A9 with A11 and A15, the reduction in total cyclic bending strain is 23 per cent. Comparing A17 with A7, the reduction in range of total torsional strain is 47 per cent.; and, again, comparing A17 with A11 and A15, the reduction in range of total cyclic bending strain is 36 per cent. It should be pointed out, however, that the components of the alternating combined *stresses* at fracture range were individually much less than the stresses at fracture range due to alternating bending and twisting applied separately.

Principal Strains.

Although the component strains at fracture ranges in the combined tests are less than the respective strains in bending or torsion separately applied to different specimens, it does not follow that the maximum principal strains will be correspondingly smaller. It is therefore of interest to make comparison of the maximum principal strains. Having calculated the latter, it is easy to obtain[7] the greatest strain difference. These quantities have an additional interest inasmuch as strains may be assumed to follow a linear law of distribution from axis to skin of specimen even when the strains are no longer elastic, whereas in the latter condition the stresses have not a linear distribution and are not accurately known.

[5] See 'Alternating Stress Experiments,' *Proc. Inst. Mech. Eng.*, January-May, 1917, p. 149.

[6] B. P. Haigh. *Proc. Inst. Mech. Eng.*, January-May, 1917, p. 190.

[7] As suggested by Prof. L. N. G. Filon.

The distances of scale from indicating mirrors are the same in the present tests as in those made four years ago, and are—

For bending strain apparatus	165 cm.
,, torsional ,, ,, ...	155 ,,

For bending, the mirrors registered a scale displacement proportional to the angle between the tangents at the ends of the specimen. The actual value of this angle is

$$\frac{\text{`Range of Strain' in cm.}}{2\times2\times165}=\frac{R_b}{660}\ \text{(say)}$$

As mentioned on page 331, the mirrors were fixed to the square shoulders of the specimen, and the length l over which this angle is measured is not known accurately. If l be regarded as an equivalent length [8] of specimen, and if r stands for the (outside) radius of specimen, then the maximum fibre strain will (provided the strains have a linear distribution and are proportional to distance from axis of specimen) be

$$\frac{r}{l}\ \text{(angle between tangents)}=\frac{r}{l}\frac{R_b}{660}$$

The angle of twist over length l will be

$$\frac{\text{`Range of Strain' in cm.}}{2\times2\times155}=\frac{R_T}{620}\ \text{(say)}$$

and the shear strain will be accordingly $\frac{r}{l}\cdot\frac{R_T}{620}$

In the Preliminary Dead-weight Tests (see Table IV.), in which the material is elastic, the bending strain

$$=\frac{r}{l}\cdot\frac{{}_eR_b}{660}=\frac{p_e}{E}\quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

where $\pm p_e$ is the range of stress which gives the range of elastic strain ${}_eR_b$ (cm.), and E = Young's Modulus.

The shear stress in torsion similarly

$$=\frac{r}{l}\cdot\frac{{}_eR_T}{620}=\frac{q_e}{C}\quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

where $\pm q_e$ is the range of shear stress giving the range of elastic strain ${}_eR_T$ (cm.), and C is the modulus of rigidity.

If we consider a specimen on which preliminary dead-weight tests were made both in bending and torsion, and if we assume that the fillet at the shoulders affected the bending and twisting stresses and strains in the same proportion, then—

$$\frac{p_e}{E}\Big/\frac{q_e}{C}=\frac{{}_eR_b}{660}\Big/\frac{{}_eR_T}{620}$$

or

$$\frac{E}{C}=\frac{p_e}{q_e}\cdot\frac{{}_eR_T}{{}_eR_b}\cdot\frac{660}{620}$$

[8] Equivalent in the sense that the angle of bending over a parallel-turned length l is to be the same as that measured on the length between the square shoulders.

In the 'preliminary dead-weight tests' of A6, B24, A9, B17, $\frac{p_e}{q_e}=2$, and we obtain the following values of $\frac{\mathrm{E}}{\mathrm{C}}$:—

$$\text{For A6} \quad \frac{\mathrm{E}}{\mathrm{C}}=2\times\frac{5{\cdot}6}{4{\cdot}9}\times\frac{66}{62}=2{\cdot}43$$

$$\text{For B24} \quad \frac{\mathrm{E}}{\mathrm{C}}=2\times\frac{4{\cdot}9}{4{\cdot}2}\times\frac{66}{62}=2{\cdot}48$$

$$\text{For A9} \quad \frac{\mathrm{E}}{\mathrm{C}}=2\times\frac{7{\cdot}2}{5{\cdot}9}\times\frac{66}{62}=2{\cdot}59$$

$$\text{For B17} \quad \frac{\mathrm{E}}{\mathrm{C}}=2\times\frac{6{\cdot}8}{5{\cdot}95}\times\frac{66}{62}=2{\cdot}43$$

giving a mean value for $\frac{\mathrm{E}}{\mathrm{C}}$ of 2.48, which, incidentally, makes Poisson's Ratio $=0.24$.

Assuming E to be 30×10^6 lb. per sq. in., this ratio of E to C makes $\mathrm{C}=12.1\times10^6$.

For specimen B24, which happens to have the mean of the above values of $\frac{\mathrm{E}}{\mathrm{C}}$,

$$\frac{r}{l}\left(\begin{smallmatrix}\text{bending}\\ \text{or}\\ \text{twisting}\end{smallmatrix}\right)=\frac{660}{4{\cdot}2}\times\frac{8{\cdot}54\times2240}{30\times10^6}=0{\cdot}100$$

Taking any test in bending, let R (cm.) be any range of strain, elastic or otherwise, then strain corresponding to R

$$=\frac{r}{l}\,\frac{\mathrm{R}}{660},$$

and substituting the value of $\frac{l}{r}$ for the specimen from equation (1), we have this strain

$$=\frac{p_e}{\mathrm{E}}\,\frac{660}{\mathrm{R}_e}\,\frac{\mathrm{R}}{660}=\mathrm{R}\frac{p_e}{{}_e\mathrm{R}_b}\times\frac{2240}{30\times10^6}=\mathrm{R}\frac{p_e}{{}_e\mathrm{R}_b}\times7{\cdot}43\times10^{-5} \quad . \quad . \quad . \quad (3)$$

For any test in Torsion, the greater principal strain corresponding to a range of strain R (cm.)

$$=\frac{r}{l}\,\frac{\mathrm{R}}{1240}=\frac{q_e}{\mathrm{C}}\,\frac{620}{{}_e\mathrm{R}_\mathrm{T}}\cdot\frac{\mathrm{R}}{1240}$$

$$=\mathrm{R}\frac{q_e}{{}_e\mathrm{R}_\mathrm{T}}\times9{\cdot}27\times10^{-5} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The strains given by formulæ (3) and (4) will be exact if the following assumptions are valid :—

(1) that E and C are the same for the material of all the specimens, and that $\mathrm{E}=30\times10^6$lb./$\square''$;

(2) that the fillets connecting the parallel part with shoulders of a specimen have, in that specimen, the same proportional effect on both bending and twisting strains and stresses;

(3) that the strains have a linear distribution from axis outwards.

Using formulæ (3) and (4), the greater principal strains and the greatest strain difference have been worked out, and set forth in Table V., for the separately applied bending and twisting tests previously done.

TABLE V.

BENDING OR TORSION SEPARATELY APPLIED.

Specimen	Kind of Test	Range of 'Strain.' Cms. on Scale $=$R	Data from Dead-weight Test		Maximum Greater Principal Strain	Maximum of Greatest Strain Difference	Maximum Shear Stress
			p_e or q_e	${}_eR_b$ or ${}_eR_T$			
			tons/□″	cms. on scale			
A15 solid ...	Bending only	10·33	10·00	6·12	·00126	·00157	6·87
A11 „ ...	„ „	11·97	10·50	6·60	·00141	·00176	7·15
A14 hollow ...	„ „	9·87	10·00	5·36	·00136	·00171	6·37
A16 „ ...	„ „	8·18	10·00	5·13	·00118	·00147	6·37
A7 solid ...	Torsion only	14·67	5·00	7·01	·000975	·00195	7·65
A10 hollow ...	„ „	9·75	4·75	5·27	·000813	·00163	6·50
B22 „ ...	„ „	10·72	5·00	5·86	·000850	·00170	6·25
B27 „ ...	„ „	11·36	5·00	6·03	·000877	·00175	6·10
B28 „ ...	„ „	10·30	5·00	5·93	·000805	·00161	6·25
B30 „ ...	„ „	10·65	5·00	5·87	·000842	·00168	6·25

NOTE.—The data in the first five columns are taken from Tables 2 and 3, pp. 136-146, *Proc. Inst Mech. Eng.*, January-May, 1917.

It will be observed that the values of the greater principal strains are widely divergent. The values of the greatest strain difference are not so uniform as the maximum shear stresses, especially if account be taken of the enhanced values of the latter for solid specimens, which greater values are presumably due to the formula of calculation.

It should be observed that the greatest strain differences for *bending*, in Table V., are calculated on the assumption that the minor principal strains e_2 are equal to σe_1 where σ is Poisson's Ratio and e_1 is the (greater principal) strain. This would be exact if the stresses were elastic, but since there is a non-elastic element, the results for e_2 are approximate. The non-elastic element is, however, only about one-sixth (at most) of the total strain, so that the error in assuming that the greatest strain difference (bending tests only), viz. $e_1—e_2$, is equal to $e_1+\frac{e}{4}$ makes, at the most, an error of 4 per cent. in the greatest strain difference.

To apply formulæ (3) and (4) to calculate the strain components for the combined tests, the assumption is made that these components are linearly distributed as in separately applied testing. The effect described on pp. 334 and 335 with respect to decrease of one component strain when the other is increased may possibly be due to non-linear distribution of these strains. It is probable, however, that there will be no marked departure from linearity, and formulæ (3) and (4) have been used to evaluate the component strains for fracture ranges.

Given two direct strains e_1 and e_2 at right angles to each other, and a slide ϕ on planes parallel to e_1 and e_2—which are the circumstances corresponding to the localities of greatest stress in the specimens—then, if δ_1 and δ_2 are the principal strains,

$$\delta_1=\tfrac{1}{2}\left\{e_1+e_2+\sqrt{(e_1-e_2)^2+\phi^2}\right\}$$

$$\delta_2=\tfrac{1}{2}\left\{e_1+e_2-\sqrt{(e_1-e_2)^2+\phi^2}\right\}$$

and the greatest strain difference is

$$\delta_1-\delta_2=\sqrt{(e_1-e_2)^2+\phi^2}$$

where

$$e_1=\mathrm{R}\frac{p_e}{{}_e\mathrm{R}_b}\times 7{\cdot}43\times 10^{-5} \qquad (5)$$

$$e_2=-\sigma\mathrm{R}\frac{p_e}{{}_e\mathrm{R}_b}\times 7{\cdot}43\times 10^{-5}$$

and

$$\phi=2\mathrm{R}\frac{q_e}{{}_e\mathrm{R}}\times 9{\cdot}27\times 10^{-5}$$

TABLE VI.

Combined Tests.

Note.—For tests at 90° phase angle the greatest values of the strain components and of the Greater Principal Strains and Greatest Strain Difference are given both for (*a*) Maximum Bending and Zero Torque, and (*b*) Maximum Torque and Zero Bending

Specimen		Range of Cm. on Bending	Strain Scale Torsion	Width of Hysteresis Loop. Cm. on Scale		Strain Components		Greater Principal Strain $\times 10^3$	Greatest Strain Difference $\times 10^3$	Maximum Shear Stress tons/□"	Remarks
				Bending	Torsion	$e_1 \times 10^3$	$\phi \times 10^3$				
Phase Angle 90°	A6 hollow	6·05	6·75	0·42	0·36	(*a*) 0·92	(*a*) 0·06	(*a*) 0·92	(*a*) 1·15	5·75	
						(*b*) 0·064	(*b*) 1·12	(*b*) 0·58	(*b*) 1·12		
	A13 solid	8·85	9·35	1·22	0·17	(*a*) 1·115	(*a*) negligible	(*a*) 1·115	(*a*) 1·39	6·48	
						(*b*) 0·154	(*b*) 1·20	(*b*) 0·67	(*b*) 1·22		
	B31 solid	10·50	15·90	1·02	2·68	(*a*) 1·10	(*a*) 0·26	(*a*) 1·11	(*a*) 1·40	6·94*	*Cycles at Fracture Range fewer than for other specimens
						(*b*) 0·106	(*b*) 1·55	(*b*) 0·815	(*b*) 1·55		
Phase Angle zero	A9 solid	8·02	7·47	—	—	1·01	0·965	1·17	1·59	7·20	
	B17 solid	6·90	7·70	—	—	0·843	1·025	1·05	1·47	6·90	

The values of δ_1 and $\delta_1-\delta_2$ taken in Table VI. have been calculated from formulæ $(5)_1$. Since σ has been taken as $\frac{1}{4}$, we have

$$e_1+e_2=\frac{3}{4}e_1$$

$$e_1-e_2=\frac{5}{4}e_1$$

and formulæ 5 reduce to

$$\delta_1=\frac{1}{2}\left(\frac{3}{4}e_1+\sqrt{\frac{25}{16}e_1{}^2+\phi^2}\right) \text{ and } \delta_1-\delta_2=\sqrt{\frac{25}{16}e_1{}^2+\phi^2}$$

In working out the values of δ_1 and $\delta_1-\delta_2$ for the specimens tested with phase angle 90°, it has been kept in mind that the maximum strain of bending will coincide with only the residual (or hysteresis) strain at zero torque, and *vice versa.* These hysteresis strains could not be measured, but the width of hysteresis loop may be taken (since the non-elastic element is, at most, only about one-sixth of the total strain) as the difference between the range of strain measured and the range that would have existed if the material had remained elastic. Considering two loops, one for bending and the other, differing in phase by 90° from the first, for the torsion, it is possible to correlate exactly only four points of the one loop with four points of the other. These four points are, of course, the maximum of strain of one kind with the strain at zero stress of the other. If the strains were elastic, and therefore simple-harmonic like the stresses, any one point of the one stress-strain curve could of course be correlated with its corresponding point on the other curve. To find the position of crank, which gives a maximum for the greatest strain difference, a simple calculation was made on the assumption that the strains were simple-harmonic. On this assumption the maximum for this quantity comes out to be at either maximum bending or maximum torque. Since the residual (hysteresis) strains are small (see Table VI.), the one or the other of these two epochs has been assumed to be that of the maximum of greatest strain-difference. The values of this quantity have been calculated for both these epochs, and are given in Table VI.

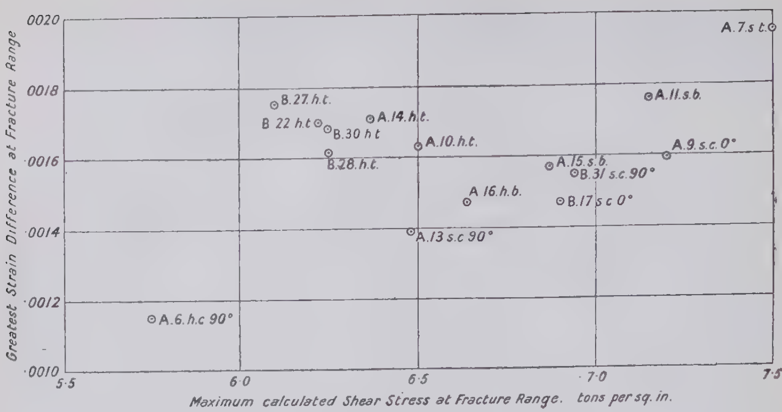


FIG. 14.

Letters after number of specimen :—
h means hollow.
s ,, solid.
t ,, tested in alternating torsion.
b ,, ,, bending.
c 90° means tested in alternating combined bending and torsion, phase angle 90°.
c 0° ,, ,, ,, ,, ,, ,, zero

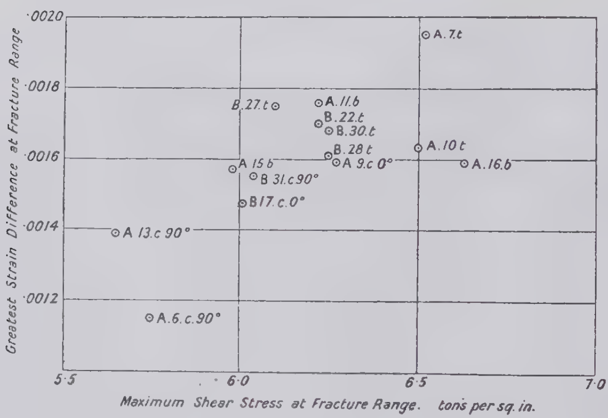


FIG. 15.

Letters after number of specimen have same meaning as in fig. 14.

Calculated stresses of *solid* specimens reduced in ratio $\frac{100}{115}$.

In Specimens A6 and A13 the maximum value (underlined in Table VI.) of greatest stress-difference was found to correspond to the maximum of bending; and in B31, to correspond to the maximum of torque. It is clear that the maximum value of the greater principal stress in A6 and A13 must also correspond to maximum bending, because to obtain this principal strain we have to take the numerical sum of $\frac{3}{4}e_1$ and $\sqrt{\frac{25}{16}e_1^2+\phi^2}$, and the latter of these two expressions, as well as the $\frac{3}{4}e_1$ happens to be a maximum at the maximum of bending moment.

For B31, having regard to the relative values of e_1 and ϕ, it appears that the maximum value of the greater principal strain is also at the maximum of bending.

Relation between Maximum Shear Stresses and Greatest Strain-difference.

An examination of Tables V. and VI. shows that the values of the maximum principal strains are not nearly so uniform as the maximum shear stresses; but they are not so widely divergent as the principal stresses (not tabulated). As might be expected, the values of the greatest strain-difference are rather more uniform, those for the combined tests appearing rather smaller than those for the separate tests.

Fig. 14 has been plotted from Tables V. and VI. It will be noted that the solid specimens group themselves to the right for the reason that their calculated stresses are too large—*i.e.* more than the real stresses. Making use of a former result of one of the authors, that the stresses calculated for solid specimens (using formulæ true only for perfectly elastic conditions) are about 15 per cent. too high, the stresses of the *solid* specimens have been reduced in the ratio $\frac{100}{115}$, and the reduced values have been plotted against the greatest strain difference in fig. 15. In fig. 15 the points corresponding to the *hollow* specimens appear in the same positions with respect to the axes as in fig. 14. Fig. 15 illustrates also the relation between the magnitudes of the maximum shear stresses of the differently-tested specimens.

VII.

On Some Problems Relating to the Design of High-Speed Discs.

By R. V. Southwell, *of the National Physical Laboratory.*

Recent investigations by Prof. H. Lamb and by the present author [1] appeared to have some bearing on the practical problem of vibrations in turbine discs,[2] and the work has accordingly been continued at the National Physical Laboratory, with the assistance of Miss B. S. Gough. The following summary of the progress made has been written in the hope that discussion will reveal the directions in which further extension could be most usefully directed. The accuracy of the calculations depends in every instance upon the validity of the theory of thin plates, as applied to the problems treated; and since the limitations of this theory are not as yet fully understood, comparative experiments will be of the greatest value.

[1] 'The Vibrations of a Spinning Disc,' *Proc. Roy. Soc.* (A), vol. 99 (1921), pp. 272-280.

[2] *Cf.* K. Baumann, 'Some Recent Developments in Large Steam Turbine Practice,' *Jour. Inst. Elect. Eng.* (1921).

The following problems have been considered, in the belief that they are the most important which confront the designer :—

I. The calculation of Centrifugal Stresses.
II. The calculation of Transverse Deflections in Diaphragms and Discs.
III. The calculation of Critical Speeds (or of the Normal Frequencies of Free Transverse Vibration) in Diaphragms and Discs.

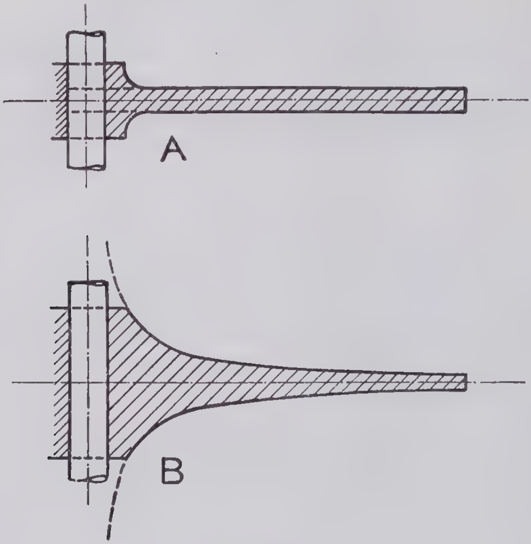


Fig. 16.

Two types of disc have been considered, as shown in the above figure : type A is a disc of uniform thickness, and type B a disc of which the thickness varies as some (negative) power of the radial distance. The fact that theory demands an infinite thickness at the axis is not a practical objection, since an actual disc is bored to receive a shaft of finite diameter, and allowance for this circumstance can be made by means of an auxiliary stress-system.

Problem I. Calculation of Centrifugal Stresses in a Disc of Specified Profile.

The analysis of these stresses, in a disc of uniform thickness, has been worked out by C. Chree,[3] and a discussion of the problem, with an extension to discs of type B, is contained in A. Morley's *Strength of Materials*.[4] Recent investigations indicate that the centrifugal stresses in a disc of any specified profile can be determined without much difficulty by means of graphical methods, but it is doubtful whether these methods are urgently needed by the designer, who will probably be content with analysis which is applicable to discs of type B.

Strictly speaking, the loading due to the blades will not be distributed uniformly along the rim, but will vary from point to point. If the stress-distribution near the roots of the blades is a matter of practical importance, it will be desirable to investigate this auxiliary stress system : the requisite analysis should not prove difficult, for since the blades are uniformly spaced, we may conveniently analyse the load system, as regards its variation along the circumference, into Fourier components, and of these it is probable that only two or three will be important.

[3] *Proc. Camb. Phil. Soc.*, vol. vii. (1891), part iv.
[4] Chap. xi.

Problem II. Calculation of the Transverse Deflections Induced in a Disc by a Specified Load System.

Two cases of this problem arise:—

(*a*) when the disc is stationary;
(*b*) when the disc is rotating.

It is evident that rotation must increase the effective flexural rigidity of the disc, since work has to be done against the centrifugal stresses by any agency which produces transverse deflections.

Under the heading (*a*), we have exact solutions for the deflection of a disc of uniform thickness (type A), under various systems of transverse load which are symmetrical about its axis.[5] Recent investigation has shown that the analysis can be extended without difficulty to any system of transverse loading which varies in intensity as some power of the radial distance, and that a further extension may be made to discs of type B. Further, in this problem also it appears that graphical methods can be employed to find the deflection due to *any* load system (axial symmetry only being postulated), in a disc of *any* specified profile : but here again it seems doubtful whether any real demand exists for methods of such generality.

Under the heading (*b*), we have to consider the effect of centrifugal force, and the difficulty confronts us that the form of the deflected disc, as well as the absolute magnitude of the deflections, will in general be altered by the rotation : had a change in magnitude been the only effect, we could very easily have determined its amount. But since the centrifugal forces will tend to reduce the deflections, the exact magnitude of their effect is of less practical interest than would have been the case if it had increased the danger of rubbing between fixed and moving parts, and in view of the analytical difficulties associated with an exact solution, it will probably be sufficiently accurate to start with the exact solution for a non-rotating disc; to calculate the additional transverse loading which would be required to maintain this deflection against the centrifugal stresses corresponding to the specified speed of rotation; and to estimate a mean coefficient whereby this may be represented as a fractional addition to the original loading : we shall then have an idea of the centrifugal effect, expressed roughly in the form of a fractional addition to the flexural rigidity.

Problem III. Calculation of Critical Speeds (or of the Normal Frequencies of Free Transverse Vibration) in a Disc of Specified Profile, Stationary or Rotating.

There is evidence that resonance is liable to occur between the frequencies of free vibration which are natural to the turbine disc and the frequencies of the small periodic disturbing forces which come into play when the turbine is running. The designer's problem is to arrange that such resonance shall not occur, at all events within the practical speed range, and evidently there are, theoretically speaking, two ways of effecting this result : either he must eliminate the periodic disturbing forces, or he must arrange that their frequencies shall be less than any of the frequencies natural to the disc. In practice, the disturbing forces are not always avoidable, and their origin is sometimes obscure : but their nature can best be guessed from the nature and frequency of the vibration which they excite, and thus our most important problem is the determination of the natural modes and frequencies of free transverse vibration, for a disc of any specified form.

As in problem II, a distinction must be drawn between the effects of the flexural rigidity and of the effective rigidity induced by rotation, and the necessity for taking rotation into account is much greater in the present instance : but it fortunately happens that the requisite allowance for it can be made much more easily. From experience gained in similar problems we may expect that the nature of the vibrations will be different according as the disc is entirely free or to some extent constrained, and that the former conditions will yield the simpler solutions.

[5] *Cf.* A. E. H. Love, *Theory of Elasticity*, §314 (*a*); and A. Morley, *Strength of Materials*, chap xiii.

The free transverse vibrations of a disc of uniform thickness, entirely unconstrained, were investigated by Kirchhoff in 1859.[6] The normal modes are characterised by concentric nodal circles and by equally spaced nodal diameters : for any given number (s) of nodal diameters, the frequency rises with the number (n) of the nodal circles, and in regard to the higher frequencies it is found that the effect of increasing n by unity is approximately the same as that of increasing s by two. The frequency of the free vibration depends to a slight extent upon the value of Poisson's Ratio, of which a representative value for steel is 0.3.

Kirchhoff's solution fails in respect of those types of vibration in which the number of nodal diameters is unity or zero, when the disc is attached at its centre to a shaft and rotor of inertia sufficient to prevent any change in the position or slope of the disc at that point. The necessary modifications have now been investigated, and the graver frequencies of vibration calculated for all types of vibration possible to a non-rotating disc.[7] It is found that substantially exact values could have been obtained with much simpler analysis, by using a method due to the late Lord Rayleigh, in which the *type* of the deflection occurring in the vibration is assumed, and an estimate of the frequency derived from the corresponding expressions for the potential and kinetic energies. The method is particularly valuable, in that it may be applied with equal accuracy (assuming the validity of the theory of thin plates) to discs of curved profile : we are thus in a position to calculate the graver frequencies of vibration for any given disc.

The effects of rotation, in a disc of uniform thickness, were discussed by Prof. Lamb and the present author in the paper to which reference has been made above. When the disc is stressed by centrifugal forces, its effective flexural rigidity is, as we have seen, increased : even though it were practically a membrane, without any flexural rigidity of its own, it would vibrate with finite periods under the restoring effect of the centrifugal system. The problem first considered, therefore, was that of finding the frequencies of free transverse vibration when the centrifugal system acts alone, and this was solved without difficulty in the case of a uniform disc. The modes are generally similar in their nature to those of the non-rotating disc, and the frequency, in any given mode, varies directly as the speed of rotation. No alteration in frequency is entailed by constraints acting at the centre.

In the general case, where vibration occurs under a restoring system to which both the flexural rigidity and the centrifugal stresses contribute, an exact solution would be very difficult to obtain, principally for the reason that the *type* of the vibration will itself change with ω. But the need for an exact solution is obviated by Lord Rayleigh's theorem, that a small error in the assumed type of vibration, when the frequency is estimated by his method, leads to an error in the frequency which will be of the second order; in fact, there is no need to do any further calculation whatever, since it can be shown that the gravest frequency corresponding to any definite number of nodal diameters will be given, with close approximation, by an expression of the form

$$p^2=p_1^2+p_2^2,$$

where p_1 is the frequency found by neglecting the rotation, and p_2 is the frequency found by neglecting the flexural rigidity.

It has been shown, further, that the gravest frequency will be *underestimated* by the formula just given. This result suggests a very simple method for investigating the gravest frequencies natural to a disc of curved profile : for the method of Lord Rayleigh may be applied with equal accuracy to the calculation both of p_1^2 and of p_2^2, and in each case, if the gravest frequency is under investigation, it will give figures for these quantities which are either exact or over-estimated. Thus the two errors involved in these approximate methods tend to cancel one another as regards their effect on the estimated value of p^2, and a closely approximate result may be expected.

[6] 'Ueber die Schwingungen einer elastischen Scheibe,' *Crelle's Journal*, vol. 40 (1850), and *Pogg. Ann.*, vol. 81 (1850). The essentials of the analysis are reproduced in Lord Rayleigh's *Theory of Sound*, §§218, 219.

[7] It is these graver frequencies which are of primary importance, since vibrations of high frequency will in practice be eliminated by air damping, etc.

VIII.

On the Stability of a Rotating Shaft, Subjected Simultaneously to End Thrust and Twist.

By R. V. SOUTHWELL *and* BARBARA S. GOUGH, *of the National Physical Laboratory.*

§1. Nearly forty years ago, Sir George Greenhill worked out the criterion of stability for a long shaft, 'simply supported' (*i.e.*, by journals which impose no constraint upon its direction) at each end, and subjected simultaneously to end thrust and twist.[1] His results showed that the influence of twist is relatively unimportant, and may, for most practical purposes, be neglected : the criterion then reduces to Euler's familiar formula for the critical load of a free-ended shaft.

When the shaft is not subjected to twist, but revolves, end thrust may combine with the rotation in bringing about instability of another type, which reveals itself as a tendency to 'whirl.' The two factors in this result are far more comparable in importance, and the criterion of instability (fortunately simple) must be used in its complete form.[2] The question then arises—when end thrust, twist and rotation act simultaneously, as will often occur in practice, are we justified by Greenhill's result in neglecting the influence of twist, or must we allow for a tendency on the part of the

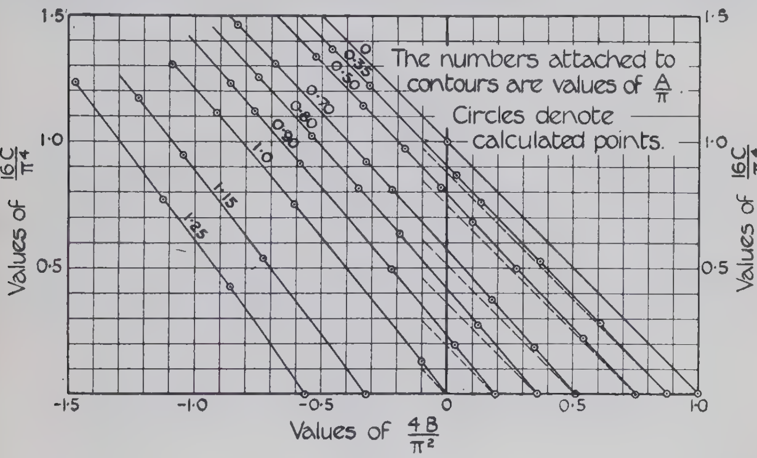


FIG. 17.

rotation to combine with the twist on more or less equal terms, as it combined with the thrust in the second of the problems just described ?

§2. An answer to this question is afforded by the accompanying diagrams. Instability will occur when a certain relation obtains between the thrust P, the rotational speed ω and the torque T, and the stability criterion is exhibited graphically by means of curves which connect critical values of the first two factors for a series of values of the third. The contours given in the figures are the results of calculation: additional contours may be obtained from these, if required, by cross-plotting and interpolation.

Fig. 17 relates to a shaft of which the ends are 'simply supported,' and fig. 18 to an 'encastré' shaft, in which change of direction, as well as displacement, is prevented

[1] *Proc. Inst. Mech. Eng.*, 1883, pp. 182–225. The case of clamped ends was also treated, with results which do not agree with the calculations of this paper : the explanation appears to be that the condition of zero displacement at either end was not realised. Some discussion was also given of the general equations employed in this paper, but solutions were not obtained.

[2] An investigation of this problem is given by A. Morley, 'Strength of Materials,' §166. It was briefly discussed by Greenhill in p. 208 of the paper referred to. *Vide* equation (18) of the present paper.

at the ends. The co-ordinates actually employed are non-dimensional quantities, A, B and C, related to the dimensions and material of the shaft, and to P, ω and T, thus:—

$$\left.\begin{aligned} A &= \frac{Tl}{2EI}, \\ B &= \frac{Pl^2}{4EI} \\ \text{and } C &= \frac{W\omega^2 l^4}{16gEI}, \end{aligned}\right\} \qquad (1)$$

where

l denotes the length of the shaft between bearings,
EI denotes the flexural rigidity of the shaft,
and $\frac{W}{g}$ denotes the mass of the shaft per unit length.

Any self-consistent system of units may be employed.

The diagrams have been extended into the region in which B is negative, for the purpose of illustrating the effect of *end tension* in maintaining stability. Negative values of C are of course inadmissible, since they correspond to imaginary values of

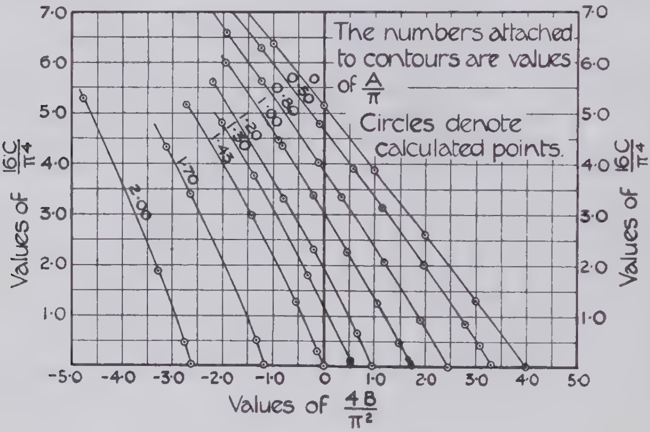


FIG. 18.

ω, and the sign of A (*i.e.*, of the torque) is immaterial. In fig. 17 dotted lines have been drawn to represent, for values of A corresponding to the calculated contours, the contours which would be given by the formula

$$\left(\frac{A}{\pi}\right)^2 + \frac{4B}{\pi^2} + \frac{16C}{\pi^4} = 1 \qquad (2)$$

This formula can be shown to be exact when either A or C is zero, and the close agreement between the two sets of contours shows it to be a very close approximation in all cases where B is positive. A similar formula, with the terms on the left hand side altered by the addition of coefficients 0·49, 0·25 and 0·195 respectively, represents, with rather less close approximation, the results of fig. 18.

§3. Perhaps the most striking feature of the problem is the relative complexity of the analysis which leads to such simple results. Although shown by actual comparison to be closely equivalent to a formula of the type (2), the exact relation obtaining in either case between A, B and C is of a highly transcendental nature, and its interpretation entails much laborious calculation.[3] For practical purposes, the diagrams will be quite sufficiently accurate, and more convenient than

[3] The authors' thanks are due to Miss S. W. Skan for her valuable assistance in this work.

algebraic formulæ of the type (2), besides applying over a wider range; but the establishment of these formulæ enables us to assert that the relative importance of end thrust, rotation and twist is of the same order, when the three actions exist simultaneously, as it has been shown to be when they are taken as acting in pairs: that is to say, any torque which a shaft of normal dimensions can sustain without damage to the material will be negligible in its effect on the stability of that shaft.

§4. *Equations of Neutral Equilibrium.*—In considering the stability of the system, we begin by assuming that the shaft is initially straight and centrally loaded, but that conditions of neutral stability are eventually reached under which the shaft can be held in equilibrium in a configuration of slight distortion, the centre-line taking a curved form, either plane or tortuous. The influence of gravity is neglected. We take the origin of co-ordinates at the central section of the shaft, the axis Oz coinciding with the axis of the shaft in the unstrained configuration; and we choose axes Ox, Oy, *which rotate with the shaft,* perpendicular to each other and to Oz. We define the distorted centre-line by the co-ordinates x, y and z, relative to these axes, of a point P on the centre-line which was originally distant s from the origin O.

The bending of the shaft at any section P is in the osculating plane, at P, to the curved centre-line; and it is produced by a couple, M, of which the axis is parallel to the binormal at P. The direction-cosines of the binormal are

$$\left.\begin{array}{l} \rho\left(\dfrac{dy}{ds}\cdot\dfrac{d^2z}{ds^2}-\dfrac{dz}{ds}\cdot\dfrac{d^2y}{ds^2}\right), \\[2ex] \rho\left(\dfrac{dz}{ds}\cdot\dfrac{d^2x}{ds^2}-\dfrac{dx}{ds}\cdot\dfrac{d^2z}{ds^2}\right) \\[2ex] \text{and } \rho\left(\dfrac{dx}{ds}\cdot\dfrac{d^2y}{ds^2}-\dfrac{dy}{ds}\cdot\dfrac{d^2x}{ds^2}\right), \\[2ex] \text{where } \rho, \text{ the radius of curvature,} = \mathrm{EI}/\mathrm{M}\,; \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and the couple M can therefore be resolved into component couples, with axes parallel to Ox, Oy, Oz respectively, of magnitudes

$$\mathrm{EI}\left(\frac{dy}{ds}\cdot\frac{d^2z}{ds^2}-\frac{dz}{ds}\cdot\frac{d^2y}{ds^2}\right),$$

$$\mathrm{EI}\left(\frac{dz}{ds}\cdot\frac{d^2x}{ds^2}-\frac{dx}{ds}\cdot\frac{d^2z}{ds^2}\right)$$

$$\text{and } \mathrm{EI}\left(\frac{dx}{ds}\cdot\frac{d^2y}{ds^2}-\frac{dy}{ds}\cdot\frac{d^2x}{ds^2}\right).$$

Now, in considering the stability of the straight shaft, we are concerned only with a configuration of infinitesimal distortion, and we may therefore regard x and y as small quantities of the first order. Neglecting small quantities of the second order, we may take dz/ds as unity, and reduce the expressions for the component couples to the simpler forms

$$-\mathrm{EI}\frac{d^2y}{ds^2},\ \ \mathrm{EI}\frac{d^2x}{ds^2} \text{ and } 0.$$

Again, the direction-cosines of the tangent to the strained centre-line at the point P are (if we again neglect small quantities of the second order)

$$\frac{dx}{ds},\ \frac{dy}{ds} \text{ and } 1.$$

Thus, if T′ is the torsional couple acting on the shaft at the section P, it may be resolved into components, with axes parallel to Ox, Oy, Oz respectively, of magnitudes

$$\mathrm{T}'.\frac{dx}{ds},\ \mathrm{T}'.\frac{dy}{ds} \text{ and } \mathrm{T}'.$$

The total magnitudes of the component couples which act at any section are, therefore,

$$T'\frac{dx}{ds} - EI\frac{d^2y}{ds^2}, \quad T'\frac{dy}{ds} + EI\frac{d^2x}{ds^2} \text{ and } T'.$$

These component couples represent actions produced by the stresses across the section P upon that part of the shaft which lies between O and P. In addition, the stresses will produce component forces, in the direction of the axes Ox, Oy, Oz, which we may represent by X, Y and Z. Now the component accelerations, due to rotation, of an element of the shaft situated at P are

$$-\frac{W}{g}\omega^2 x, \quad -\frac{W}{g}\omega^2 y \text{ and } O,$$

in the directions Ox, Oy, Oz respectively; hence, the equations of equilibrium for the element are

$$\frac{dX}{ds} + \frac{W}{g}\omega^2 x = O, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$\frac{dY}{ds} + \frac{W}{g}\omega^2 y = O, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\frac{dZ}{ds} = O, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$\frac{d}{ds}\left(T'\frac{dx}{ds} - EI\frac{d^2y}{ds^2}\right) - Y + Z\frac{dy}{ds} = O. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\frac{d}{ds}\left(T'\frac{dy}{ds} + EI\frac{d^2x}{ds^2}\right) + X - Z\frac{dx}{ds} = O. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$\text{and } \frac{dT'}{ds} - X\frac{dy}{ds} + Y\frac{dx}{ds} = O. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

From (4) and (5) it is evident that X and Y are small quantities of the first order. Hence, neglecting small quantities of the second order in (9), we may write

$$\frac{dT'}{ds} = O,$$

whence

$$T' = \text{const.} = T \text{ (by end conditions)}. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

We have similarly, from (6),

$$Z = \text{const.} = -P \text{ (by end conditions)}; \quad . \quad . \quad . \quad . \quad . \quad (11)$$

and on substituting from (10) and (11) in (7) and (8), differentiating, and substituting for $\frac{dX}{ds}$ and $\frac{dY}{ds}$ from (4) and (5), we obtain, finally, as the equations of neutral stability,

$$\frac{d^2}{ds^2}\left(T\frac{dx}{ds} - EI\frac{d^2y}{ds^2}\right) + \frac{W}{g}\omega^2 y - P\frac{d^2y}{ds^2} = O, . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

and

$$\frac{d^2}{ds^2}\left(T\frac{dy}{ds} + EI\frac{d^2x}{ds^2}\right) - \frac{W}{g}\omega^2 x + P\frac{d^2x}{ds^2} = O. \quad . \quad . \quad . \quad . \quad . \quad (13)$$

These two equations replace the six equations (4)–(9).

§5. *Special Case of Zero Torque.*—When T is zero, a solution of (13) may be obtained by assuming that $x = O$ identically. Equation (12) then reduces to

$$EI\frac{d^4y}{ds^4} + P\frac{d^2y}{ds^2} - \frac{W\omega^2}{g}y = O, \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

and a solution is obtainable in the form

$$y = K_1 \sin \alpha s + K_2 \cos \alpha s + K_3 \sinh \beta s + K_4 \cosh \beta s, \quad . \quad . \quad . \quad (15)$$

where K_1, K_2, K_3, K_4 are arbitrary constants, and where

$$\left.\begin{aligned} \alpha^2 &= \sqrt{\frac{P^2}{4E^2I^2} + \frac{W\omega^2}{gEI}} + \frac{P}{2EI}, \\ \beta^2 &= \sqrt{\frac{P^2}{4E^2I^2} + \frac{W\omega^2}{gEI}} - \frac{P}{2EI}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (16)$$

The values of K_1, K_2, K_3, K_4—or rather, their *relative magnitudes*—are determinable from the end conditions of the problem. But since they cannot all vanish simultaneously (for the shaft would then remain straight), we have only three ratios at our choice for satisfying four specified conditions; hence, by elimination of the four constants, we obtain the criterion of neutral stability.

It will be convenient in this section of the paper to take our origin at one end. Then, when the ends are simply supported we have the relations

$$\left.\begin{aligned} y &= 0, \\ \text{and } \frac{d^2y}{ds^2}\left(=\frac{M}{EI}\right) &= 0, \end{aligned}\right\} \text{when } s = 0 \text{ or } l,$$

and substituting from (15) we find that

$$K_2 = K_3 = K_4 = 0,$$
$$K_1 \sin \alpha l = 0.$$

It follows that we must have

$$\sin \alpha l = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

and, by (16), the criterion for neutral stability in this case is

$$\frac{\pi^2}{l^2} = \frac{P}{2EI} + \sqrt{\frac{P^2}{4E^2I^2} + \frac{W\omega^2}{gEI}},$$

or

$$\frac{Pl^2}{\pi^2EI} + \frac{W\omega^2l^4}{\pi^4 gEI} = 1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

When the ends are clamped, so that we have the relations

$$y = \frac{dy}{ds} = 0, \text{ when } s = 0 \text{ or } l,$$

we find, on substitution from (15), that

$$\left.\begin{aligned} K_2 + K_4 &= 0, \\ \alpha K_1 + \beta K_3 &= 0, \end{aligned}\right\}$$

so that

$$y = K_1 \left(\sin \alpha s - \frac{\alpha}{\beta} \sinh \beta s\right) + K_2 (\cos \alpha s - \cosh \beta s).$$

The criterion then takes the form

$$\begin{vmatrix} \sin \alpha l - \frac{\alpha}{\beta} \sinh \beta l, & \cos \alpha l - \cosh \beta l, \\ \cos \alpha l - \cosh \beta l, & -\left(\sin \alpha l + \frac{\beta}{\alpha} \sinh \beta l\right), \end{vmatrix} = 0,$$

and on expanding the determinant we obtain, finally, as the criterion for neutral stability,

$$2(1 - \cos \alpha l \cosh \beta l) = \frac{\alpha^2 - \beta^2}{\alpha\beta} \sin \alpha l \sinh \beta l,$$

α and β being given by (16), so that . . . . (19)

$$\alpha^2 l^2 = 2\left[\sqrt{B^2 + 4C} + B\right],$$
$$\beta^2 l^2 = 2\left[\sqrt{B^2 + 4C} - B\right],$$

where B and C are the quantities defined in equation (1).

§6. *General Case.*—Reverting to equations (12) and (13), and eliminating x or y, we obtain the equation

$$\left[T^2\frac{d^6}{ds^6}+\left(EI\frac{d^4}{ds^4}+P\frac{d^2}{ds^2}-\frac{W}{g}\omega^2\right)^2\right]x,\,y=0. \qquad (20)$$

The solution may be written in the form

$$(x,y)=\Sigma[A_1\sin\lambda_1 s+B_1\cos\lambda_1 s],$$

where λ_1 λ_2, . . . etc., are the roots of the equation

$$\left(EI\lambda^4-P\lambda^2-\frac{W}{g}\omega^2\right)^2-T^2\lambda^6=0.$$

Writing this equation in the form

$$EI\lambda^4-P\lambda^2-\frac{W}{g}\omega^2=\pm T\lambda^3,$$

we see that the roots obtained by taking the negative sign on the right-hand side are equal and opposite in sign to those which are obtained by taking the positive sign. The most general expression for x is therefore given by

$$\begin{aligned}x&=A_1\sin\lambda_1 s+A_2\sin\lambda_2 s+A_3\sin\lambda_3 s+A_4\sin\lambda_4 s\\&+B_1\cos\lambda_1 s+B_2\cos\lambda_2 s+B_3\cos\lambda_3 s+B_4\cos\lambda_4 s,\end{aligned} \qquad (21)$$

where λ_1, λ_2, λ_3, λ_4 are roots of the equation

$$EI\lambda^4-T\lambda^3-P\lambda^2-\frac{W}{g}\omega^2=0\,; \qquad (22)$$

and the corresponding expression for y can be obtained from (13) in the form

$$\begin{aligned}y&=B_1\sin\lambda_1 s+B_2\sin\lambda_2 s+B_3\sin\lambda_3 s+B_4\sin\lambda_4 s\\&-A_1\cos\lambda_1 s-A_2\cos\lambda_2 s-A_3\cos\lambda_3 s-A_4\cos\lambda_4 s.\end{aligned} \qquad (23)$$

These expressions are legitimate whether λ_1, λ_2, λ_3, λ_4 be real, imaginary, or complex.

The Criterion for 'Simply Supported' Ends.—On the assumptions (a) that the journals prevent movement of the ends of the shaft, but without constraining the *direction* of the bent centre-line, and (b) that the twisting couple applied at the ends of the shaft has its axis parallel to Oz, the terminal conditions may be expressed as follows:—

$$\left.\begin{aligned}x&=0,\\y&=0,\\T\frac{dy}{ds}+EI\frac{d^2x}{ds^2}&=0,\\T\frac{dx}{ds}-EI\frac{d^2y}{ds^2}&=0,\end{aligned}\right\}\text{when } s=\pm\frac{l}{2}. \qquad (24)$$

The last two equations represent the condition that at the ends the resultant couple on the shaft has Oz for its axis.

If we substitute for x and y from (21) and (23), these conditions yield the relations

$$A_1\sin\lambda_1\frac{l}{2}+A_2\sin\lambda_2\frac{l}{2}+A_3\sin\lambda_3\frac{l}{2}+A_4\sin\lambda_4\frac{l}{2}=0, \qquad (25)$$

$$A_1\cos\lambda_1\frac{l}{2}+A_2\cos\lambda_2\frac{l}{2}+A_3\cos\lambda_3\frac{l}{2}+A_4\cos\lambda_4\frac{l}{2}=0, \qquad (26)$$

$$\begin{aligned}&(EI\lambda_1^2-T\lambda_1)A_1\sin\lambda_1\frac{l}{2}+(EI\lambda_2^2-T\lambda_2)A_2\sin\lambda_2\frac{l}{2}\\&+(EI\lambda_3^2-T\lambda_3)A_3\sin\lambda_3\frac{l}{2}+(EI\lambda_4^2-T\lambda_4)A_4\sin\lambda_4\frac{l}{2}=0,\end{aligned} \qquad (27)$$

$$(EI\lambda_1^2 - T\lambda_1) A_1 \cos \lambda_1 \frac{l}{2} + (EI\lambda_2^2 - T\lambda_2) A_2 \cos \lambda_2 \frac{l}{2}$$
$$+ (EI\lambda_3^2 - T\lambda_3) A_3 \cos \lambda_3 \frac{l}{2} + (EI\lambda_4^2 - T\lambda_4) A_4 \cos \lambda_4 \frac{l}{2} = 0, \quad . \quad . \quad . \quad (28)$$

and a similar set of four relations, which may be obtained by writing $B_1, B_2, \ldots$ for $A_1, A_2, \ldots$ in the above. Either set of four relations obviously yields the same equation in l, if we eliminate the coefficients $A_1, A_2, \ldots$ or $B_1, B_2, \ldots$

Subtracting (25), multiplied by P, from (27), and remembering that, by (22),

$$EI\lambda^2 - T\lambda - P = \frac{W}{g} \cdot \frac{\omega^2}{\lambda^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

when λ has any of the values $\lambda_1, \lambda_2, \lambda_3, \lambda_4$, we see that for (27) we may substitute the relation

$$\frac{A_1}{\lambda_1^2} \sin \lambda_1 \frac{l}{2} + \frac{A_2}{\lambda_2^2} \sin \lambda_2 \frac{l}{2} + \frac{A_3}{\lambda_3^2} \sin \lambda_3 \frac{l}{2} + \frac{A_4}{\lambda_4^2} \sin \lambda_4 \frac{l}{2} = 0. \quad . \quad . \quad (30)$$

Similarly, for (28) we may substitute the relation

$$\frac{A_1}{\lambda_1^2} \cos \lambda_1 \frac{l}{2} + \frac{A_2}{\lambda_2^2} \cos \lambda_2 \frac{l}{2} + \frac{A_3}{\lambda_3^2} \cos \lambda_3 \frac{l}{2} + \frac{A_4}{\lambda_4^2} \cos \lambda_4 \frac{l}{2} = 0. \quad . \quad . \quad (31)$$

Then from (25), (26), (30) and (31), eliminating A_1, A_2, A_3, A_4, we have, as the required criterion for a condition of neutral stability in the free-ended shaft,

$$\begin{vmatrix} \sin \lambda_1 \frac{l}{2}, & \sin \lambda_2 \frac{l}{2}, & \sin \lambda_3 \frac{l}{2}, & \sin \lambda_4 \frac{l}{2}, \\ \cos \lambda_1 \frac{l}{2}, & \cos \lambda_2 \frac{l}{2}, & \cos \lambda_3 \frac{l}{2}, & \cos \lambda_4 \frac{l}{2}, \\ \frac{1}{\lambda_1^2} \sin \lambda_1 \frac{l}{2}, & \frac{1}{\lambda_2^2} \sin \lambda_2 \frac{l}{2}, & \frac{1}{\lambda_3^2} \sin \lambda_3 \frac{l}{2}, & \frac{1}{\lambda_4^2} \sin \lambda_4 \frac{l}{2}, \\ \frac{1}{\lambda_1^2} \cos \lambda_1 \frac{l}{2}, & \frac{1}{\lambda_2^2} \cos \lambda_2 \frac{l}{2}, & \frac{1}{\lambda_3^2} \cos \lambda_3 \frac{l}{2}, & \frac{1}{\lambda_4^2} \cos \lambda_4 \frac{l}{2} \end{vmatrix} = 0. \quad . \quad . \quad (32)$$

The expansion of the determinant on the left-hand side of (32) is

$$\frac{\operatorname{cosec} \lambda_1 \frac{l}{2}}{\lambda_1^2 \lambda_2^2 \lambda_3^2 \lambda_4^2} \Big[(\lambda_1^2 - \lambda_2^2)(\lambda_3^2 - \lambda_4^2) \sin \lambda_2 \frac{l}{2} \sin (\lambda_1 - \lambda_3) \frac{l}{2} \sin (\lambda_1 - \lambda_4) \frac{l}{2}$$
$$- (\lambda_1^2 - \lambda_3^2)(\lambda_2^2 - \lambda_4^2) \sin \lambda_3 \frac{l}{2} \sin (\lambda_1 - \lambda_2) \frac{l}{2} \sin (\lambda_1 - \lambda_4) \frac{l}{2}$$
$$+ (\lambda_1^2 - \lambda_4^2)(\lambda_2^2 - \lambda_3^2) \sin \lambda_4 \frac{l}{2} \sin (\lambda_1 - \lambda_2) \frac{l}{2} \sin (\lambda_1 - \lambda_3) \frac{l}{2} \Big],$$

and since

$$(\lambda_1^2 - \lambda_4^2)(\lambda_2^2 - \lambda_3^2) = (\lambda_1^2 - \lambda_3^2)(\lambda_2^2 - \lambda_4^2) - (\lambda_1^2 - \lambda_2^2)(\lambda_3^2 - \lambda_4^2),$$

and the quantity

$$\frac{1}{\lambda_1^2 \lambda_2^2 \lambda_3^2 \lambda_4^2},$$

—which, by (22), is equal to $\left(\frac{gEI}{W\omega^2}\right)^2$—cannot, by hypothesis, be zero, it is easily

shown that the condition (32) may be written in the form

$$\begin{aligned}&(\lambda_1^2-\lambda_2^2)\,(\lambda_3^2-\lambda_4^2)\,\cos\,(\lambda_1+\lambda_2-\lambda_3-\lambda_4)\frac{l}{2}\\ &-(\lambda_2^2-\lambda_3^2)\,(\lambda_4^2-\lambda_1^2)\,\cos\,(\lambda_2+\lambda_3-\lambda_4-\lambda_1)\frac{l}{2}\\ &-(\lambda_1^2-\lambda_3^2)\,(\lambda_2^2-\lambda_4^2)\,\cos\,(\lambda_1+\lambda_3-\lambda_2-\lambda_4)\frac{l}{2}=0, \qquad (33)\end{aligned}$$

which may therefore be taken as the criterion of neutral stability for 'simply supported' ends.

The Criterion for 'Clamped Ends.'—On the assumption that the bearings prevent both displacement and change of direction, the terminal conditions take the form

$$\left.\begin{aligned}x&=0,\\ y&=0,\\ \frac{dx}{ds}&=0,\\ \frac{dy}{ds}&=0,\end{aligned}\right\}\text{when } s=\pm\frac{l}{2}. \qquad (34)$$

Substituting for x and y in these equations from (21) and (23), we obtain the relations

$$A_1\sin\lambda_1\frac{l}{2}+A_2\sin\lambda_2\frac{l}{2}+A_3\sin\lambda_3\frac{l}{2}+A_4\sin\lambda_4\frac{l}{4}=0, \qquad (35)$$

$$A_1\cos\lambda_1\frac{l}{2}+A_2\cos\lambda_2\frac{l}{2}+A_3\cos\lambda_3\frac{l}{2}+A_4\cos\lambda_4\frac{l}{2}=0, \qquad (36)$$

$$\lambda_1A_1\sin\lambda_1\frac{l}{2}+\lambda_2A_2\sin\lambda_2\frac{l}{2}+\lambda_3A_3\sin\lambda_3\frac{l}{2}+\lambda_4A_4\sin\lambda_4\frac{l}{2}=0, \qquad (37)$$

$$\lambda_1A_1\cos\lambda_1\frac{l}{2}+\lambda_2A_2\cos\lambda_2\frac{l}{2}+\lambda_3A_3\cos\lambda_3\frac{l}{2}+\lambda_4A_4\cos\lambda_4\frac{l}{2}=0, \qquad (38)$$

and a similar set of four relations, which may be obtained by writing B_1, B_2, . . . for A_1, A_2, . . . in the above. Either set of four relations yields the same equation in l, if we eliminate the coefficients A_1, A_2, . . . or B_1, B_2, . . . etc., and the condition for neutral stability may therefore be written in the form

$$\begin{vmatrix}\sin\lambda_1\frac{l}{2}, & \sin\lambda_2\frac{l}{2}, & \sin\lambda_3\frac{l}{2}, & \sin\lambda_4\frac{l}{2},\\ \cos\lambda_1\frac{l}{2}, & \cos\lambda_2\frac{l}{2}, & \cos\lambda_3\frac{l}{2}, & \cos\lambda_4\frac{l}{2},\\ \lambda_1\sin\lambda_1\frac{l}{2}, & \lambda_2\sin\lambda_2\frac{l}{2}, & \lambda_3\sin\lambda_3\frac{l}{2}, & \lambda_4\sin\lambda_4\frac{l}{2},\\ \lambda_1\cos\lambda_1\frac{l}{2}, & \lambda_2\cos\lambda_2\frac{l}{2}, & \lambda_3\cos\lambda_3\frac{l}{2}, & \lambda_4\cos\lambda_4\frac{l}{2},\end{vmatrix}=0. \qquad (39)$$

Expanding the determinant, we obtain, as the criterion in this instance, the equation

$$\begin{aligned}&(\lambda_1-\lambda_2)\,(\lambda_3-\lambda_4)\,\cos\,(\lambda_1+\lambda_2-\lambda_3-\lambda_4)\,\frac{l}{2}\\ &-(\lambda_2-\lambda_3)\,(\lambda_4-\lambda_1)\,\cos\,(\lambda_2+\lambda_3-\lambda_4-\lambda_1)\,\frac{l}{2}\\ &-(\lambda_1-\lambda_3)\,(\lambda_2-\lambda_4)\,\cos\,(\lambda_3+\lambda_1-\lambda_2-\lambda_4)\,\frac{l}{2}=0, \qquad (40)\end{aligned}$$

in which λ_1, λ_2, λ_3, λ_4 are, as before, the roots of equation (22).

§7. *Equal Roots of Equation (22) not Admissible.*—At first sight it would appear that the conditions (33) and (40) of neutral stability would both be satisfied if equation (22) had equal roots: and real values can be found for P, T and ω^2 which will satisfy this requirement. But if we merely substitute λ_1 for λ_2 (say) in the expressions (21) and (23) for x and y, we shall in effect be reducing the number of arbitrary constants involved in these expressions from eight to six, and it is not difficult to show that the boundary conditions can only be satisfied by making x and y vanish for all values of s: the shaft then remains straight, and, although this is a configuration of equilibrium, it is of no interest for present purposes. If, on the other hand, we write down the complete solution (with eight constants) under the condition of equal roots, we are in effect substituting λ_1 for λ_2 in expressions for x and y which differ from (21) and (23) in having $\sin \lambda_2 s$ and $\cos \lambda_2 s$ replaced by $s \cos \lambda_2 s$ and $s \sin \lambda_2 s$ respectively; but when this latter modification is made in (21) and (23), and the criterion obtained by the same methods as before, it is found to be no longer satisfied by making λ_2 equal to λ_1.

§8. *Solution for 'Simply-supported' Ends.*—We proceed to interpret the solutions (33) and (40) in terms of the physical constants of our problem. Writing μ for $\frac{\lambda l}{2}$, we may throw equation (22) into the form

$$\mu^4 - A\mu^3 - B\mu^2 - C = 0, \qquad (41)$$

where A, B and C are the quantities defined in (1); and we may notice that two of the roots are necessarily real, since the constant term in this equation is negative.

If we express the roots in the form

$$\left.\begin{aligned} \mu_1,\ \mu_2 &= a \pm b, \\ \mu_3,\ \mu_4 &= c \pm d, \end{aligned}\right\} \qquad (42)$$

equation (41) must factorise as follows:—

$$\{\mu^2 - 2\mu a + a^2 - b^2\}\ \{\mu^2 - 2\mu c + c^2 - d^2\} = 0,$$

and we have

$$\left.\begin{aligned} &2(a+c) = A, \\ &a^2 - b^2 + c^2 - d^2 + 4ac = -B, \\ &a(c^2-d^2) + c(a^2-b^2) = 0, \\ &(a^2-b^2)(c^2-d^2) = -C. \end{aligned}\right\} \qquad (43)$$

Again, the criterion (33) may be written as

$$\left.\begin{aligned} &(\mu_1^2-\mu_2^2)(\mu_3^2-\mu_4^2)\sin(\mu_1-\mu_3)\sin(\mu_2-\mu_4) \\ &= (\mu_1^2-\mu_3^2)(\mu_2^2-\mu_4^2)\sin(\mu_1-\mu_2)\sin(\mu_3-\mu_4), \end{aligned}\right\} \qquad (44)$$

and if we substitute from (42) this becomes

$$\begin{aligned} &16abcd \sin(a+b-c-d)\sin(a-b-c+d) \\ &= (a^2+b^2-c^2-d^2+2ab-2cd)(a^2+b^2-c^2-d^2-2ab+2cd)\sin 2b \sin 2d, \\ &= \{8abcd + (a^2-b^2)^2 + (c^2-d^2)^2 - 2(a^2+b^2)(c^2+d^2)\}\sin 2b \sin 2d, \end{aligned}$$

or

$$\begin{aligned} &8abcd\{\cos 2b \cos 2d - \cos 2(a-c)\} \\ &= \{(a^2-b^2)^2 + (c^2-d^2)^2 - 2(a^2+b^2)(c^2+d^2)\}\sin 2b \sin 2d. \end{aligned} \qquad (45)$$

In this form, we can obviously deal with either real or imaginary values of b or d, observing that, if $b = \beta i$,

$$\frac{\sin 2b}{2b} = \frac{\sinh 2\beta}{2\beta},$$

and

$$\cos 2b = \cosh 2\beta$$

The solution of (45) has been obtained by trial, and is exhibited in fig. 17. The procedure adopted is to trace a contour line (T = const.) on a diagram of which the co-ordinates are P and ω^2. We are then, by (43), given the value of $(a+c)$, but can, with this limitation, vary the separate values: choosing any pair of values, we have from the third of (43) a range of corresponding values of b^2 and d^2. The sign is immaterial, and for each pair we calculate the left- and right-hand expressions in (45). Proceeding in this way by trial, when the expressions are equal we know a, b, c, d, and hence B and C, from (43)

It may be noticed that a possible solution of the criterion (45) is given by $a = c = 0$, $2b = n\pi$. This gives

$$A = 0,$$

$$B = d^2 + \frac{n^2\pi^4}{4},$$

$$\frac{d^2 n^2 \pi^2}{4} = -C,$$

and hence

$$\frac{4B}{n^2\pi^2} + \frac{16C}{n^4\pi^4} = 1,$$

or

$$\frac{Pl^2}{n^2\pi^2 EI} + \frac{W\omega^2 l^4}{n^4\pi^4 g EI} = 1,$$

which reduces to (18) when $n = 1$. When T is small, therefore, we may expect a solution round about the value $2b = n\pi$, and for practical purposes n will be 1.

§9. *Solution for Clamped Ends.*—Employing as before the substitutions of (41)-(43), and multiplying (40) throughout by $\frac{l^2}{4}$, we may write this criterion in the form

$$\begin{aligned}
&(\mu_1 - \mu_2)(\mu_3 - \mu_4)\cos(\mu_1 + \mu_2 \quad \mu_3 - \mu_4) \\
&-(\mu_1 - \mu_3)(\mu_2 - \mu_4)\cos(\mu_3 + \mu_1 - \mu_2 - \mu_4) \\
&= (\mu_2 - \mu_3)(\mu_4 - \mu_1)\cos(\mu_2 + \mu_3 - \mu_4 - \mu_1), \\
&= \{(\mu_1 - \mu_2)(\mu_3 - \mu_4) - (\mu_1 - \mu_3)(\mu_2 - \mu_4)\}\cos(\mu_2 + \mu_3 - \mu_4 - \mu_1),
\end{aligned}$$

or

$$\begin{aligned}
&(\mu_1 - \mu_2)(\mu_3 - \mu_4)\sin(\mu_1 - \mu_3)\sin(\mu_2 - \mu_4) \\
&= (\mu_1 - \mu_3)(\mu_2 - \mu_4)\sin(\mu_1 - \mu_2)\sin(\mu_3 - \mu_4), \qquad (46)
\end{aligned}$$

which corresponds with equation (44) of the last section. Hence, if we substitute from (42), we obtain the relation

$$\begin{aligned}
&4bd \sin(a + b - c - d)\sin(a - b - c + d) \\
&= (a + b - c - d)(a - b - c + d)\sin 2b \sin 2d,
\end{aligned}$$

or

$$\begin{aligned}
&2bd\{\cos 2(b - d) - \cos 2(a - c)\} \\
&= \{(a - c)^2 - (b - d)^2\}\sin 2b \sin 2d,
\end{aligned}$$

which may also be written in the form

$$\begin{aligned}
&2bd\{\cos 2b \cos 2d - \cos 2(a - c)\} \\
&= \{(a - c)^2 - b^2 - d^2\}\sin 2b \sin 2d, \qquad (47)
\end{aligned}$$

for convenience in dealing with imaginary values of b or d.

Equation (47) has been solved, like (45), by trial, and the results are exhibited in fig. 18.

The following tables give the numerical results which have been obtained for the purpose of constructing figs. 17 and 18.

TABLE I. SIMPLY-SUPPORTED ENDS.

$A/\pi=0·00$	$4B/\pi^2=$	+1·00	0·00	Relation between A and B is linear, by (18).							
	$16C/\pi^4=$	0·00	+1·00								
$A/\pi=0·35$	$4B/\pi^2=$	+0·8775	+0·603	+0·369	+0·132	+0·037	−0·312	−0·458	—	—	—
	$16C/\pi^4=$	0·000	+0·284	+0·5255	+0·761	+0·864	+1·220	+1·365	—	—	—
$A/\pi=0·50$	$4B/\pi^2=$	+0·750	+0·514	+0·275	+0·098	−0·028	−0·170	−0·335	−0·526	−0·752	−1·240
	$16C/\pi^4=$	0·000	+0·221	+0·496	+0·686	+0·819	+0·976	+1·141	+1·337	+1·573	+2·067
$A/\pi=0·70$	$4B/\pi^2=$	+0·510	+0·342	+0·178	−0·220	−0·323	−0·683	−0·830	—	—	—
	$16C/\pi^4=$	0·000	+0·191	+0·3725	+0·8065	+0·919	+1·309	+1·463	—	—	—
$A/\pi=0·80$	$4B/\pi^2=$	+0·360	+0·121	−0·188	−0·351	−0·538	−0·751	—	—	—	—
	$16C/\pi^4=$	0·000	+0·2715	+0·633	+0·815	+1·020	+1·252	—	—	—	—
$A/\pi=0·90$	$4B/\pi^2=$	+0·190	+0·030	−0·222	−0·586	−0·762	−0·860	—	—	—	—
	$16C/\pi^4=$	0·000	+0·196	+0·494	+0·913	+1·120	+1·228	—	—	—	—
$A/\pi=1·00$	$4B/\pi^2=$	0·000	−0·102	−0·607	−0·914	−1·090	—	—	—	—	—
	$16C/\pi^4=$	0·000	+0·129	+0·750	+1·111	+1·307	—	—	—	—	—
$A/\pi=1·15$	$4B/\pi^2=$	−0·3225	−0·731	−1·045	−1·224	—	—	—	—	—	—
	$16C/\pi^4=$	0·000	+0·539	+0·943	+1·170	—	—	—	—	—	—
$A/\pi=1·25$	$4B/\pi^2=$	−0·5625	−0·858	−1·122	−1·475	—	—	—	—	—	—
	$16C/\pi^4=$	0·000	+0·423	+0·771	+1·233	—	—	—	—	—	—

TABLE II. CLAMPED ENDS.

$A/\pi=0·0$	$4B/\pi^2=$	+4·00	+3·00	+2·00	+1·00	0·00	−1·00	−3·00	—	—	—
	$16C/\pi^4=$	0·00	+1·31	+2·60	+3·87	+5·14	+6·37	+8·85	—	—	—
$A/\pi=0·5$	$4B/\pi^2=$	+3·29	+3·08	+2·77	+1·97	+1·146	+0·57	−0·092	−1·26	−2·36	—
	$16C/\pi^4=$	+0·044	+0·43	+0·84	+2·02	+3·13	+3·89	+4·78	+6·29	+7·70	—
$A/\pi=0·8$	$4B/\pi^2=$	+2·436	+1·896	+1·184	+0·344	−0·125	−1·228	−1·924	—	—	—
	$16C/\pi^4=$	+0·015	+0·92	+2·07	+3·345	+4·03	+5·63	+6·58	—	—	—
$A/\pi=1·00$	$4B/\pi^2=$	+1·732	+1·702	+1·656	+1·480	+1·046	+0·474	−0·212	−0·832	−0·915	−1·959
	$16C/\pi^4=$	+0·014	+0·068	+0·148	+0·476	+1·263	+2·265	+3·37	+4·35	+4·47	+5·99
$A/\pi=1·20$	$4B/\pi^2=$	+0·952	+0·645	−0·206	−0·79	−2·20	—	—	—	—	—
	$16C/\pi^4=$	+0·003	+0·652	+2·305	+3·29	+5·61	—	—	—	—	—
$A/\pi=1·30$	$4B/\pi^2=$	+0·524	−0·324	−1·381	−2·019	—	—	—	—	—	—
	$16C/\pi^4=$	+0·100	+1·796	+3·75	+4·81	—	—	—	—	—	—
$A/\pi=1·43$	$4B/\pi^2=$	0·00	−0·12	−0·55	−1·43	−2·73	—	—	—	—	—
	$16C/\pi^4=$	0·00	+0·285	+1·26	+2·98	+5·16	—	—	—	—	—
$A/\pi=1·70$	$4B/\pi^2=$	−1·19	−1·33	−2·66	−3·125	—	—	—	—	—	—
	$16C/\pi^4=$	+0·003	+0·49	+3·39	+4·31	—	—	—	—	—	—
$A/\pi=2·00$	$4B/\pi^2=$	−2·636	−2·77	−3·28	−4·78	—	—	—	—	—	—
	$16C/\pi^4=$	+0·033	+0·46	+1·871	+5·28	—	—	—	—	—	—

IX.

The Stresses in Cylinders and Pipes with Eccentric Bore.

By G. B. JEFFERY, *M.A.*, *D.Sc.*

In a recent paper[1] the author developed a general method for the solution of two-dimensional elastic problems in which the stresses are given over two circular non-intersecting, non-concentric boundaries. One of the problems which most readily yields to this method of solution is that of a cylinder or pipe, whose internal and external sections are both circular but not concentric, in stress under a uniform hydrostatic pressure either internal or external. The problem is soluble in finite terms and reference may be made to the original paper for a discussion of the stress distribution. These results may be applied to the determination of the diminution in the strength of a pipe or cylinder due to a small error of centring as between the external surface and the bore. If the radius of the bore is r_1 and that of the external surface r_2 and if the distance between their centres is d the maximum stress when the pipe is under an internal pressure P is on the internal surface at the thinnest part if $d < \frac{1}{2}r_1$ and is of magnitude.

$$P\left[\frac{2r_2^2(r_2^2+r_1^2-2r_1d-d^2)}{(r_1^2+r_2^2)(r_2^2-r_1^2-2r_1d-d^2)}-1\right]$$

This depends only upon the ratios of r_1, r_2, d, and we may therefore take α = ratio of mean thickness of cylinder wall to internal diameter, β = ratio of centre distance to mean thickness of cylinder wall, or

$$\alpha=(r_2-r_1)/2r_1 \qquad \beta=d/(r_2-r_1)$$

The maximum stress them becomes

$$P\left[\frac{2(1+2\alpha)^2\{(1+2\alpha)^2+1-4\alpha\beta-4\alpha^2\beta^2\}}{\{1+(1+2\alpha)^2\}\{(1+2\alpha)^2-(1+2\alpha\beta)^2\}}-1\right]$$

In the appended tables the maximum stress in the material is given in lbs. per sq. in. per 1000 lbs. per sq. in. internal pressure for values of α, β lying between 0 and ·2 in each case.

[1] Plane Stress and Plane Strain in Bi-polar Co-ordinates. *Phil. Trans. Royal Society*, Vol. 221 A, p. 265.

TABLE SHOWING THE MAXIMUM STRESS IN A TUBE WITH ECCENTRIC BORE UNDER INTERNAL FLUID PRESSURE IN LBS. PER SQ. IN. PER 1000 LBS. PER SQ. IN. INTERNAL PRESSURE (*continued on next page*).

$\alpha =$	·01	·02	·03	·04	·05	·06	·07	·08	·09	·10
$\beta =$										
·00	50505	25510	17181	13019	10524	8862	7676	6787	6097	5545
·01	51010	25762	17349	13145	10624	8945	7747	6849	6152	5595
·02	51525	26019	17521	13273	10727	9030	7820	6913	6208	5645
·03	52051	26282	17695	13404	10831	9117	7894	6977	6265	5696
·04	52588	26550	17874	13538	10938	9206	7969	7043	6324	5749
·05	53136	26824	18056	13674	11047	9296	8047	7111	6383	5802
·06	53636	27104	18242	13814	11158	9389	8126	7180	6444	5857
·07	54267	27389	18433	13956	11272	9483	8207	7250	6507	5913
·08	54852	27681	18627	14101	11388	9580	8289	7322	6570	5970
·09	55449	27980	18826	14250	11506	9678	8373	7395	6636	6028
·10	56059	28285	19029	14402	11628	9779	8459	7471	6702	6088
·11	56683	28596	19236	14558	11752	9882	8548	7547	6770	6149
·12	57321	28915	19448	14717	11879	9988	8638	7626	6840	6211
·13	57974	29241	19666	14879	12008	10095	8730	7706	6911	6275
·14	58642	29575	19888	15046	12141	10206	8824	7789	6984	6340
·15	59326	29917	20115	15216	12277	10319	8921	7873	7058	6407
·16	60026	30267	20348	15390	12416	10435	9020	7959	7135	6476
·17	60743	30625	20587	15569	12559	10553	9121	8048	7213	6546
·18	61478	30992	20831	15752	12705	10674	9225	8138	7293	6618
·19	62230	31368	21081	15939	12855	10799	9331	8231	7375	6691
·20	63002	31753	21338	16131	13008	10926	9440	8326	7460	6767

Continued from previous page.

$\alpha=$ $\beta=$	·11	·12	·13	·14	·15	·16	·17	·18	·19	·20
·00	5095	4720	4404	4133	3899	3694	3514	3354	3211	3083
·01	5140	4761	4441	4167	3931	3724	3542	3380	3236	3107
·02	5185	4802	4479	4203	3963	3754	3570	3407	3261	3130
·03	5232	4845	4518	4238	3997	3785	3599	3434	3287	3155
·04	5279	4888	4558	4275	4031	3817	3629	3462	3313	3179
·05	5327	4932	4598	4313	4065	3849	3659	3491	3340	3205
·06	5377	4977	4640	4351	4101	3883	3690	3520	3367	3230
·07	5427	5023	4682	4390	4137	3916	3722	3549	3395	3257
·08	5479	5071	4725	4430	4174	3951	3754	3580	3424	3284
·09	5532	5119	4770	4471	4212	3986	3787	3611	3453	3311
·10	5586	5168	4815	4513	4251	4023	3821	3643	3483	3340
·11	5641	5218	4861	4555	4291	4060	3856	3675	3514	3369
·12	5698	5270	4909	4599	4331	4097	3891	3708	3545	3398
·13	5755	5323	4957	4644	4373	4136	3928	3742	3577	3428
·14	5815	5377	5007	4690	4416	4176	3965	3777	3610	3459
·15	5875	5432	5057	4737	4459	4216	4003	3813	3643	3491
·16	5937	5489	5109	4785	4504	4258	4042	3849	3678	3524
·17	6001	5546	5163	4834	4549	4301	4081	3887	3713	3557
·18	6066	5606	5217	4884	4596	4344	4122	3925	3749	3591
·19	6132	5667	5273	4936	4644	4389	4164	3965	3786	3626
·20	6201	5729	5330	4989	4693	4435	4207	4005	3824	3662

The Distribution of Bronze Age Implements.—*Interim Report of the Committee* (Professor J. L. Myres, *Chairman*; Mr. Harold Peake, *Secretary*; Dr. E. C. R. Armstrong, Dr. G. A. Auden, Mr. H. Balfour, Mr. L. H. D. Buxton, Mr. O. G. S. Crawford, Sir W. Boyd Dawkins, Professor H. J. Fleure, Mr. G. A. Garfitt, Dr. R. R. Marrett, Mr. R. Mond, Sir C. H. Read, Sir W. Ridgeway).

The Committee has had throughout the assistance of Dr. H. S. Harrison, representing the Royal Anthropological Institute; and Lord Abercromby, representing the Society of Antiquaries of Scotland.

The Committee's draughtsman, Mr. C. H. Howell, has been employed throughout the year, and has paid several visits to museums and private collections within the Home Counties. Mr. E. Billing completed a few cards that he had in hand, while Mr. C. O. Waterhouse has drawn about 160 specimens in the British Museum.

Among the many volunteers who have assisted us the first place must be given to Mrs. Porter, who has now completed the sketches and measurements of the large collection of bronze implements in the Cambridge Museum, and the next to Miss M. Levin, who has done many of the North Country museums and collections as well as the London Museum, and has placed these and a considerable number of others upon cards. Mr. E. C. Middleton has also drawn most of the specimens in the museums and private collections of the Midland Counties.

Many others have helped in making sketches, including some of the boys at Eton, Harrow, Winchester, Wellington and Tonbridge, who have drawn the specimens in their school museums. We have also received great assistance from the Curators of many museums.

Mr. E. C. R. Armstrong has kindly lent to the Committee the original drawings of his catalogue of gold ornaments; these are being copied on to cards.

London (with the exception of the British Museum), Middlesex, Kent and Surrey are practically finished, Bucks, Herts, Essex and Sussex nearly so, as are Cornwall, Devon, Dorset and Gloucestershire. Northumberland, Durham, Cumberland, and Westmorland have also been finished all but a few specimens in private hands, and the majority of the museums in Yorkshire and the Midland Counties are finished.

The number of cards completed on June 30 was about 4,300, which means that about 2,650 have been completed during the year; besides this, a great number of sketches have been made which have not yet been placed on cards.

It is too soon yet to form an accurate estimate of the number of specimens remaining to be drawn, but if equal progress is made during the coming year it should be possible at its close to give an approximate forecast of the work outstanding.

Towards the close of 1920 the Committee issued an appeal for funds, in response to which sufficient money has been received to enable them to continue the work.

The following sums have been received:—

	£	*s.*	*d.*
The British Association	100	0	0
The Royal Society (Gore Fund)	40	0	0
The Society of Antiquaries	10	0	0
The Earl Iveagh	10	0	0
Henry Wellcome, Esq.	10	0	0
R. F. Nicholson, Esq.	10	0	0
The Lord Abercromby	5	0	0
H. Gordon Selfridge, Esq.	5	0	0
A. Colegate, Esq.	5	0	0
C. Martin, Esq.	2	2	0
G. Cadbury, Esq., jun.	2	0	0
Col. W. L. Morgan	1	1	0
Col. E. Kitson Clark	1	1	0
James Booth, Esq.	1	1	0
W. Heward Bell, Esq.	1	0	0
E. C. R. Armstrong, Esq.	1	0	0
Frank R. East, Esq.	0	10	0
Carried forward	204	15	0

	£	s.	d.
Brought forward	204	15	0
Dr. R. Macalister	0	10	0
T. G. Barnet, Esq.	0	10	0
R. Garroway Rice, Esq.	0	5	0
	206	0	0
Balance from last year	78	0	10
Total receipts	£284	0	10

The expenditure has been mainly on draughtsmen :—

	£	s.	d.	£	s.	d.
C. H. Howell, salary	156	0	0			
C. O. Waterhouse, at piece rates	15	4	0			
E. Billing	3	19	0			
				175	3	0
C. H. Howell, expenses	37	3	6			
E. Billing	0	8	3			
E. C. Middleton	1	17	6			
				39	9	3
W. J. Butler, boxes for cards	2	7	0			
Edwards and Godding, calipers	0	11	3			
				2	18	3
R. Midgeley, printing appeal	2	3	6			
Cheque-books	0	12	6			
				2	16	0
Total expenditure				£220	6	6

SUMMARY.

	£	s.	d.
Total receipts	284	0	10
Total expenditure	220	6	6
Balance June 30	£63	14	4

It is estimated that the expenditure for the coming year will not be less, as the draughtsmen's expenses are likely to be higher; so that about 250*l*. more will be required to continue the work until the autumn of 1922.

Training in Citizenship.—*Report of the Committee* (Right Rev. Bishop WELLDON, *D.D.*, *Chairman*; Lady SHAW, *Secretary*; Lieutenant-General Sir ROBERT BADEN-POWELL, Mr. C. H. BLAKISTON, Mr. G. D. DUNKERLEY, Mr. W. D. EGGAR, Mr. MAXWELL GARNETT, *C.B.E.*, Sir RICHARD GREGORY, Mr. SPURLEY HEY, Miss E. P. HUGHES, *LL.D.*, Sir THEODORE MORISON).

INTRODUCTION.

THE response from educational authorities to the questionnaire sent out by the Committee on Training in Citizenship has not been so complete as the Committee hoped. The selections from the replies, however, contained in the two reports of 1920 and 1921 give evidence of considerable interest in the subject and variety in the method of dealing with it.

The Committee regards Sir Robert Baden-Powell's Boy Scout and Girl Guide Associations as the most effective practical training on the social side; the isolated associations organised in some schools lack the democratic training of the public organisation.

The Missions supported by some schools, though valuable as practical evidence of the different modes and possibilities of life amongst young people, require very careful handling if they are not to inculcate a conscious philanthropy that is destructive of democratic citizenship. The free mingling in games and other competitions of boys and girls from neighbouring schools of all grades and classes should afford more effective training. The separation of social classes as it exists at present is due mainly to difference of education, and will disappear as education advances, and it is desirable that children from all schools should feel that they are fellow-citizens and can act together.

The Committee estimates highly the interchange of teachers throughout the Empire and the visits of parties of scholars to other countries. It considers that correspondence between pupils in different parts of the Empire and in other countries should be encouraged, and that the teaching of history, literature, and geography should be extended by continual reference to contemporary events. The value of the study of biographies of great men and women appears to be fully appreciated by many readers.

THE REPORT.

The Civic Education League has represented to the Committee that the issue of the 1920 report makes a definite and remarkable advance in civic education, and has suggested that it should be distributed to teachers throughout the country.

This suggestion was approved by the Committee, and permission has been given by the British Association to print the reports, if money can be raised for doing so. An appeal is being made through the Press and otherwise, and if the answers be favourable 20,000 copies of the report will be circulated. The Civic Education League has generously undertaken the necessary clerical work, if the reports are supplied to them and the postage, &c., provided.

The Committee had in view the preparation of a bibliography which was to form an additional appendix. It has, however, been thought best to defer the printing of the bibliography to the final report to be presented by the Committee in 1922, when the book on Civics, under the ægis of the Committee, will, it is hoped, be in the hands of the publishers.

A letter has been received from New Zealand asking for testimony from 'leading British Educationists' on the necessity of Bible reading in schools, and enclosing a broadsheet, *A World's Survey of the Bible-in-Schools Question*, which contains a mass of evidence in favour of the use of the Bible in schools. (The State system of New Zealand prohibits the use of the Bible or religious instruction in Government schools.)

In the interim report presented at Cardiff in 1920 a few typical schemes of training in Civics and Self-Government were outlined, and a sufficiently wide syllabus of things that a citizen should know was printed, and forms the basis of a book which is being prepared under the auspices of the Committee. The present report assumes the need for Civic instruction to be generally admitted, and aims at giving some account of the work which has been, and is being, done in different parts of the Empire.

The older Public Schools of this country have never been wanting in the spirit of Citizenship, although the workings of that spirit may have been limited to a narrow field. Lack of knowledge, rather than lack of patriotism, is likely to be the ground of any charge of deficiency in these institutions; and even in this respect there is ample evidence of efforts made in recent years to widen the knowledge and the sympathies of the average Public School boy. Apart from the increased importance of Modern History and Geography as school subjects, special lectures on Civics are given at many schools, and some have started Political Societies, in which modern social problems are discussed, and School and House Debating Societies of older standing are continually choosing social subjects. A Schoolmaster writes: 'The boys are ever so much more open-minded and serious about social questions than they were three or four years ago.'

An Inspector of Secondary Schools gives similar testimony, remarking that Sixth Forms throughout the country are adopting what he imagines to have been the mental attitude of the Rugby Sixth Form in the days of Arnold. In response to a questionnaire addressed to Headmasters as to instruction in Civics and schemes of Self-Government, a number of answers has been received in which, as might be expected, there is some difference of opinion as to the letter of the instruction, though none at all as to the spirit.

In Section I. a series of extracts from the answers received from the older Public Schools is given, while Section II. contains those from Public Girls' Schools.

Section III. contains the substance of replies from co-education schools, including an interesting experiment introducing a League of Nations.

Section IV. deals with Scotland, Ireland, and Wales.

Section V. with the overseas Dominions, Colonies, and Dependencies.

World-Citizenship is doubtless the ideal of every serious reformer, whether he be a missionary of Religion, of the League of Nations, of Imperialism, or of Communism, but the Civics of the family, the school, the parish, the district, and the constituency are probably wide enough for most boys and girls to study in detail.

SECTION I.

Extracts from Letters received from Headmasters and Assistant Masters of Public Schools.

The Committee accepts no responsibility for the opinions expressed in the extracts. The order of arrangement of the extracts is fortuitous.

Answers were received from correspondents at 1, Oundle; 2, Giggleswick; 3, Eton; 4, Leys School, Cambridge; 5, Haileybury; 6, Harrow; 7, Malvern; 8, Rugby; 9, Cheltenham; 10, Repton; 11, Epsom; 12, Fettes; 13, Mill Hill; 14, Manchester Grammar School; 15, Shrewsbury; 16, Westminster; 17, Wellington (Berks); 18, Winchester: 19, Bootham School, York; King Edward's School, Birmingham: King's School, Canterbury; St. John's School, Leatherhead; Sherborne; Uppingham.

1. 'I confess to a distrust of instruction in Citizenship. As far as I have seen, it means instruction in ideas of things as they are, not as I think as they might be for new needs.

'I also have doubts about "Boy" government. It may mean government on the old ideals, but I have not a great knowledge of the working, except that I believe Committees of boys assess and give punishments and maintain order and so on. But these things may all be wrong, and no punishments needed.

'Boys are useful in Boarding Houses, and give punishments for trivial offences, and save the master much trouble, and one good thing is that the boys who are punished do not feel any disgrace.

'But my own judgment is that even here boys carry out a "dominant" code, and really I think the system prevents the growth of the newer ideals. Again, they manage games, and with apparent satisfaction; but here also they leave on one side a large number of boys and cater only for the best. I am sorry that I cannot enter further into this important question, but my feeling is that it wants all changing.

'Democracy is not a form of government (is it?); it is an ideal.

'A good democracy will appoint an autocrat.'

2. 'We are not doing any systematic instruction in Citizenship. I occasionally take a book bearing on the subject with my Sixth Form, but I do not consider that it should form a separate subject for school instruction. The whole History and a good deal of Geography and much English Literature and Classical teaching offer plenty of scope for inculcating Citizenship.'

3. 'The self-government of boys is a very remarkable feature of English life, and I see no reason to doubt that boys learn much practical Citizenship from their experience in governing one another.'

4. 'In the Upper Fourth Civics is one of the Form subjects, and in the Lower Fifth Modern it has been customary to include some teaching on Political Economy in the course on Commercial Subjects.

'Every now and again the times allotted to History teaching are given up for a term to the study of Civics—*e.g.* the term before last was devoted to Swann's *Primer of English Citizenship*, treated differently according to age and capacity, all through the School.'

5. 'We make no attempt to give formal lessons in Citizenship throughout the School. I suspect they would degenerate into lessons on Constitutional History, and unless they were given by a genius might become a pure waste of time.

'Citizenship must be "caught, not taught." The primary lesson of a Public School is that no man lives to himself, but is a member of a larger community, his House, and his School. The excessive reverence paid to athletes is partly due to the sound instinct that they do more for the honour and glory of their community than the mere self-seeking scholar. This feeling for the community is in many cases (not in all) carried with a boy when he leaves School. But on the whole the Public School boy finds it more acceptable to die for his country than to live for it. He will go cheerfully to Passchendaele or to the West Coast of Africa, but he shies at the Town Council. He has it rubbed into him two or three times a year, notably at present by the Cavendish Association. This body does not confine itself to the inculcation of works of charity, but emphasises the need of men of education in local government.'

6. 'There is a Civics Class in the Sixth Form, conducted by our Head History Master, and it is running into a second class next term, since the numbers are too many for one man. Here Civics is definitely taught: the rest of the School get their chance through their English Classes, and through the various lectures from the various outsiders who come down to talk to the School at fairly frequent intervals on such subjects as the Workers' Educational Association, Employers and Employed, &c. Not anything very formal or organised, but the duties of citizens, especially of future landowners or employers, are fairly often brought before the boys in one form or another.'

7. 'No formal lessons are given in this subject; all boys who are physically able to do so have to join the O.T.C. (Junior Branch), and when in uniform are, of course, under military discipline. In addition to this, the Prefect System and the whole tradition of a Public School tends to foster that *esprit de corps* which it is the aim of "Training in Citizenship" to inculcate.'

8. 'There is not in this School at present any system of teaching Civics. A great deal of work of this kind is done, but in sporadic and rather casual ways. Some masters manage to work in the subject in connection with History teaching. There are Discussion and Essay Societies in various parts of the School, and lectures are given on branches of the subject by visitors to the School.

'I had intended, before the war, to have systematised the work, but I have not been able to resume that intention.'

9. 'I am sending you a note about our Cavendish Society.

'The Society is so called in order to bring it into some sort of relation with the Cavendish Club or Association, which is once more being brought before the notice of boys leaving School. It is composed of ten or twelve of the elder and abler boys interested in social questions, with a boy as Secretary and a master as President. It is a purely voluntary Society, and the design is to make it as much as possible a Boys' Society and as pleasant as possible, so that besides the President there is seldom more than one master present, and the proceedings are informal, and always begin with tea. The object is two-fold : (1) To gain some sort of knowledge of the historical background to the present state of affairs; and (2) to investigate scientifically the simpler economic and industrial problems of to-day and the remedies proposed. The Society is, of course, non-party, and to avoid the chance or suspicion of propagandising the Society seldom meets at the same master's house twice running, and seldom has the same master as a guest twice running. Where possible, the papers introducing the discussion are prepared by boys . . . . Some of the subjects of discussion have been : The Manorial and Guild Systems of the Middle Ages, the Industrial Revolution, England 100 Years Ago, the Railway Strike, the Sankey Commission; we have also held a discussion with some Bolshevistic Trade Unionists, and with a model employer from the North. Plainly the Society touches only a very few boys; but the idea is that in the end it does far more good to get a few able boys really interested than a large number at best slightly edified.

'I know one sure way of sickening boys of Civic Duties, and that is to have a lesson called "Civics." Any teacher of History who is worth anything of course works it in incidentally. . . . The greatest lever of all ought to be the College Mission. At Whitsuntide we are going to make a real effort that way to let the College see and get to know its Mission boys.

'As in all other schools of our type with which I am acquainted, every House is largely self-governed. I believe the system works admirably, and produces both for peace and war the type of citizen the Empire needs.

'I think it possible to detect a much more serious tendency in the thought of the average schoolboy of to-day; he recognises that cricket and football are not the only things that matter. In my opinion, the one thing we have to impress on the boy beyond all else is that *privileges* connote *duties*, and I feel sure that most boys are quite willing to believe this. It can be brought home to them out of School as well as in School, and perhaps even more vividly in the playing-fields than in the classroom.'

10 '. . . The Civics Class, a voluntary class intended for boys in their last year here and for a certain number of others, meets once a week. Membership of it is voluntary, though when once a boy has joined he must attend regularly throughout the term. Lectures are given by masters and sometimes by persons outside, such as the Master of Balliol and the Warden of Toynbee Hall, who were down last term, on various subjects such as the History of the Trade Union Movement, Capital and Labour, Education, Housing, and so forth. These classes serve a very useful function, and are always as full as the conditions permit.'

11. 'There are no definite lessons in the middle part of the School. History and Geography are, however, given a distinctly "imperial" bias. . . . My experience at Winchester in the days of Father Dolling leads me to consider a "live" School Mission as easily the first agent in developing a right spirit among Public School boys.

'Political subjects are treated (*a*) in set lessons to the Sixth Form; (*b*) in the Essay Society which meets once a month. . . . I believe myself that social duty is best inculcated by inspiration; in the Chapel sermons and . . . by addresses to the elder boys from such men as Prof. Zimmern, Alexander Paterson, Albert Mansbridge, and Canon Temple.'

12. 'Knowledge and interest in the subject is gained by sermons, lectures, essays, and perhaps, most of all by practice. Each House here has an "allotment" as a reminder of the urgency of the food problem. The food is sold to the College, and the proceeds given to local charities.'

13. 'The members of the Sixth Form write an English Essay every week, and many of these are on subjects which would come into any good course on Citizenship. . . . In addition to this, a flourishing Debating Society amongst

the older boys discusses seriously the questions of the day. The reading, too, of the older boys includes articles in such Reviews as the *Hibbert Journal*, *The Round Table*, *The New Europe*, &c., and various books dealing with the questions of Citizenship.

'There are representative Committees, chosen according to rule from the older boys of each house, to manage, with the assistance of one or more masters, the various School Societies, as well as the games and the library. The monitors have extensive powers, and these are shared by the prefects, or sub-monitors, to a lesser degree. Last year we had visits from George Lansbury and W. M. Hichins, who spoke to the School on the industrial problem, and were freely questioned at the close of their address by the boys present. It will thus be seen that there is no direct teaching, but indirectly a good deal of attention is bestowed on the questions of the day, and boys are encouraged not merely to write but to think. The results have, on the whole, been distinctly encouraging.'

14. 'We believe in teaching Citizenship through civic life, and do everything we can to encourage School Institutions run by the boys themselves. There is a large number of these, including Debating, Natural History, Photographic, Model Engineering, Musical, Astronomical, Chess Societies, and the latest addition to these is the League of Nations Union, run in two sections, one for the Sixth and one for Juniors. There is a Secretary in each Form, who tells off different subjects in connection with the League to different groups of boys to study, and he gets ten-minute lectures from any boy—in the dinner hour—who may volunteer in these different groups. Further, we have a general election at the School when there is a general election in the country. . . . All through the war we were running National Service Camps. . . . We have five Scout Troops, and each of these has a Court of Honour, which runs the affairs of the Troop. There is also self-government in our camps. Masters go with them, but do not "boss the show."'

15. 'Very great interest is shown at the School Debating Society, which consists of a large number of boys in the upper part of the School. Joint debates are held from time to time with the Workers' Educational Association (there were two last year). At the last there were seventy or eighty boys and thirty or forty townspeople. The subject was "Communism," defended by a railwayman of admittedly Bolshevist leanings, and opposed by a boy in the Sixth Form. In the ordinary weekly debates a social subject is sometimes chosen.

'Interest in Houses is well maintained in the School Mission. . . . A very fair number of boys go there in the holidays; there is a joint camp for the Club boys and the School boys in the summer; the Missioner comes down occasionally and gets in touch with boys in various Houses. . . . In 1918 a a Society was started (called the "1918 Society"). It consists of about twenty boys, mainly from the Sixth Form, and about ten masters, who meet informally in a master's room on Sundays, and read papers about three times a term. Subjects such as Local Government, Elementary Education, and the Labour Movement have been taken, and the papers mean a good deal of work on the part of the boy or master who reads them. . . . Keen discussion always follows the papers, but only those who are interested come to the meetings.'

16. 'The monitorial system is no doubt to some extent an illustration of the principles which underlie Civic duty. Some concrete acquaintance with the conditions of life in classes of society other than their own is also obtained by the boys by means of their active participation in the work of the Mission Clubs which we support in a poor district of Westminster. There is also in this School a peculiar opportunity afforded to the King's Scholars of learning something of the larger aspects of Citizenship through the privilege which they enjoy of attending debates in both Houses of Parliament.'

17. 'From time to time certain Forms take a course of Citizenship, and I am continually bringing the subject, directly or indirectly, into the teaching of the Sixth Form. . . . I should myself deplore any rigid scheme of Citizenship teaching. It is liable to become political propaganda, and is very often premature. It should be taught in connection with the History, Geography, and English lessons of the School, and depends in no small measure on the religious teaching given.'

18. 'Boys in a Public School probably now receive as much definite exhortation in lectures, sermons, teaching, etc., as they can digest, and I think many of us, while very willing to make many experiments, are convinced that boys learn more real Citizenship from their everyday duties and responsibilities and work, and from discussion in Societies, &c., than they would from a formal scheme of Civics instruction. After all, it is mainly a moral question, and we should doubt whether morality is best taught by lectures on ethics. So far as it is more than a moral question and involves, while still at school, knowledge of social problems . . . a good deal is done to bring these before boys—and I should not forget the College Mission. None of us, I expect, are content—at any rate that is not to be wished!—but I hope we are on the right lines. . . . As regards masters, many of us serve on all sorts of Committees and Municipal Bodies in the town, and this is not without effect on teaching and the general outlook of the staff.'

19. 'I write to express the hope that the Committee will make suggestions for short courses in the training of Citizenship as well as long courses. Some boys . . . take external examinations three or four weeks before the end of the term. I have found it possible to arrange an intensive course of Citizenship for these weeks, taking such subjects as Economics, Political Institutions, Housing and Health. As practical work boys can attend an occasional meeting of the City Council, visit slums and garden villages, spend a day in a large city with an arranged programme, visit a University Settlement, a Factory with a Welfare Department, a Juvenile Employment Bureau. These details can, of course, be varied according to environment.

'My real object in writing is to urge your Committee to help in a practical way schools that uphold the idea of Citizenship throughout the school career, and are unable to find the time for more than a short intensive course of lessons.'

SECTION II.

From Aske's Hatcham School for Girls.

'We do not treat Citizenship as a subject by itself and give it a definite place in the time-table and curriculum.

'1. We have therefore no "Scheme of Citizenship given throughout the School."

'2. We have no "School Houses," but the prefects and monitors take an active part in the government of their particular Forms and of the School as a whole. Elections to these and other offices are preceded by nominations duly made and seconded formally, and the responsibility of the election of suitable representatives is inculcated.

'There are Committees and officers for every School Society, and these also are duly elected by ballot after nomination.

'3. As to the general question of training in Citizenship, I consider that this comes into many lessons, and is always kept in view in general School training. Thus in Modern History special attention is given to the function of Parliament, forms of government, social legislation, &c., while lessons on current events and the keeping of calendars recording any such event of importance stimulate interest in public affairs.

'The encouragement given in the School to various forms of social service seems to me also a part of training in Citizenship. The girls learn not only to contribute money and work, but also to take an *intelligent* interest in such activities as clubs for working girls and boys, crêches, play-centres, as well as hospitals, orphan homes, &c.'

St. Paul's Girls' School.

'I am very glad to tell you my views about "Training in Citizenship," but, of course, I need not say that I am speaking for myself only, and I do not wish to seem to criticise the theories of other persons or their methods of carrying out their theories, because I believe that uniformity, whether of theory or of practice, in a matter of this kind is pretty sure to be disastrous.

'1. We have no scheme of Citizenship given throughout the School. I have been fortunate in having, almost from the opening of the School, Chief History

Mistresses who have felt strongly the necessity for teaching English History in such a way as to arouse a sense of Citizenship and a desire to know both the rights and the duties of citizens. We have a Union for Social Work which is constantly increasing in importance, and in connection with this we have had from time to time Reading Circles for studying books specially important in regard to our Social Work. Perhaps if I enclose with this letter a statement of what is being done you will see that it could be supported only by girls who were to some extent interested in Citizenship from a social point of view.

'2. With regard to self-government, if by that the Committee of the British Association means government of the girls in the School by one another, I am opposed to the practice with all my heart. I happen to know of one Headmistress who in one of the largest—if not the largest—Girls' Day Schools in London makes it a success, but I am quite certain that I could not make it a success, and I am not sure that I should wish to do so. The discipline of the School is entrusted very largely to the prefects, who constitute the Eighth Form, and are a body of girls between eighteen and nineteen, varying in different years in number from fourteen to twenty. They never give punishments, because there are no punishments in the School, but they have the right of forbidding the girls to do certain things, and if the girls are disobedient or are found breaking the School rules, such as the rule of silence in the passages and on the stairs, the prefects may tell the girls to report themselves to their Form Mistress. The Form Mistress has no power to punish, but if after a girl has been reported to her she repeats the offence the worst that can happen to the girl is that she should be sent to me. A great deal of power is put into the hands of the elder girls here, and I think I may say it is never abused. They carry on their own Societies—Science, History, Music, Literature, &c.—and I am the President of each and a few mistresses belong to each, but we never interfere with the conduct of affairs, and when I go, as I generally do, to a meeting I simply go to enjoy myself. Mistresses occasionally give help if they are asked, especially in the Science and Musical Societies.

'I am afraid my remarks have run into my answers to No. 2, so that I will end here by saying that I always feel anxious lest definite Citizenship Training, excellent though it might be in one school or another, might over a wide area sacrifice the spirit to the letter.'

From Ladies' College, Cheltenham.

'1. There is no scheme in Citizenship given throughout the School, but lectures on subjects of general interest are given from time to time.

'2. The fifteen Houses all have their own schemes of self-government; in the larger Houses these are developed to a greater extent than in the smaller ones, but the seniors in most of the Houses have a good deal of authority. In each case the head girl has many duties. Debates are held from time to time in the larger Houses, and also in the Forms of the Upper School.

'3. We have a Prefect System in the College, which I think is very valuable; we give a good deal of authority to the prefects. When difficulties of discipline occur I generally consult them on the subject, and in matters concerning the general welfare of the girls their opinions are welcomed by me. I see the prefects each week.'

From Croham Hurst School, South Croydon.

'We have no unique system, but I think we carry further than the *majority* of schools, at any rate, the principle of self-government, though it has many limitations.

'The School Officers are prefects and sub-prefects. Prefects are the most responsible senior girls; they have considerable authority and meet with the staff in consultation on many matters. Nominations for new prefects are brought before a joint meeting of staff and prefects, and the appointments made or rejected by a majority vote.

'Sub-prefects are Form Officers. Nominations are sent in from the Forms. Staff and prefects make the appointments.

'Every Friday morning there is a period given to Form work. During this time the interests of the Form are considered; representatives are elected to serve on School Committees; order is discussed; suggestions are freely offered of new methods, and criticism of unsatisfactory methods is freely given. The record of the Form is noted. Stars are lost for disorder, gained for good work (gains and losses being recorded for the *Form*, not for the individual). A girl is generally chairman on these occasions, another girl secretary. In some Forms, if time allows, news of public interest is contributed by different members.

'It is an axiom that girls are responsible for their own order, but in the actual working mistresses take an active share. We try for *Free Order*; sometimes the *freedom* tends to become licence; sometimes the *order* is too tight; but we are, I am sure, only doing what is being done by many other schools where there is a strong feeling that it is a matter of gigantic importance that girls should recognise, both in theory and practice, that there is work for all loyal citizens, and that it is their business to begin to know its character and how to tackle it.'

SECTION III.

From Bedales School.

'1. *Class Teaching in Citizenship.*—Occasional lessons have always been given, in connection with the ordinary History course, on our Constitution and the various branches of government, and in particular on the growth and working of local government. We are now proposing to combine these into an annual course of at least one lesson a fortnight for the upper classes, so that all, before they leave school, may have some knowledge of the Public Services and the way they work.

'2. *Parliamentary Debating Society.*—For some years the whole School has met twice or three times in each of the winter terms to debate on matters of public interest under Parliamentary forms. There is a Speaker (a member of the staff of the School), a Government, consisting of Prime Minister and other Ministers, and a recognised Opposition, sitting on opposite sides of the hall, and the rest of the School on cross-benches. All posts are open to both sexes alike. A Bill is brought in by the Minister-in-Charge, and, after debate, is voted on, and the Government stands or falls by the result. If defeated, a new Government is formed by the leading party in opposition. There are several parties, with differing programmes, not necessarily on the same lines as the actual Parliamentary parties. Such questions have been debated as Women's Suffrage, an Education Bill, the Nationalisation of Mines, &c., and Soviet *v.* Parliamentary Government. The debates are keenly prepared and conducted, there is never any lack of speakers, and the time has usually to be extended to allow of all sides being heard.

'3. *School Government.*—A large part of the actual government of the School, both executive and legislative, is in the hands of the boys and girls themselves. There are two grades of prefects: School prefects, who have full authority and can punish at their own discretion, subject to appeal to the Headmaster; and House prefects, whose authority is limited and their punishments checked by the Housemaster or Housemistress. All prefects are appointed, not elected by the School. Similarly, head and vice dormitory captains are appointed each term. Form captains are usually elected by the members of the Form, or they can ask the Form Master or Mistress to appoint one, if they prefer. In addition, we have for several years had a School Parliament, composed of two members, a boy and a girl, elected from each Form, the head boy and girl *ex officio*, all the Form Masters and Mistresses, and the Lady-in-Charge of the Girls' House, meeting once a fortnight under the presidency of the Headmaster. There are thus twenty-four members of the School and twelve members of the Staff. The purpose of the Parliament is to discuss, and periodically revise, the School Rules; to deal with new questions laid before it by the Headmaster or raised by any of the members, and to make new rules as required; to deal with serious or unusual matters of discipline; and to discuss from time to time the principles on which the School rests, and their application in its rules and traditions.

'In practice, after a year or two of keen and active life, in which the School Rules were thoroughly revised, there was a period of disappointment and lack

of interest. It was found that many of the questions brought forward and discussed could not be decided by a majority vote, as having consequences beyond the view of many of those voting; and it had been laid down as a foundation principle that nothing profoundly affecting the constitution of the School should be carried by a single generation of voters. Many questions, therefore, after discussion, were withdrawn and not put to the vote. This led for a time to the feeling that, as it was not the final authority on all matters, "Parliament" was a misnomer, and it was useless to bring any but matters of executive detail before it. To meet this difficulty it was arranged that alternate meetings of the School Parliament should be held informally between the School members and the Headmaster to discuss together questions of this kind, and that at the regular meetings only questions should be taken that could be put to the vote. This arrangement was found to work well, and interest revived in the meetings, especially the informal ones. It was proposed to extend these occasionally to the whole School, instead of confining them to the elected members; and this was done for a series of discussions on rules and punishments with great success. At present there is a question of altering the name to "School Council," in order to prevent misconceptions and to express more exactly its function in the government of the School.

'4. *Remarks on School Training in Citizenship.*—There is, I think, considerable value in class teaching in this subject, which is needed in order to give the necessary knowledge and to awaken interest in the duties that Citizenship will bring. But more important still, I feel, is to give boys and girls a share in the actual administration of their school life, in order to give them the habit of self-government and to lead them to realise the principles on which the government must rest and the methods by which it is best carried out. The actual forms under which this is done are not so material as that, so far as it goes, their responsibility should be real, and that the reasons for the rules by which their life is ordered should be, as far as possible, made clear to them, and reason and conscience alike be enlisted in carrying them out.'

Report on 'Training in Citizenship' at St. George's School, Harpenden.

I.—Teaching of Citizenship as Part of the Curriculum.

'1. There is no general scheme at present. A new scheme of History teaching throughout the School is under consideration, in which it is hoped to embody some instruction in the organisation of public affairs (political, social, economic) which the average citizen should know.

'2. From time to time Study Circles are formed to consider social problems, which is also the aim of the Social Science Section of the School Society in connection with the School Mission in Limehouse.

'3. In the top Forms Economics and kindred subjects are frequently dealt with by way of essays or discussions.

'4. The Upper Forms have this year been studying two of Ruskin's "Essays in Political Economy," and discussing questions arising therefrom.

'5. *Current Events.*—See attached report, which appeared in the *Daily Telegraph*, October 1919 :—

II.—Self-Government : Prefect System.

'1. There is a large measure of Self-Government in the School. There is a carefully graded Prefect System, each full prefect being given as far as possible a particular sphere of office (*e.g.* Captain of School, of Games, of Labour, of Lower School), as well as general responsibility for discipline. While not responsible for policy, prefects act in the capacity of a permanent executive directly responsible to the Headmaster, by whom they are appointed after consultation with members of the Staff and Captain of the School.

'2. There are two popularly-elected bodies of eight, one of boys and one of girls, who hold office for one term and who make recommendations and suggestions to the Headmaster on matters of School organisation.

'3. From time to time experiments in direct Self-Government are tried to meet particular occasions—*e.g.* (*a*) During the war boys and girls undertook a great deal of the upkeep of the School premises, &c., and the boys worked

a Labour Bureau which was for some time entirely in their hands, and worked by means of offering jobs to contractors and firms composed of boys.

'(*b*) *Property Court.*—To inculcate a greater respect for School property, a Court was instituted to try offenders, presided over by the Head Girl. The whole assembly acted as jury, and punishment was left entirely in the hands of the judge.

'4. As many of the Departments of School organisation as possible are either partially or entirely in the hands of the boys and girls. They run their own Debating Society, Pocket Money Accounts, &c., &c., and assist in the running of the Library, Magazine, Sports, School Society, &c. On three mornings in the week the three Upper Forms are each responsible for the Chapel Service.

General Remarks on Self-Government.

'Apart from particular experiments, the system usually adopted and generally approved is that of practical Self-Government, with due deference to authority and experience. It is held that, apart from other considerations, in a School which aims at a full curriculum and the encouragement of a width of artistic and literary interest, the boys and girls have not the time necessary for the complete organisation of any more than minor departments of School life, without undue interference with more important claims on their energies. But it is considered that, just as any effective discipline must be based on self-discipline, so Self-Government is the highest ideal to be aimed at in School organisation, and, with increasing experience and the development of greater freedom, it is hoped that the system may be considerably extended. One of the next experiments to be attempted will be Self-Government in a Form.'

(*Signed*) H. W. Howe (*Second Master*).

Current Events.

'We have realised more than ever this term that a proper understanding of modern affairs is not only essential for anyone who wishes to keep abreast of the times, but indispensable also for an adequate appreciation of the meaning of History. For a casual glance at any daily paper shows that History of the first importance is being enacted every day of our lives. The period of History through which we are passing at the present moment is at least as important as any period recorded in our History books. And it is, we maintain, only by understanding contemporary History that we can gain a right view of the History of the past. But the difficulties in the study of contemporary History are, first, that almost for the first time we realise that History is not the story of any one country or people, and the field of our study is nothing less than the whole world; and, second, that, whereas in studying the History of the past the sorting of the evidence has all been done for us, in the present the evidence is more abundant and confusing than ever before, and it is we who have to do the sorting. Now some attempt has been made during the war to draw attention to the significance of events by means of a weekly talk by a member of the staff. This method, however, besides putting an undue strain on what our American friends call the "faculty," failed to arouse much interest, and tended to defeat its own ends in that it encouraged boys and girls to feel that their newspaper reading was being done for them. The problem was to arouse interest and to ensure that everyone should make some attempt to study affairs for himself.

'Each Form was, therefore, offered a portion of the world for special study, and it soon became evident that the interest was there, and needed little arousing. It happened, fortunately, that every Form had a particular fancy. Form IV.A, Latin, for instance, would have none of Italy, and insisted on Russia. So that the world got itself split up willy-nilly as follows: The Sixth undertook the Balkans and the Near East; the Fifth, India and the Far East; V.B, Germany and Austria; the Remove (the Form where the budding politicians live), Home Affairs; IV.A, Latin, Russia and Poland; IV.A, non-Latin, America; IV.B, Latin, France; IV.B, non-Latin, Italy; and, as further division was found impossible, the Staff undertook the Colonies. It was felt that the rest of the world might be overlooked unless some other country could justify its claim to be represented. Every effort was made to induce Forms to try to see events as a whole through the eyes of their adopted country.

'The method of procedure was as follows: Two half-hour periods were assigned each week for a general meeting of the Forms. At the first of these a report was presented by each Form, and any time left over was set aside for asking questions. At the second the questions previously asked were answered. A chairman of the assembly was elected, and a secretary for the recording of questions. Each Form appointed weekly a secretary to draw up the report, and an orator to deliver it. Every Form had its own newspaper, and, as far as was practicable, as wide a selection as possible was made. As the *Daily Herald* was known to flourish in the Remove, the *Daily Mail* was imported as a salutary antidote. Home Affairs soon found that it was necessary to appoint Sub-Committees, and their report always took the form of a collection of minor reports.

'It was the intention that reports should deal, as far as possible, entirely with fact, and not with interpretation of fact. This has naturally proved a difficulty, but the difficulty itself has tended to show the point previously mentioned, that the first requisite for the would-be historian is that he should be able to sift out the truth from conflicting evidence. At first the reports failed, either from the incapacity of the Form secretary, who was often inclined to hand in undigested newspaper cuttings, but more often from the inaudibility of the orator. After certain elementary rules of procedure had been adopted, such as that anyone who could not hear might get up and interrupt the reader, more interest was aroused, and the standard of oratory improved. There was usually a great deal of difference in the quality of reports, some Forms being consistently concise and exhaustive, while others often caused inattention or disagreement. Another rule of procedure which was adopted was that the chairman asked at the end of each report whether anyone could supply any omissions. On one occasion the Colonies aroused antagonism by presenting what the Army delights in calling a Nil Report. The assembly was highly incensed, but when asked by the representative of the Colonies to supply omissions they were unable to do so, wherefore it was reasonably contended that the report was not unsatisfactory. This apparently justifiable claim, however, was strongly repudiated, and the situation was saved by a member of the VIth, who remembered having seen something about a strike in Winnipeg (the progress of which, incidentally, had been recorded weekly by the representative of the Colonies). It was decided, somewhat arbitrarily, that the Nil Reports would not be required in future.

'At the end of the reading of the reports there was invariably a hurricane of questions. "I should like to ask America, &c.," became the stereotyped formula, and, needless to say, America (if the Form were awake at the time) promptly retaliated by a counter-question. As a rule, notice was taken for answering questions, but as knowledge increased it was found desirable that some questions, points of geography, for instance, should be answered on the spot. It naturally proved difficult at times to find the necessary evidence for answering some questions, but for a representative to plead that "evidence had not yet come to hand" was allowed, provided that he undertook to keep his eyes upon the matter and to report later if possible. The formula "I will answer that question next time" fortunately became less frequent with the increase of confidence and experience, but it is still too common a subterfuge for the unprepared.

'Though the experiment is still in its early stages, and capable of further development, it has been abundantly proved already that interest in international politics does not require much stimulating. Interesting debates between actual or potential national opponents have been frequent, and one has felt at times over how slight a point one nation is liable to declare war upon another. It is noticeable that the majority has gone very strongly over to the left. The Government of our own country has come in for far more criticism than support. It has been difficult sometimes to adjust the balance of opinion, especially, for instance, in the matter of our policy in Russia. The Russian delegates have shown a marked tendency to support Bolshevism, but a report presented which was likely to sway the assembly over too uncritically to the side of Revolution always evoked a demand from the more balanced listeners for a contrary report from the other side. And if the second report did not succeed so well in appealing to the emotions of the

crowd as the first, it at least tended to make it felt that the chief danger in forming a judgment on contemporary History lies not so much in being unable to come to any definite conclusion, but rather in forming conclusions rashly on insufficient evidence. If the experiment has done anything, as we think it has, to help us to realise how History is written, and to enable us to base our judgments on a critical attitude, it has already done much towards helping on the study of History. It has, at any rate, given an outlet for the interest in present-day events that is evidently widespread in most parts of the School.'

M. J. O.
H. W. H.

SECTION IV.

Scotland, Ireland, Wales.

SCOTLAND.

Primary Schools.—The Fifth Schedule to the Day School Code prescribes 'certain studies bearing upon matters which it is of concern that all the pupils should know, whatever their occupations in after life are to be,' including laws of health, money matters, the Institutions of Government under which we live, the Empire, &c., and the explanatory circular states that the information should be such 'as will help to make intelligent and patriotic citizens. Pupils should have 'a direct knowledge of their own neighbourhood, of its historical monuments, and of the machinery of its Local Government.'

Secondary Schools.—There is little or no direct teaching of Citizenship; but the teaching of History, which is insisted on, is no doubt valuable in this connection. On the other hand, there is much more effort to develop self-government than in the Primary Schools.

Among some interesting replies the following, from a Headmistress, may be noted: 'In my experience girls at the adolescent stage have less of the power than boys have of taking things easily. They are apt to be over-conscientious about . . . duties that are entrusted to them, and I have seen among the prefects that even the limited duties that we impose upon them do lay on them a burden of responsibility which they feel, and which I should be very sorry to increase further.'

IRELAND.

A course of lessons in Citizenship is compulsory in all Irish National Schools, although no definite instructions are given as to the extent of this course. There is no such requirement for Intermediate (Secondary) Schools. The following extracts from a letter received give some idea of the difficulties surrounding Secondary Education in Ireland :—

'Perhaps you will be interested to know about the position of Secondary Schools and teachers in this country generally, and in this locality in particular.

'1. For many years this branch of education has been starved in Ireland. Grants admittedly equivalent to those given in England and Scotland have been withheld by the British Treasury. Education Bills have been passed for England and Scotland; none for Ireland. Teachers in England and Scotland get at least a living wage and a pension; Secondary teachers in Ireland are scandalously underpaid, and at present on strike. One result is that many Secondary teachers are leaving for posts in England and Scotland. The position is unbearable, and unworthy of a great Empire like ours.

'2. This particular School is struggling to give a Secondary education to over 200 boys and girls. There is no endowment and no local rate; very moderate fees are charged and very meagre grants obtained from the Board of Education. Out of this income a debt is being paid off, rent of buildings, teachers' salaries, upkeep, apparatus, equipment, &c., all have to be met. To crown all, the landlord, who is an absentee, living in England, will not *even rent* to us a field for the development of our school sports, although there is a derelict demesne with plenty of grazing land available. About 250 old

pupils of the School volunteered during the War, but even this seems to have no effect on this member of the class which stood to lose most by the loss of the War.

'What are teachers in Ireland to do?

'Are they to teach loyalty and affection to, and pride in, an Empire which treats them so badly? Am I, in particular, to teach brotherhood towards a class, one of whose members throttles our efforts towards development?

'As regards your two questions: (1) There are no lectures or lessons given in the subject of Civics at present. (2) The pupils organise and govern their own school games; the School is a Day School.'

WALES.

The following extracts from the Rhondda Urban District Council Syllabus are typical:—

'In the Secondary Schools there is systematic training through the medium of Constitutional History, as well as specific training in the subject of Civics, giving the pupils an intelligent appreciation of the origin, growth, and functions of Parliament. Lessons are given on Local Government . . . Economics, Home Production, Foreign Trade, Interdependence, and Taxation. . . . The Qualities of a good Citizen. . . . Conferences are held from time to time with the Upper children, in which the welfare of the School as a whole is discussed. . . . The pupils have the fullest opportunity of expression.'

Citizenship is taught very widely in Glamorgan, and a large number of excellent syllabuses have been drawn up for individual schools. Among the many experiments in Self-Government described is a 'Sunshine League.' If cheerfulness be not an essential part of Citizenship it should always be 'breaking in.'

SECTION V.

Canada, Australia, New Zealand, South Africa, India.

CANADA.

The outstanding feature in Canadian education is the sovereignty of the Provinces in educational matters. At each of the Provincial capitals there is a Central Authority for Education. The central organisation of Quebec differs fundamentally from that of the other Provinces, since it is complicated by problems of language and religion. The Council of Public Instruction is divided into two Committees—Roman Catholic and Protestant. All teachers in Roman Catholic Schools must pass an examination in Civics with a view to teaching it. In other schools the subject is taught in an incidental way in connection with History and Geography. Civics is taught as a separate subject in Alberta and Nova Scotia. British Columbia includes Civics in its Commercial Course, and has added Canadian History and Civics to the list of subjects prescribed for first-year High School pupils. In Ontario and Manitoba 'manners and morals' are taught.

AUSTRALIA.

The separate States have independent educational systems. In New South Wales some teaching in Civics is given in connection with History in the upper classes in Elementary Schools. In Queensland teaching in Civics and Morals is given both incidentally and by means of separate lessons throughout the school course. In South Australia a new course of instruction in Primary Schools has been issued, in which an effort is made to bring Civics into the whole atmosphere of the school, more particularly in connection with the teaching of History. The effort has already been made in Tasmania under the same Director, and good effects are already manifest. In Western Australia definite instruction in Civics is given in the VIIth and VIIIth classes. No information has been received from Victoria.

NEW ZEALAND.

Instruction is given in Civics in connection with History in the middle and senior divisions of the Public Schools.

SOUTH AFRICA.

The peculiar difficulties connected with the bilingual white and the native population make the study of Civics as applied to South African problems more than ordinarily interesting and important. There has recently been a movement in the direction of the separation of Dutch- and English-speaking children, which is the subject of unfavourable comment by *Ons Land*.

CAPE PROVINCE.

The *Outline of the Secondary School Course* contains, under the heading Geography and History, a detailed syllabus of South African History, Civics, and Modern European Industrial History. There is, however, a preamble, in which the following paragraph occurs :—

'The selection of courses and subjects to be taken in any school is left to the School Managers.'

The Primary School Course includes 'Simple lessons on Social Institutions, local and national.'

The Committee is indebted to Mrs. Stratton, of Rondebosch, for a copy of the Annual Report of the Boy Scouts' Association for the Cape Province, from which it appears that no less success attends that movement in South Africa than elsewhere.

NATAL.

No definite instruction is given in Civics in the Primary Schools, but stress is laid on Empire Day, Trafalgar Day, Union Day, &c.

TRANSVAAL.

The Education Department lays down that 'in addition to the facts of History which are to be taught . . . an appreciation of what is meant by Citizenship and Civic duty should be carefully cultivated. This may be done in two ways : incidentally in connection with the discussion of historical facts and events, and formally by means of set lessons.'

INDIA.

The Committee has information of a scheme originated by Pundit Shyam Shankar, M.A., Barrister-at-law (of Jhalawar), to supplement University education by auxiliary civic training, and we append his syllabus as promised in the 1920 report. Civic duties are defined as including 'Good citizenship and discipline, organised humanitarian work of social service, public morals and public health.' The essential points in character-building are given as 'Loyalty, true patriotism and duties to the Empire.'

From the Quinquennial Review (1912-1917) of the Progress of Education in India, it does not appear that any special attention to Civics has been paid in any portion of that country; though it is probable that recent events have brought the subject of Citizenship into prominence. Allusion is made in this review to the growth of the Boy Scout Movement, and the following statement by Sir Robert Baden-Powell may serve to close this Report.

Character-building in the Empire.

'One particularly patent effect of the War common to India, Egypt, Palestine, and even Burma and Ceylon, was their aspiration to be considered as definite nations. Yet practically not one of these countries could yet carry on self-government independent of military protection or financial aid on the part of

some outside Power, nor was the character of the peoples themselves in any one case sufficiently strong, in the mass, for them to be able to run a stable Government independent of all protection.

'At present these national aspirations are directed by politicians, who trade largely on the emotional qualities of the people. The statesmen, who look ahead, realise that independence cannot be safely jumped into at one bound, but must be gradually built up, mainly on a foundation of moral and material qualities in the nation—that is, through improved education and through the development of national resources in industry and commerce. It is the character of the people themselves that counts. It is here that the Boy Scout and Girl Guide movements come in. Different though conditions are in the different countries, the statesmen in each seemed to recognise that this training is what is needed to put character into the oncoming generation of citizens. In every one of the countries visited we found Boy Scouts, and in most of them Girl Guides, already being raised among the youth of the country with that intention.

'In India half-a-dozen different Scout associations had been started among the Indians for improving their education; and similarly in Egypt we found four or five different Scout organisations at work. These in each case were distinct from the branches of the British or other foreign Boy Scouts' associations. Fortunately these movements were still young; in many cases we found them a little off the line, if not in principle, at least in detail; and in many cases they had adopted the form, but not—what is more important—the spirit. Everywhere we were most generously received, and our criticisms, instead of being resented, were whole-heartedly accepted, and are generally being acted upon. In India the suggestion that the different Indian organisations should amalgamate in one great whole for the good of the nation was cordially carried out; I have every reason to hope that the same broad-minded spirit will prevail in Egypt and in Palestine also.

'In each country the great need is instruction to enable scoutmasters to imbibe the true ideals and grasp the practical methods of the training. If we can send out trained instructors to establish schools of instruction on the right lines to this end, Imperial headquarters can do a work that may have an immense influence on the future character and well-being of those countries and their relations with Britain. If we can afford to employ a small staff of capable instructors to give the right direction to the training, it is going to mean not only a very big step towards equilibrium in the nation at home, but the development of balanced character, brotherhood, and self-sacrificing service among all the countries in the British Commonwealth.'

Charts and Pictures for use in Schools.—*Final Report of the Committee* (Sir Richard Gregory, *Chairman*; Mr. G. D. Dunkerley, *Secretary*; Mr. C. E. Browne, Dr. Lilian J. Clarke, Mr. E. N. Fallaize, Mr. C. B. Fawcett, Professor S. J. Hickson, F.R.S., Mr. O. J. R. Howarth, Mr. A. E. L. Hudson, Mr. C. C. T. Morison, Mr. H. J. E. Peake, Professor S. H. Reynolds, Professor H. E. Roaf, Sir Napier Shaw, F.R.S., Dr. T. W. Woodhead).

Introduction.

This Committee was formed at the Bournemouth meeting of the British Association in 1919, with the following terms of reference :—'To inquire into the provision of Educational Charts and Pictures for display in Schools.' It presented an interim report to the Cardiff meeting in 1920. The Committee hoped to arrange for an exhibition of selected pictures, but it had reluctantly to give up the project owing to the difficulty of meeting the necessary expenditure.

The Committee, at its earlier meetings, came to the conclusion that, to keep the report within reasonable limits and at the same time serve a useful purpose, attention should be confined to the compilation of a list of pictures suitable for display in schools and for decorative rather than purely educative purposes, the list to be select rather than expansive.

Supply of Suitable Pictures.

Many sets of pictures for use in schools have been published from time to time, yet, in the main, these have been designed more as adjuncts to the school lesson than as decorative, artistic, and stimulative in effect as well as of educational value.

Of late years, the National Society, the Art for Schools Association, and other organisations have done excellent work in cataloguing and supplying pictures of artistic merit, but little attempt has been made in this country to produce and supply decorative pictures intended to inspire interest in national objects and scenes or the achievements of man. Unfortunately, the Art for Schools Association has been compelled to curtail its activities and steps have been taken to dissolve the Association. However, to meet the considerable demand for orders, the office has been transferred, by the invitation of the Director of the International Students' Bureau, to 56 Russell Square, W.C.1, and is still at the service of school authorities. The London County Council has for some time had the matter of school pictures under consideration, and in an interim report of a Special Advisory Committee of the Education Committee, July 1920, occurs the following statement :—

'It is only within comparatively recent times that it has been realised that the school picture should be designed for its purpose. About 1891 Heywood Sumner, Selwyn Image, Christopher Whall, and Louis Davis designed a series of pictures which were published by Mr. Heywood Sumner under the name of the FitzRoy Publishing Society. These pictures may be regarded as the beginning of the modern conception of the school picture as a wall decoration of suitable size and simple colour treatment in addition to its class use. In spite of this excellent lead, the work of British publishers has been, on the whole, inferior in conception and execution to that of the French, and especially of the Germans, who, for some time before the war, by the quality of their productions were gaining command of the market in this important branch of educational supply. This situation may be regarded as disastrous for two reasons. In the first place, it is generally recognised that one of the chief influences which such pictures may exert lies in the direction of national inspiration, and it is unthinkable, for this reason at least, that the preparation of them should be in alien hands, and that they should present an alien point of view.

'Secondly, so long as the matter is left uncontrolled as a chance commercial speculation, institutions and others are forced to choose from what is brought to them, with little chance of getting what they most require.

'In this connection it is interesting to record that, so long ago as 1906, there was held at the London County Council Central School of Arts and Crafts an exhibition

of pictures for school decoration, the exhibits in which, apart from the few excellent FitzRoy pictures, were almost entirely of German origin. The object of this exhibition was to call attention to the need for improvement in British work, but our publishers, failing to understand the warning intended to be conveyed by the exhibition, unfortunately only saw in it an advertisement of German enterprise.

'Various movements have been recently set on foot in this country for the consideration of the whole subject, with a view to replacing the existing supply with material which should be more carefully thought out and of a higher standard. The Victoria League, for example, arranged some time ago for an exhibition of Medici prints in the Dominions, towards which the Medici Society and the Senefelder Club gave valuable assistance; and the Board of Education has been for a long time considering the educational possibilities of such a movement. Whilst, however, each of these movements was doing its own specific work, it became more and more clear that steps should be taken to co-ordinate them, and, more especially, to carry out certain preliminary work which was essential if the best results were to be obtained. As a preliminary, it was desirable to effect some sort of co-operation between the principal bodies interested in the subject, if possible from the commercial as well as from the official side. It was, of course, necessary in this respect that care should be taken to avoid any interference with commercial susceptibilities or trade competition, but it was felt that in such a matter as this the trade would recognise the importance of obtaining official guidance and advice both as to standard of work and character of subject.

'In order to obtain the best results it was desirable that the question should be approached from the technical and artistic as well as from the educational standpoint, and that, if possible, a series of experiments should be made in co-operation with the best authorities on these points, and that the results should be published as a guide both to educational authorities and also to commercial firms. It was felt that the subject was worthy to enlist the co-ordinated efforts of all the best-trained intelligences that could be brought to bear upon it, so that, when the experimental stage was passed, the results might be put at the disposal of the trade, to serve at once as a guide and a stimulus to their production. For the purposes of the experimental stage, the London County Council, with their wide educational experience and their admirably equipped Central School of Arts and Crafts, was obviously best fitted to undertake the work, and in the summer of 1917 they accordingly appointed a special Advisory Committee to consider the matter.'

The London County Council Advisory Committee had six pictures on the subjects shown below painted by artists and executed at the Central School of Arts and Crafts.

Lambeth Palace.	Print illustrating the use of the Breast Plough.
Stirling Castle.	Reproduction of Old Map of Tower of London.
Mountain Scene.	A Scene in the Docks.

These pictures have been in use in the schools experimentally, and the general opinion of the head teachers who were consulted is that they are suitable for school work and could usefully be employed for that purpose. Unfortunately, little interest has been shown by British publishers in the movement, and, having regard to the present financial stringency, the London County Council has decided to discontinue the research. The work of the Advisory Committee has been most valuable in demonstrating that auto-lithographs can be produced in this country at a price which would justify English publishers entering into competition with other countries.

Before the war the London County Council issued a classified list of framed pictures which have been approved for school use, and arranged for the display of specimen copies of these pictures at the Education Offices. This list is now, however, out of date, and present trade conditions, so far as the publication of school pictures is concerned, have rendered it impossible to issue a revised list.

It is of interest to note that industrial engineering is given prominence in this year's exhibition of the Royal Academy, and the day may soon come when other branches of science may be accepted as fit subjects for the work of future exhibitors.

An exhibition of maps, pictures, and charts illustrating modern methods of History teaching was organised by Prof. F. J. C. Hearnshaw at King's College, London, in July 1914, and the catalogue can still be referred to. Prof. Hearnshaw also contributed an excellent article on Historical Pictures to the *School World* for July 1910.

The Function of School Pictures.

The use of charts and pictures for the purpose of illustrating lessons in the class-room has been growing steadily for some time, but the subjects chosen, and the execution of the pictures, have not been suitable for decorative effect. The presence of artistically executed pictures upon the too often bare walls of the class-room would go far to improve and brighten the surroundings of the school and to enliven and inspire the daily routine of the child. Early in its proceedings, the Committee instituted a search for pictures which, while primarily educational in value, were accurate representations of the subjects, and would also stimulate the imagination of the pupil, inculcate the spirit of inquiry, and foster the appreciation of artistic merit. The man of science can represent facts rigidly, the draughtsman can produce strictly accurate representations of scientific phenomena and engineering feats, but it is the artist alone who can visualize Nature and combine these requirements in a picture which is neither coldly magnificent nor purely scientific, and at the same time replete with artistic representation and warm with the moving and human element.

Pictures of this type are not common, and the Committee earnestly invites the consideration of publishers as to whether the provision of such pictures should not be attempted. The Committee is of opinion that pictures of this nature would be a most welcome item for school requisitions, and, certainly, a collection of striking pictures of decorative nature would appeal to all types of children and be likely to interest teachers and pupils generally, and, though not necessarily intended for instruction on specific points, would be of great educational value.

Examples of Pictures of the Required Type.

A collection of drawings meeting excellently the requirements of the Committee as indicated above is afforded in Joseph Pennell's ' Pictures of War Work in England.' These are reproductions in black and white of a series of drawings and lithographs made by him, with the permission and authority of the British Government, of the munition works. They are published in book form by W. Heinemann, price 6*s.* The lithographs are also published in large size by Messrs. Colnaghi and Obach, New Bond Street, London, price 3*l.* 3*s.* each. An American series by the same artist—' War Work in America '—is published by the J. B. Lippincott Co., London and Philadelphia, price 9*s.*, and a corresponding series of enlarged lithographs by F. Keppel & Co., New York. The originals of the English series are in the Print Room of the British Museum, and the American drawings in the Print Division of the Library of Congress, Washington. These pictures, showing the remarkable scientific feats and inventions which accompanied the war, are realistic in effect and full of life. The suggestion arises immediately : Why should not an artist produce similar pictures depicting the work of the early alchemists, of spinning and weaving, of prehistoric Nature, and innumerable other subjects ?

Other drawings by Mr. Pennell of a similar type and excellence are :—

' The Panama Canal.' W. Heinemann, London. Now out of print.
' Pictures in the Land of Temples.' Ditto. Price 5*s.* net.
' The Wonder of Work.' Ditto. Now out of print.

The decorative effect is being enthusiastically worked out in the American Museum of Natural History under the direction of Professor H. F. Osborn. A series of eight large mural paintings illustrating the life of the African and American continents during the final period of maximum glaciation, and representing the four seasons of the year in mid-Glacial time, have been executed in accordance with the general theory of exhibition which prevails throughout the American Museum—namely, to present animals, extinct as well as living, in their environment. A list of pictures published by the American Museum may be seen in *Natural History*, American Museum, Vol. XX., May-June 1920, No. 3, and an article by Professor Osborn dealing with some of them will be found in *Nature* of April 21, 1921. (See also pages 379 and 387.)

Another example of the successful combination of human interest with scientific fact is the fresco depicting John Dalton collecting Marsh Gas (Assembly Room, Town Hall, Manchester), painted by Ford Madox Brown. (*Cf.* page 383.)

No doubt there are other drawings and paintings equally appropriate in various picture galleries and public buildings about the country, but the Committee in the limited time at its disposal can only cite the above.

Lists of Available Pictures, or Prints suitable for Enlargement as Pictures.

The subjoined lists of pictures in various subjects are limited to pictures or prints easily accessible, and can be regarded only as selections of the most suitable of the available pictures for school use. The lists by no means constitute a catalogue, and they are inserted on the responsibility of the subject-experts on the Committee or of others who have been consulted. They are not intended to be exhaustive but rather illustrative of what exists at present in one form or another. It would be possible to make up a good collection of pictures of scientific subjects from the lists, yet no such general selection of wall pictures has been published up to the present. A striking picture of terrestrial or celestial object or scene may stimulate interest in the subject and thus achieve an educational purpose. What the Committee would like to see is many more pictures representing great events in the history of science, but artis s have very rarely attempted such subjects. It would be a great gain to education in science if a firm of publishers would commission artists to prepare series of pictures of this kind.

Selected Lists of Pictures.

I. ANTHROPOLOGY.

A. Among the Mural Paintings in the Hall of the Age of Man in the American Museum (presenting mammalian life during the final period of maximum glaciation) are :—

1. A mid-winter steppe scene of Northern France, showing the Woolly Rhinoceros, the Saiga Antelope, and the Woolly Mammoth.
2. Early Spring—The Reindeer and Mammoth on the River Somme, France. Representing a northward march in the spring.
3. Midsummer—The Mastodon, Royal Bison, and Horse on the Missouri River.
4. Crô-Magnon Artists at work on a Palæolithic Mural.

For further details of Murals, see also pages 378 and 387.

B. Völkertypen (Folk Types). Collection of 37 art pictures in deep copper-plate, from sculptures by Rudolf Marcuse. Published in portfolio by Gustav Fock, Leipzig (with an appreciation by Prof. von Luschan, of Berlin, 40 marks).

C. Illustrations in 'Evolution in the Past,' by H. R. Knipe (London: Simpkin, Marshall & Co., 12*s.* 6*d.*). Recommended by Dr. A. Smith Woodward, Natural History Museum.

II. ARCHITECTURE.

Selection by the Art for Schools Association.

Barraud F. P.
: Oxford Colleges—(1) Magdalen, from the Meadows. (2) Magdalen (First Court). Chromo-lithographs. 6¾ by 10 ins. 1*s.* 6*d.* each.

George, Sir Ernest.
: Views of London—Cheapside. Ludgate Hill. Wych Street. Drury Lane. Fleet Street. Staple Inn. Chromo-lithographs. 14 by 9 ins. 2*s.* 6*d.* each.

Hine, Mrs. Harry.
: Cambridge Colleges—(1) Gateway, St. John's. (2) Clock Tower, Trinity. (3) St. John's. (4) Great Court, Trinity. Chromo-lithographs. Nos. 1 and 2, 10⅝ by 7½ ins. Nos. 3 and 4, 7⅝ by 10⅜ ins. 1*s.* 6*d.* each.

Piranesi, Giovanni Battista.
: The Colosseum. The Pantheon. Arch of Constantine. Temple of Janus. Interior of the Colosseum. Temple of Concord. Etchings. 16 by 27 ins. 3*s.* 6*d.* each.

Delauney, A. A.
: Westminster Abbey. Etchings. 24⅝ by 19¼ ins. 1*l.* 1*s.*

Hollar, Wenzel.
: View of London from Bankside (Southwark) in 1647 (Topographical Society). A complete impression of the original etching is in the British Museum (Art for Schools Association). Collotype in six parts. Size of each, 11 by 9⅝ ins. 10*s.* 6*d.* the set.

III. ASTRONOMY.

Selection (in consultation with Professor H. H. Turner) of Celestial Photographs, published by the Royal Astronomical Society.

The following photographs may be obtained through Fellows of the Royal Astronomical Society as prints, either platinotype or aristotype, mounted on sunk cut-out mounts, measuring 12 inches by 10 inches (30·5 cm. by 25·5 cm.); also unmounted, and as lantern slides, $3\frac{1}{4}$ inches square (8·2 cm.).

Price: prints, mounted 1*s.* 6*d.* each, unmounted 1*s.* each; lantern slides 1*s.* each.

R.A.S. Ref. No.	Subject and date:	Photographed by:
91	Portion of Moon (Mare Serenitatis)	Yerkes Observatory
98	Nebula in Andromeda	Yerkes Observatory
107	The Moon	P. Puiseux
151	Region of Θ Ophiuchi	E. E. Barnard
157	Sun-spot, 1906, July 31	Royal Observatory, Greenwich
158	Sun-spot, 1906, August 3	Royal Observatory, Greenwich
178	Sun-spot Vortices, 1908, October 7	G. E. Hale
184	Comet, 1908, III, November 25d. 5h. 55m.	Royal Observatory, Greenwich
223	Spiral Nebula in Canes Venatici	G. W. Ritchey
227	Annular Nebula in Lyra	G. W. Ritchey
228	Crab Nebula in Taurus	G. W. Ritchey
230	Nebula in Comæ Berenices	G. W. Ritchey
234	Nebula in Cygnus	G. W. Ritchey
254	North America Nebula in Cygnus	Yerkes Observatory
288	Cluster in Hercules M.13	J. S. Plaskett

Small-scale photographs of Mars, Jupiter and Saturn are also available.

IV. BOTANY AND NATURE STUDY.

A.

1. The Medici Galleries.
   Crome. The Poringland Oak (in colours).
2. The Autotype Fine Art Co., New Oxford Street. (Large pictures, 15*s.* each separately, 10*s.* each if a number is taken.)

Trees:—

Leader. The Valley of the Llugwy. View with Silver Birches	$10\frac{1}{2}$ by $18\frac{1}{2}$ ins.	Tate Gallery
MacWhirter. May. Silver Birches	$18\frac{3}{4}$ by 13 ins.	Royal Academy, 1906
Ernest Parton. A Fairy Woodland. Birches near Lake	$18\frac{3}{4}$ by $14\frac{1}{4}$ ins.	Royal Academy, 1901
Simmonet. Birches in Winter	$13\frac{1}{2}$ by 18 ins.	Franco-British Exhib.
Hobbema. Avenue of Poplars	$13\frac{1}{2}$ by 18 ins.	National Gallery
MacWhirter. Lake Como. Afternoon	19 by $12\frac{3}{4}$ ins.	Royal Academy, 1904
Vicat Cole. A Pause in the Storm	$12\frac{1}{2}$ by $18\frac{3}{4}$ ins.	Original in possession of Lord Armstrong
Leader. Green Pastures and Still Waters	13 by 19 ins.	Franco-British Exhib.
Leader. Evening Hours	$18\frac{1}{2}$ by 15 ins.	Royal Academy, 1905
Leader. A Shallow Stream at Eventide	$12\frac{1}{2}$ by $18\frac{1}{2}$ ins.	Royal Academy, 1904
W. H. B. Davis. Thorn Trees	$9\frac{1}{2}$ by 19 ins.	Royal Academy, 1904
Crome. The Poringland Oak	$17\frac{1}{2}$ by $14\frac{1}{2}$ ins.	Original painting at Somerleyton

Flowers:—
MacWhirter. June in the Austrian Tyrol.

Lake Vegetation:—
Millais. Chill October.

3. Photographs by Henry Irving, Reading, in Permanent Carbon. Similar to those shown in the Natural History Museum, London.

Trees:—
20 ins. by 16 ins. 9*s.* each or 100*s.* dozen.
Deciduous Trees, 28 in number.
(1) Summer aspect of each.
(2) Winter aspect of each.
Evergreen Trees, 8 in number, 20 ins. by 8 ins.

4. Photographs by Henry Irving, Reading.

Whole range of British Trees, Native and Naturalised. Permanent platinum, 10 ins. by 8 ins., 5*s.* each, or 56*s.* dozen. Deciduous, Summer and Winter aspects, 54 in number. Evergreen, 25 in number.

Reproductions of most of the above photographs can be seen in 'Trees and their Life Histories,' by Groom. (Now out of print.)

Plant Associations	6 by $4\frac{1}{4}$ ins.	1*s.* 9*d.* each.*
Wild Flowers *in situ*	6 by $4\frac{1}{4}$ ins.	1*s.* 9*d.* each.*

* Enlargements of some of these can be made.

B. *Selection by the Art for Schools Association.*

Detmold, E. J.

The Great Eagle Owl. Chromo-collotype (Art for Schools Association). Size $17\frac{1}{2}$ by $14\frac{1}{2}$ ins. 3*s.* 6*d.*

Decorative Drawing of a Cock. Chromo-collotype (Art for Schools Association). Size $12\frac{1}{8}$ by 18 ins. 3*s.* 6*d.*

Griset, Ernest.

A Nubian Lion Asleep. Chromo-lithograph (Art for Schools Association). Size $22\frac{1}{2}$ by $32\frac{1}{4}$ ins. 2s. 6*d.*

Hobbema, Meindert.

The Avenue, Middelharnis, Holland (National Gallery, London). Facsimile Aquarell. Size $15\frac{1}{2}$ by $21\frac{3}{8}$ ins. 4*l.* 4*s.*

Kirkpatrick, Ida.

Garden Poppies. Chromo-lithograph (Art for Schools Association). Size $25\frac{7}{8}$ by 18 ins. 3*s.*

Mori, So-Sen.

Monkeys and Plum Tree (British Museum). Chromo-lithograph (Art for Schools Association). Size $28\frac{1}{2}$ by $13\frac{1}{2}$ ins. 2*s.* 6*d.*

Riviere, Henri.

Studies of Nature :—Twilight. A Steep Cliff. Night at Sea. The Mountain. The Rising of the Moon. Sunset. A Wood in Winter. The River. The Island. A Summer Evening. The Bay. The Rivulet. Colour Prints. Size $21\frac{3}{4}$ by 33 ins. 6*s.* each.

Thorburn, Archibald.

A Sea Gull. Chromo-lithograph (Art for Schools Association). Size 16 by $20\frac{1}{8}$ ins. 3*s.*

Varley, Miss E. L.

Hollyhocks. Chromo-lithograph (Art for Schools Association). Size $26\frac{1}{4}$ by $17\frac{1}{2}$ ins. 3*s.*

Kearton, Richard and Cherry.

Pictures from Nature. Rembrandt Photogravures from Photographs of Birds and Beasts at home amidst natural surroundings.

Black-throated Diver	$7\frac{3}{4}$ by 11 ins.	10*s.* 6*d.* the set.
Kittiwakes at Home	9 by $7\frac{3}{4}$ ins.	
Leverets in their Form	$9\frac{3}{8}$ by $7\frac{1}{2}$ ins.	
Kingfisher Waiting for its Prey	$7\frac{3}{4}$ by $10\frac{1}{8}$ ins.	
Squirrel	11 by $7\frac{3}{4}$ ins.	
Puffins at Home	$7\frac{1}{2}$ by $11\frac{3}{8}$ ins.	
Young Willow Wrens	$7\frac{7}{8}$ by $9\frac{3}{8}$ ins.	
Ring Dove	$7\frac{3}{4}$ by $10\frac{1}{2}$ ins.	
Young Long-eared Owls	$7\frac{7}{8}$ by $10\frac{3}{4}$ ins.	
Gannet or Solan Goose	$7\frac{1}{2}$ by 11 ins.	
Peewit or Lapwing	10 by $7\frac{3}{4}$ ins.	
Sparrowhawk adding Sticks to her Nest	9 by 8 ins.	
Great Tit, or Oxeye	9 by $7\frac{1}{2}$ ins.	
Young Cuckoo and Sedge Warblers	$11\frac{3}{8}$ by 8 ins.	
Hedgehog	$7\frac{7}{8}$ by $10\frac{1}{2}$ ins.	

Hardy, Heywood.

African Elephant's Head. Etching. $7\frac{1}{4}$ by $10\frac{1}{4}$ ins. 1*s.* 6*d.*

Morland, George.

Rabbits. Collotype (Art for Schools Association). $14\frac{1}{4}$ by $18\frac{1}{4}$ ins. 3*s.*

Slocombe, F.
Silver Birches. Etching (The Fine Art Society). 19 by 13 ins. £1 1*s.*

Vinci, Leonardo da.
Study of Oak Leaves. Autotype. $7\frac{1}{2}$ by 6 ins. 3*s.* 3*d.*

Detmold, E. J.
Baby Birds. Twelve reproductions in colour after water-colour drawings. (The Medici Society.)

1.	Redshank	7 by $5\frac{1}{2}$ ins.	
2.	Blue Tit	7 by $5\frac{3}{8}$ ins.	
3.	Long-tailed Tits	$5\frac{1}{2}$ by $6\frac{1}{2}$ ins.	
4.	Willow Warblers	7 by $5\frac{1}{2}$ ins.	
5.	Whitethroat	7 by $5\frac{1}{2}$ ins.	
6.	Yellow Hammers	7 by $5\frac{1}{2}$ ins.	
7.	Chicken	7 by $5\frac{1}{2}$ ins.	1*s.* each or 10*s.* 6*d.* the set
8.	Duckling	7 by $5\frac{1}{2}$ ins.	
9.	Cygnet	7 by $5\frac{1}{2}$ ins.	
10.	Long-eared Owl	7 by $5\frac{1}{2}$ ins.	
11.	Jays	$4\frac{3}{4}$ by $5\frac{1}{2}$ ins.	
12.	Magpies	7 by $5\frac{1}{2}$ ins.	

Smith, Reginald.
The Home of the Sea Birds. Mezzochrome proof. $15\frac{5}{8}$ by $22\frac{1}{2}$ ins. £1 1*s.*

Waite, E. W.
Hawthorn Blossoms. (Burlington Proof.) 18 by 24 ins. £1 7*s.* 6*d.*

Baudoin, Paul Albert. Living Artist. Decorative Frieze. (Set of Five in colour.) Representing :—(1) Ploughing. (2) Harrowing. (3) Mowing. (4) Carting. (5) The Produce of the Field. Ink Photos (*The Architect*). Nos. 1 to 4, size 9 by 28 ins. No. 5, $8\frac{1}{4}$ by 23 ins., 10*s.* 6*d.*

C.

Professor F. W. Gamble suggests the production of a new set of pictures on Biological subjects adapted from photographs based on a definite ecological classification, and cites the following suitable sources of illustrations, which, however, he thinks might be much extended by other biologists.

1. 'Vegetations-bilder,' Karsten and Schenck. (Jena: Gustav Fischer. One doz. vols., 1900 to 1914.)
2. Kerner and Oliver. 'The Natural History of Plants.' (London: Blackie & Son. Two vols. £1 10*s.* net.)
3. Langeron. 'Atlas Colorié des plantes et des Animaux marines des Côtes de France.' (Hachette et Cie.)
4. Saville Kent. 'Great Barrier Reef of Australia.'
5. Agassiz. 'Coral Islands.' Harvard Museum Memoirs.
6. J. G. Millais's Books. (London: Longmans, Green & Co.) 'British Diving Ducks.' Two vols. £12 12*s.* 'Wild Fowler in Scotland.' £1 10*s.* 'Mammals of Great Britain.' Three vols. £18 18*s.*
7. Thorburn's 'British Birds.'
8. 'Fur, Feather and Fin Series.' E. T. Watson. (London: Longmans, Green & Co. £2 10*s.* complete.)
9. Wolf's Animal Studies, illustrating papers published in the Proceedings and Transactions of the Zoological Society of London.
10. 'The Life and Habits of Wild Animals.' Wolf. (Macmillan & Co.—Now out of print. English edition, 1874, 18*s.* 6*d.* German edition, 1879, 10*s.* 6*d.*)
11. Haeckel's 'Kunst-Formen der Natur.' Fischer. (Excellent for decorative design.)
12. Macmillan's Wall Pictures of Farm Animals. (London: Macmillan & Co.)
13. 'The Book of Nature Study' Edited by Bretland Farmer. (London: Caxton Publishing Co. Six vols. £4 16*s.*)
14. Hensen 'Pflanzen Geograph. Tafeln.' (Berlin-Steglitz 1899. Neue Photogr. Gesellschaft.) Permanent carbon prints of such subjects as a Mediterranean Olive Grove, a Date Palm Grove in a N. African Oasis, Tropical Rain Forest, &c.
15. Plant Geography. Schimper. (Oxford: Clarendon Press.)
16. Monograph of the Pheasants. Beebe. (Witherby.)
17. Monograph of the Polychæt Annelids. McIntosh. (Ray Society.)

Mr. R. Catterton-Smith, Municipal School of Art, Birmingham, considers the drawings of the late John Swan, R.A., and the enlarged photographs from Bewick's famous 'Book of Birds' as appropriate subjects. He also suggests photographs of the animals in the Assyrian Basement of the British Museum and of the Japanese Birds in the British Museum.

Mr. O. H. Latter directs attention to the excellent whole-plate photographs which have appeared during the last eight years in 'Wild Life' (now out of print), and the fine series by Mr. Douglas English, dealing with British creatures.

Mention may also be made of the striking animal photographs by Mr. Gambier Bolton, published by the Autotype Co., Oxford Street, London.

Mr. J. Reeves, Leighton House, Leighton Buzzard, has prepared a series of seven long charts showing graphically the 'Evolution of the Earth and its Organisms,' but they have not yet been published.

V. CHEMISTRY AND PHYSICS.

Pictures, etc., for Decorative and Educational Purposes in Science Buildings.

There are few satisfactory reproductions of pictures suitable for the purposes of the Committee, and not many pictures themselves of the type required. A good example is the fresco already referred to, page 378, painted by Ford Madox Brown of John Dalton, assisted by a number of boys, collecting Marsh Gas from a woodland pool. Such a picture presents not only the portrait of an eminent man of science, but it attracts by its human interest and suggestiveness. Artists might be found who would undertake the painting of a few such arresting subjects connected with the life and work of some of our great physicists and chemists. Their reproductions would form admirable pictures for our Science Halls and Libraries. Failing these the reproduction of a few good portraits such as those in the National Portrait Gallery might be made; but their value for decorative purposes would depend a great deal on the quality of the reproduction.

The chief portraits of Scientific Men in the National Portrait Gallery are given below with their catalogue number and artist.

1.	Sir Humphry Davy	No. 1573 by Sir T. Lawrence
2.	Michael Faraday	269 by T. Phillips
3.	Charles Darwin	1024 by Hon. J. Collier
4.	Sir Charles Lyell	1387 by L. Dickinson
5.	Sir William Huggins	1682 by Hon. J. Collier
6.	T. H. Huxley	1742 by Hon. J. Collier
7.	James Clerk Maxwell	1189 by Photography
8.	Sir Isaac Newton	558 by J. Vanderbank
9.	Sir Richard Owen	938 by H. W. Pickersgill
10.	Joseph Priestley	175 by Mrs. Sharples
11.	Lord Kelvin	1708 by Miss A. G. King
12.	Sir William Crookes	1846 by Paul Ludovici
13.	A. R. Wallace	1765 by Paul Ludovici
14.	John Tyndall	1287 by S. McLure
15.	Group of Eminent Science Men, 1807-8	1075 by Sir J. Gilbert
16.	Sir Michael Foster	Room xxv by Hon. John Collier
17.	Sir William Henry Perkin	xx by Sir Arthur S. Cope
18.	Isaac Barrow	xxviii Pencil drawing by David Loggan

The Art for Schools Association is prepared to supply photographs of the above up to 30 ins. by 35 ins. at a reasonable cost.

Other suitable paintings are :—

1. Pasteur in his Laboratory, by Albert Edelfelt.
2. The Orrery, by Joseph Wright.
3. The Air Pump, by Joseph Wright.
4. Astronomers using Telescopes at Greenwich. Etching by Francis Place.

The following series of portraits of Scientific Worthies, which have appeared in *Nature*, are published by Messrs. Macmillan & Co., Ltd. :—

Steel and Photogravure portraits.

The engraved surface of the Steel portraits averages $2\frac{1}{2}$ by 3 ins., and that of the Photogravure portraits 5 by $6\frac{1}{2}$ ins.

Michael Faraday	Sir Richard Owen
Thomas Henry Huxley	James Clerk Maxwell
Charles Darwin	James Prescott Joule
John Tyndall	William Spottiswoode
Sir George Gabriel Stokes	Arthur Cayley
Sir Charles Lyell	Sir C. W. Siemens
Sir Charles Wheatstone	John Couch Adams
Sir Wyville Thomson	James Joseph Sylvester
Robert Wilhelm Bunsen	Dmitri Ivenowitsh Mendeléeff
Lord Kelvin	Louis Pasteur
Baron A. E. Nordenskjold	Sir Archibald Geikie
Hermann L. F. Helmholtz	Lord Lister
Sir Joseph Dalton Hooker	Stanislao Cannizzaro
William Harvey	Von Kölloker
Sir George B. Airy	Simon Newcomb
J. Louis R. Agassiz	Sir William Huggins
Jean Baptiste Andre Dumas	Lord Rayleigh
Eduard Suess	Dr. A. Russel Wallace
Sir William Crookes	Professor H. Poincare
Sir William Ramsey	Sir J. J. Thomson

Sir Norman Lockyer

Proof impressions of any of the above, on India paper, may be had from the Publishers at 5*s*. each. Office of *Nature*, St. Martin's Street, London, W.C. 2.

VI. ENGINEERING AND TRADES.

Pictures of War Work in England. Joseph Pennell. (Fifty-one reproductions of drawings and lithographs in black and white of Munition Works, 6 by 4 ins. and 6 by $7\frac{1}{2}$ ins. London: W. Heinemann. Price 6*s*.)

Do. enlarged lithographs. Messrs. Colnaghi and Obach. London: New Bond Street. Price £3 3*s*. each.

War Work in America. Joseph Pennell. (J. B. Lippincott Co., London and Philadelphia. Price 9*s*.)

Do. enlarged lithographs. (Keppel & Co., New York.)

The Panama Canal. Joseph Pennell. (London: W. Heinemann.—Now out of print.)

Pictures in the Land of Temples. Joseph Pennell. (London: W. Heinemann. Price 5*s*. net.)

The Wonder of Work. Joseph Pennell. (London: W. Heinemann.—Now out of print.)

Decorative Frieze in 18 pieces, representing the Trades. Galland, P. V., 10 by 15 ins £1 5*s*. the set. (Recommended by the Art for Schools Association.) Architecture, Founding, Smith's Work, Stone Cutting, Brick Laying, Joinery, Carpentry, Turning, Pottery, Glass Working, Sculpture, Painting, Textile Decoration, Musical Instrument Making, Metal Working, Engraving, Armourer's Work, Arboriculture (Hotel de Ville, Paris), Ink Photos (*The Architect*).

Motor Garage and Motor Car. 262, Royal Academy, 1921.

Railway Sidings and Factory Chimneys. 654, Royal Academy, 1921.

'The Ages Meet' (Welding Tramway Rails, by Cleopatra's Needle, Embankment). Stanhope Forbes. 156, Royal Academy, 1921.

Photographs of the Frieze around the Albert Memorial.

Mr. C. H. Creasey, Mellor Lodge, Marple, has also compiled a list of photographs illustrative of engineering features and including many remarkable prints of bridges.

VII. GEOGRAPHY.

A selection of photographs of land forms, etc., with the idea that they shall be solely or primarily decorative, should at the same time illustrate merely the simplest forms. Any such list is obviously capable of extension. No existing series of enlargements has been found to be selected precisely from this point of view, but that published

by Messrs. G. Philip & Son. 32 Fleet Street, London, E.C. 4, includes several photographs, admirable in themselves, which more or less nearly approximate to the ideas of the Committee. These are included in Messrs. Philips' series 1, and are as follows :—

No. 3. A Waterfall (Aysgarth), Lower Fall, Yorkshire.
5. Chalk Cliffs and Natural Ridge on the Dorset Coast.
7. Volcano. (The Crater of Mount Tarawera, New Zealand.)
10. Glacier and Mountains. (The Mer de Glace, Alps.)
11. Antarctic Ice and Snow Views.
14. Desert. (Tunis.)
16. Plain in Uruguay.

In addition to the above, a few photographs may be noted which would appear specially suitable for enlargement for the purpose which the Committee has in view. These photographs are copyright and have been used in various publications, the references to which are given. These references are not, of course, to enlargements, but to plates in books.

No. 1. Rocky Mountains in British Columbia ; a cirque ; lake ; coniferous woods ; precipitous ridge. Phot. Office of the High Commissioner for Canada. See Oxford Survey of the British Empire, London : Oxford University Press. Six vols., 70*s*. Vol. 4, page 176.
2. River entering Alluvial Plain. (River Tay in Carse of Gowrie, Scotland.) Deciduous Woods. Phot. Wilson Bros., *ibid.*, vol. 1, page 27.
3. Savanna in South Africa. Phot. Office of the High Commissioner for South Africa, *ibid.*, vol. 3, page 97.
4. Tropical Palm Forest in North Borneo. Phot. Visual Instruction Committee, *ibid.*, vol. 2, page 434.
5. Downs in Victoria, Australia. Phot. Office of the High Commissioner for Australia, *ibid.*, vol. 5, page 14.
6. A Highland. (The Black Forest.) Phot. Photochrome Ltd., Clarendon Geography. Vol. 1, page 68. (London : Oxford University Press. 2 vols. 4*s*. each.)
7. The Khaibar Pass, India. Phot. Visual Instruction Committee. Oxford Survey of the British Empire. Vol. 2, page 18.
8. The Gorge of the Middle Rhine, with flood plain beyond. Phot. Photochrome, Ltd. Clarendon Geography. Vol. 1, page 54.
9. Snowdon, North Wales, showing watershed, screes, etc. Phot. W. H. Spooner, Oxford Survey of the British Empire. Vol. 1, page 82.

Mr. Herbert G. Ponting has many striking photographs illustrating Antarctic subjects which could be enlarged for school use. He also suggests the suitability of some of the Scott Expedition pictures for the same purpose. (An exhibition of Mr. Ponting's Photographic pictures taken during the British Antarctic Expedition, 1910–13, was arranged by the Fine Art Society, 148 New Bond Street, London, and a catalogue, priced, can still be obtained.)

VIII. GEOLOGY.

The geological photographs included in the following list are of a pictorial character and illustrate subjects easily understood and mostly lying on the border-line between Geology and Geography. The more strictly geological subjects are generally of too technical a character for the purpose contemplated.

1. Fingal's Cave, Staffa. Illustrating marine erosion of columnar and non-columnar basalt. J. Valentine & Co., Ltd., Dundee. (LIII.)
2. North Golton Castle, Stromness. Illustrating the production of a sea stack by erosion along joints. J. Valentine & Co., Ltd., Dundee. (1047.)
3. Mist Effect on Scuir nan Gillean, Skye. Illustrating bare and nearly precipitous-sided gabbro mountains. Abraham Brothers, Keswick.
4. Giant's Causeway, Antrim. Columnar basalt. R. Welch, Lonsdale Street, Belfast. (1652.)
5. The Fairy Glen, Gough's Cave, Cheddar. Illustrating the form of stalactites. Pictorial Stationery Company, London.
6. Ingleborough and the Limestone Plateau below. Illustrating the soil-less character of a limestone plateau. S. H. Reynolds, The University, Bristol. (1919–47.)
7. The Scuir of Eigg. Illustrating differential erosion. A. S. Reid, Trinity College, Glenalmond, Perth, N.B. (2240.)

8. Fossil Forest, Partick, Glasgow. Depicting stumps of coal trees in position of growth. J. R. Stewart, 32 Boyd Street, Largs, Ayrshire.
9. Summit of Rough Tor, Camelford, Cornwall. Illustrating weathering of granite. H.M. Geological Survey. (A481.)
10. Grand Canyon of Arizona. Illustrating river erosion in an arid climate. Detroit Publishing Company. (10452.)
11. The Drei Zinnen. Illustrating erosion along joints on a colossal scale. B. Lehrburger, Nuremberg. (1561.)
12. Vesuvius. pl. 17625. From Album illustrating the eruption of 1906. E. Ragozino, Naples.

Mr. J. Allen Howe, Geological Museum, Jermyn Street, London, S.W.1, has made a selection of photographs of geological subjects prepared and catalogued by the Geological Survey (England and Scotland). These can be obtained as prints, slides, negatives or enlargements at reasonable prices.

IX. HISTORY.

'Wall Pictures of British History.' London: Longmans, Green & Co. 3*s*. each. Recommended by Professor F. J. C. Hearnshaw. (*Cf.* pages 377 and 378.)

X. METEOROLOGY.

Illustrations of meteorological principles on the scale of educational pictures mostly take the form of wall-maps or diagrams which are not exactly pictures.

There are many pictures by distinguished artists which represent various aspects of weather, but so far as is known none of them give effective expression to the principles of meteorology which is the study of weather. If, therefore, educational pictures of meteorological subjects are desired they are not stock articles, but have to be 'purpose made.' With that object the following tentative suggestions may be put forward. Pictures of the catastrophes of weather, such as the damage done by floods or tornadoes, which is generally more impressive than picturesque, have been avoided.

1. The Exploration of the Upper Air.—On the walls of the staircase of the Meteorological Office is an effective picture representing the results of the exploration of the upper air. It is reproduced on small scale in 'The Weather Map,' Meteorological Office publication No. 225 (*i*), pages 46–47.
2. British Westerly Weather.—In one of the cases on the same staircase are two photographs of a windy sky which could be enlarged to form a fine picture. In the library of the Office is a fine water-colour drawing of the same type of cloud by Mr. A. F. Purkin.
3. A Storm in the Distance.—A photograph of a fine specimen of a cumulo-nimbus, or thunder-cloud, with a veil of false cirrus reproduced in Meteorological Office Glossary, page 65.
4. A Valley Fog on a Winter Morning.—Meteorological Glossary, page 65. The same aspect as No. 3, but with fog instead of thunder-cloud. There are many striking photographs of similar fog in Californian valleys in American Meteorological publications.
5. The Grey Skies of Britain.—Photograph of strato-cumulus cloud:
   (*a*) from below;
   (*b*) from above. (Photographs of cloud-sheet from an aeroplane.) Glossary, page 64.
6. The Red Skies of Sunset.—Reproductions of the coloured sketches of the sunsets which followed the Krakatoa eruptions. (Report of the Krakatoa Committee of the Royal Society, Frontispiece.)
7. Rime.—Trees covered with feathery ice. (Observer's Handbook, 1919 edition, Plate I., page 54.)
8. A Snow Scene.—Photographs of exceptional occasions.
9. River Ice.—Snowfalls and frozen Thames or other river which may be found in the album collections of the Meteorological Office or the Royal Meteorological Society.
10. The Halo Ring. Sketches by G. A. Clarke, of the Observatory, Aberdeen. (Report of the Meteorological Committee, 1914–15, Cd. 8028, 1915, facing page 54.)

11. The Rainbow.—An accurately coloured sketch of a rainbow in natural surroundings.
12. Northern Lights.—Correct sketches of streamers and curtain auroras to be compiled from Professor Stormer's pictures.

Coloured plates and photographs of clouds shown in 'Clouds.' G. A. Clarke. London: Constable & Co. 20*s.* net, 1920. (*Cf. Nature*, March 19, 1921.)

XI. PICTURES PUBLISHED PARTLY AS ADVERTISEMENTS BUT OF AN EDUCATIVE NATURE.

Charts of Prehistoric Life. Four coloured charts illustrating the life forms characteristic of four Prehistoric Ages. Size 20 by 24½ ins., price 7*s.* 6*d.* Published by Messrs. J. & J. Colman, Ltd., Norwich.

Great Central Railway. The King's Dock, Immingham. 'England's latest Deep-Water Port.' Coloured charts in six sections, each 5 ft. by 3 ft. 4 ins. Supplied free to schools.

Great Western Railway. 'See Your Own Country First.' (Cornwall or Italy.) Coloured chart, 4 ft. by 3 ft. 3 ins. Supplied free to schools.

Metropolitan Railway. 'A Glade in Chorley Wood.' 20 ins. by 17 ins. 'Autumn Foliage in Herts and Bucks.' 4 ft. 3 ins. by 3 ft. 4 ins. Two posters. 'A Country Scene,' 17 ins. by 17 ins., and various other scenic posters all in colours.

London and North-Western Railway. Map of the Railway—coloured chart on rollers, 40 ins. by 32 ins.

Midland Railway.

'A Donegal Trout Stream'	50 by 40 ins. 5*s.*
'Interior of St. Pancras Station'	80 by 60 ins., 5*s.*
Ditto	50 by 40 ins., 3*s.* 6*d.*
Ditto	26 by 19 ins., 2*s.* 6*d.*
'Derbyshire's Natural Garden'	50 by 40 ins., 3*s.* 6*d.*

XII. SETS OF PICTURES AND CHARTS.

Some of these are listed in publishers' catalogues as follows:

Botany and Nature Study	G. Philip & Son, Ltd., London.
Ditto	Longmans, Green & Co., London.
Ditto	J. M. Dent & Sons, Ltd., London.
Ditto	A. Brown & Sons, London.
Ditto	G. W. Bacon & Co., Ltd., London.
Ditto	Macmillan & Co., Ltd., London.
Geography	G. Philip & Son, Ltd., London.
Ditto	G. W. Bacon & Co., Ltd., London.
Human Life	G. Philip & Son, Ltd., London.
History	Longmans, Green & Co., London.
Ditto	G. Philip & Son, Ltd., London.
Ditto	Adam and C. Black, London.
Ditto	Macmillan & Co., Ltd., London.

THE MURAL PAINTINGS AND RESTORATIONS OF EXTINCT ANIMALS IN THE HALL OF THE AGE OF MAN, AMERICAN MUSEUM.

(*Information received too late to be included in section* 1, *page* 379.)

Mural Paintings.

1. 'The Neanderthal Flint Workers.' Dordogne, France. With herds of Woolly Rhinoceros and Woolly Mammoth in the distance. (Not yet completed.)
2. 'Crô-Magnon Artists of Southern France.' At work on a Palæolithic Mural in the Cave of Font-de-Gaume, Dordogne, France. (No. 37952, 9 by 4¼ ins.)
3. 'The Neolithic Stag Hunters of the New Stone (Campignian) Age.' (No. 37953, 9 by 4¼ ins.)

4. Murals of the Four Seasons in the Old Stone Age near the close of the Glacial Epoch in the Northern Hemisphere :—

   Midwinter : 'The Woolly Rhinoceros in Northern France.' With Saiga Antelope and Mammoth in the distance. (No. 37227, $8\frac{1}{4}$ by $3\frac{1}{2}$ ins.)

   Late Winter or Early Spring : 'The Reindeer and Mammoth on the River Somme.' Representing a northward march. (No. 37230, 9 by $1\frac{3}{4}$ ins.)

   Midsummer : 'The Mastodon, Royal Bison, and Horse on the Missouri River in the latitude of Kansas.' (No. 37225, 9 by $1\frac{3}{4}$ ins.)

   Autumn : 'The Deer-moose, Tapir, and Giant Beaver in Northern New Jersey.' (No. 37953, $8\frac{1}{2}$ by $3\frac{1}{2}$ ins.)

5. 'A Loess Storm on the Pampas of Argentina.' (No. 37228, 8 by $3\frac{1}{4}$ ins.)

The American Museum of Natural History is prepared to furnish prints or slides of the Murals (reference numbers and sizes as above) and Restorations of Extinct Animals, for educational purposes, at prices which approximately cover cost of production. Application should be made to :—Mr. G. H. Sherwood, Curator of Public Education, American Museum of Natural History, New York City, U.S.A.

Restorations of Extinct Animals. By Charles R. Knight.

Subject.	Neg. No.
Ichthyosaurus	35808
Tylosaurus and Portheus (Mosasaru)	46624
Allosaurus	35804-5-6
Brontosaurus	35799
Ceratosaurus	19758
Trachodon	35789
Diplodocus	35807
Dryptosaurus	35781
Ornitholestes and Archæopteryx	35820
Stegosaurus	35824
Tyrannosaurus and Triceratops	35827
Phororhachos (missing)	19790
Arsnoitherium and Pterodon	35832-33
Giant Beaver, Deer, and Moose	37953
Borophagus	35836
Brontotherium platyceras	35774
Cænopus	35748
Cervalces	35794
Coryphodon	35796
Deer and Mammoths	35940
Dinictis and Protoceras	35817
Dinoceras — Mid-Eocene Landscape	35941
Dolichorhinus, Eobasileus, Protitanotherium, Manteoceras, Harpagolestes—Heads	28642
Elephas colombi	35837
Elephas imperator	35819
Elephas primigenius (Mammoth)	35930-35
Elotherium	35772
Eobasileus, Protitanotherium	28462
Dolichorhinus, Manteoceras, Eosiren and Manatus	28455
Eotitanops and Brontotherium	35773
Hoplophoneus	35797
Hyracodon	35776
Hyrax, Megalohyrax, Rhinoceros	28454
Manteoceras and Dolichorhinus	35835
Mastodon americanus	35779
Megaceros	35802
Mesonyx	35777
Metamynodon	35771
Mœritherium, Palæomastodon, Elephas	35830-31
Oxyæna and Eohippus	35810
Palæosyops	46623
Phenacodus	35795
Patriofelis	35765
Platygonus Leptorhinus	35811
Protoceras	35770
Prozeuglodon	35828
Pterodon and Arsnoitherium	35832-33
Smilodon	218965, 35809
Teleoceras	35798
Trilophodon productus	35803
Uintatherium	35775
Evolution of the Horse	35829
Eohippus venticolus	35767
Eohippus and Protorohippus	28486
Equus scotti	35826, 28481
Hippidium	28482
Hypohippus	35816, 28484
Neohipparion	35821, 28483
Mesohippus	35825, 28485

Size of Negatives.

Nos. beginning with 3 are 8 by 10 ins.
" " " 1 " 5 by 7 ins.
" " " 2 " 4 by 5 ins. or $3\frac{1}{4}$ by $4\frac{1}{4}$ ins.
" " " 4 " $6\frac{1}{2}$ by $8\frac{1}{2}$ ins.

PHOTOGRAPHS.

Size.	Unmounted. $	On Plain Mounts. $
$3\frac{1}{4}$ by $4\frac{1}{4}$	·06	·10
$3\frac{1}{4}$ by $5\frac{1}{2}$	·10	·15
4 by 5	·10	·15
5 by 7	·15	·20
$6\frac{1}{2}$ by $8\frac{1}{2}$	·20	·30
8 by 10	·30	·40
11 by 14	·50	65

ENLARGEMENTS, BLACK AND WHITE.

Size.	Unmounted. $	On Plain Mounts. $
8 by 10	·50	·60
11 by 14	1·00	1·15
14 by 17	1·50	1·65

SEPIA.

Unmounted. $	On Plain Mounts. $
·75	·85
1·50	1·65
2·25	2·40

SLIDES.

	$
Uncoloured	·50
Coloured	1·25

An excellent account of the Murals and the epoch which they illustrate, together with mention of the Restorations, is given in a descriptive pamphlet: 'The Hall of the Age of Man' (H. F. Osborn, American Museum of Natural History, Guide Leaflet No. 52, May 1921).

An International Auxiliary Language.—*Report of Committee appointed to inquire into the Practicability of an International Auxiliary Language* (Dr. H. FORSTER MORLEY, *Chairman*; Dr. E. H. TRIPP, *Secretary*; Mr. E. BULLOUGH, Professor J. J. FINDLAY, Sir RICHARD GREGORY, Mr. W. B. HARDY,[1] F.R.S., Dr. C. W. KIMMINS, Sir E. COOPER PERRY, Prof. W. RIPMAN, Mr. NOWELL SMITH, *and* Mr. A. E. TWENTYMAN).

I. Introduction.

Among the many problems bequeathed to us by the war, that of placing international relations upon a more stable basis is admittedly of outstanding importance. We now realise far better than we did how closely interwoven are the interests of all civilised nations and communities, and how one nation's ignorance of another's character may lead to the direst consequences. Hence, mutual intelligibility as the condition precedent of mutual understanding and concord must be cultivated more intensively than hitherto; for nations, like individuals, learn to bear and forbear, as well as to progress, in proportion as their knowledge grows and their horizon widens. It is a truism that modern science has revolutionised the material conditions of our existence, and that, in particular, the development of means of inter-communication—railway, steamship, telegraph—has added to the amenities of life; but, unfortunately, opportunities for strife have increased almost *pari passu*, and what is now required is some means of attaining greater mutual knowledge as an insurance against future conflicts and misunderstandings. Experimental science has forged the wheels of civilised life; can humanistic science provide a lubricant to make them run more smoothly?

In the opinion of many, a practicable means of increasing such mutual knowledge, and at the same time of effecting great economics in time, work, and money, would be the adoption of an auxiliary language as a means of international communication—auxiliary because, unlike the fantastic ideal of a 'universal' language, it would not be intended to replace existing national languages for domestic use. An international auxiliary language, it is felt, should above all be easily acquired; it should be sufficiently precise and flexible for all ordinary purposes; and, if possible, it should be neutral from the standpoint of nationality. The practicability of its adoption would depend largely upon the fulfilment of these requirements, and its realisation mainly upon the recognition by the peoples of its desirability. Although many attempts have been made to solve the problem during the past two hundred years, and particularly in the last thirty, the fact that in no instance has success been generally recognised should constitute no barrier to its renewed investigation. As has been indicated above, the present time seems particularly opportune to revive the question, and even if the result be negative, it should at any rate save the time of future inquirers.

The resuscitation of this problem in official and academic circles was due primarily to the initiative of the United States' representatives at the meeting of the International Research Council which was held in Brussels in August, 1919. On that occasion the question was raised of instituting an international abstract journal of chemical literature, but no agreement could be reached concerning the language or languages in which such a journal should be published; and out of this there arose a fundamental discussion of the whole problem of an international language. In the end the following resolutions were adopted unanimously:—

(*a*) That the International Research Council appoint a committee to investigate and report to it the present status and possible outlook of the general problem of an international auxiliary language.

(*b*) That the committee be authorised to co-operate in its studies with other organisations engaged in the same work, *provided* that nothing in these resolutions shall be interpreted as giving the committee any authority to commit the Council to adhesion to or approval of any particular project.

[1] Mr. Hardy was chairman of the Committee until May, 1921, when he retired owing to pressure of work.

Dr. F. G. Cottrell, formerly Director of the United States Bureau of Mines and now Chairman of the Division of Chemistry and Chemical Technology of the National Research Council, was appointed Chairman of the International Committee, and among the other members appointed were :—Prof. Charles Moureu, of the Collège de France, Prof. R. Nasini, University of Pisa, Prof. A. Tanakadate, University of Tokyo, and Dr. Paul Otlet, of the Institut International de Bibliographie, Brussels. The above-named were subsequently appointed chairmen of their respective national committees. At the suggestion of Prof. H. H. Turner it was left to the British Association to nominate the British representatives, and the committee was given power to add to its number. As an outcome of this, the present committee was appointed at the Bournemouth Meeting of the Association (1919), with the intent that its chairman, at least, should represent Great Britain on the committee of the International Research Council.

This Committee has been assisted in its deliberations by a special committee of the Classical Association. In the United States a joint committee has been set up by the National Academy of Science, the American Association for the Advancement of Science, and the National Research Council. In addition, the following bodies have appointed, or authorised the appointment of, similar committees :—The American Council on Education (including the Modern Language Association) ; the American Philological Association ; the American Council of Learned Societies ; the American Classical League ; and study-groups have been organised at a number of universities. As Commerce is regarded as one of the most important fields for an international auxiliary language, steps have been taken to awaken the interest and secure the collaboration of Chambers of Commerce as well as of individual business men.

It will thus be seen that efforts are being made to enlist the sympathy and co-operation of very diverse interests, in order that the problem may be approached in a broad and comprehensive manner. The aim of the International Committee is gradually to build up a large and competent group of investigators, having both theoretical and practical knowledge of the subject, from which a number will be selected to form a central international organisation, preferably under the League of Nations, which shall be empowered to make the final selection of the international auxiliary language, if feasible, and to take measures to secure for it the greatest possible degree of stability.

Having approved unanimously of the desirability of an I.A.L. (International Auxiliary Language), the Committee turned its attention to the advantages and disadvantages of the following three types :—

(A) A dead language, *e.g.*, Latin.
(B) A national language, *e.g.*, English.
(C) An invented or artificial language, *e.g.*, Esperanto and Ido.

In considering the claims of Latin, the Committee received much assistance from the special committee of the Classical Association, and also information and advice from a number of Latin scholars, including Monsignor W. F. Brown (of the Catholic Church, Vauxhall, S.E.), Prof. R. S. Conway, Father A. L. Cortie, the late Prof. L. C. Miall, F.R.S., Prof. J. P. Postgate, Dr. W. H. D. Rouse, and Dr. L. Storr-Best. The claims of English have been ably brought before the Committee by one of its members, Prof. W. Ripman, and also in the form of literature published by the Northern Peace Union and the English Language Union. Information concerning invented languages has been supplied mainly by the British Esperanto Association and the International Language (Ido) Society of Great Britain. To all of these the Committee wishes to express its thanks and indebtedness.

In order to confine the inquiry within moderate limits, Latin, English, Esperanto, and Ido were selected for consideration, and it was unanimously agreed that, for the purpose of this Report, specialists should be asked to present their respective claims as concisely as possible. These claims are set out below.

II. The Claims of Latin.

The two essential requirements of an international auxiliary language seem to be :—(*a*) that it should be easily understood ; (*b*) that it should not be easily misunderstood. In both these respects, for use by any communities which share, or desire to share, the civilisation of Europe, Latin has advantages which may well be thought decisive, over any artificial 'language' such as Esperanto, over all the Romance languages, over German, and over English.

1. *Standard of Meaning.*—When any doubt arises as to the meaning in which a Latin word is used, a Latin dictionary, even a small one, will quickly show how the word is used by the standard authors in actually existing books. The same is true of any real modern languages which possess a literature and dictionaries competently written. But it is not true of any artificial language. Putting aside the names of concrete things and persons, which a mere vocabulary can provide without ambiguity, such as *chair, railway, wine,* or *king, soldier, mother*—there can be no certain guide to the meaning of any term—especially of words denoting more abstract ideas, which are often of great importance, such as *to compensate, to compromise*—except in an express declaration from the inventor of the language. Even if he is supposed omniscient, he cannot be always and everywhere accessible; even if he were both, he cannot be immortal.

Few of those who are familiar from actual study with the complex conditions and stages by which a word is created, develops new meanings, or passes out of use, will believe that an artificial language can serve any but limited purposes or be maintained beyond a limited time. Applied to serious use in business, it may always involve dangerous and costly ambiguities. In Latin, just because it is no longer in colloquial use, the meaning of a word is fixed and cannot be altered.

2. *Brevity.*—The inflexions of Latin make it possible to express any given meaning in fewer words than in any modern language. Two examples will suffice.

English.	*French.*	*Latin.*
At what price did you buy it?	Combien l'avez vous payé?	Quanti emisti? (or quanti emistis? according as *you* is sing. or pl.; so we escape another ambiguity).
Who made profit out of it?	Qui en a tiré le profit?	Cui bono fuit?

It is to be observed in the second case that the French version involves an idiomatic use of one of the pronominal adverbs, whose meaning and position are well-known stumbling blocks to all who learn to write French.

3. *Phonetic Spelling.*—In the restored pronunciation now adopted in all English schools (except Eton and Westminster), Latin spelling offers no difficulty since it is entirely phonetic. This is a very great advantage as compared with all living languages, especially with English.

4. *Ease of Acquirement.*—It is important that the I.A.L. should be one whose vocabulary carries its own meaning to the speakers of as many languages as possible. As the parent of all the Romance languages, of a great part of English, especially English that is written, and of a considerable part of German, Latin has great advantages over any other language. The speakers of English and of the Romance languages easily recognise in Latin the originals of their own derivatives; whereas they do not so easily recognise the parallel equivalents in the other modern languages. The Latin *via* is more intelligible both to English and Italians and Spaniards than the French *voie*, and no more difficult to the French than either the Italian or the Spanish and English *via*. Similarly every Englishman and Frenchman understands the Latin word *includere* because of the English words *include* and *inclusive,* and the French *enclos, inclusion*; but he would not so easily recognise the Italian words beginning with *inchiu-*; on the other hand, Italians who knew any European language other than their own would recognise the Latin at once because they will have inevitably become familiar with the change of the sound *cl* which has taken place in Italian. Similarly the Latin *facere* is more recognisable than the Spanish *hacer* or the French *faire.* The following are other examples, and these could be multiplied indefinitely :—

English.	*Spanish.*	*Italian.*	*French.*	*German.*	*Latin.*
faith	fe	fede	foi	Vertrauen	fides
leaf	hoja	folio	feuille	Seite. Blatt	folium
iron	hierro	ferro	fer	Eisen	ferrum
strong	fuerte	forte	fort	stark	fortis
middle	medio	mezzo	milieu	mitten	medium

In one or two of these cases, though the actual words in English and German are totally unlike the Latin word, so that the English or German form does not suggest its meaning to the speaker of any Romance language, yet there are derivatives in

English *fidelity, fortitude,* and in German like *fidel* and *foliant,* which would suggest the meaning of the Latin word to speakers of English and German.

5. *Capacity for New Development.*—The machinery for forming new words in Latin is well established and well known. For example, the use of the endings *-atio* to denote a process; *-tia* to denote a quality; *-um* a concrete thing; *-are* to denote making; *-isare* to denote impregnating one thing with another: *-atum* to denote a product; and *-tor* to denote a person acting. A great deal of new coinage will be necessary whatever language be adopted; if Latin is chosen, the Classical Association will no doubt be prepared to appoint a small standing committee, which could meet periodically to draw up lists of suitable Latin equivalents for any terms, however numerous, which might be submitted to it.

6. *Existing Use.*—In Botany, Anatomy, Zoology, and Medicine all technical terms are in Latin by international agreement. In Chemistry all the terminology is based on Latin or Græco-Latin endings, for example, *Sulphate, Sulphide, Sulphite, Sulphuric, Sulphurous,* and nearly all the technical terms, *e.g., Molecule, Atom, Acid, Precipitate,* are derived from Latin. Further, the religious use of Latin in all Catholic countries, and in the headings of the English Prayer Book renders the look of many Latin words familiar; not to speak of the numerous Latin phrases in daily use, like *vice versa*; *sine qua non*; *nolens volens.*

7. *Lucidity as compared with English.*—Four characteristics of Modern English render it exceedingly difficult to use and still more difficult to learn. They are:—(*a*) The non-rational character of English spelling, which is more anomalous than that of any other language, though some of the anomalies are necessary to distinguish words of like sound but of different meaning.

(*b*) The almost total absence of inflexion, which involves constant doubt as to whether a particular form is a noun, or verb, or adverb, or adjective. For example, *lead, appeal, escape, move, glance, catch, hold, grasp, return, cool, second, arrest, make, press, crowd, rise, fall, temper, try, reach, desert, shock, time, pain, touch, seal, line, hammer, smile, laugh, cry, fix, set, stand.* Whether any one of these words is a verb or a noun can only be discovered by a careful study of the whole sentence, a study which in Latin the inflexions make entirely unnecessary. Take a sentence like this: 'The French use, of all these methods, only the first, and that in moderation, attempts all the time being made to improve upon it.' This kind of ambiguity a foreigner finds most baffling. The further ambiguity of the English word 'that' and the absence of any inflexions to mark the adverbial use of the accusative *time,* or the absolute use of the noun and participle (*attempts* and *being*) are additional difficulties of very common occurrence. Arising from this are many peculiar necessities of order in English which can only be indicated here (*e.g., Without support the man will fail* means something different from *The man without support will fail*).

(*c*) The disuse of hyphens in English compounds, so that any noun can be used as an adjective, has introduced a new and grave difficulty to foreigners in reading English. Phrases like *the old town road, the white ladies' dress material* are simple examples of the ambiguity which this practice creates.

(*d*) One fundamental difficulty lies in the very nature of English; it often happens that a simple noun or verb, frequently monosyllabic, is of Saxon origin, but that the corresponding derivative ideas are expressed by words of Latin origin. For example: sea: marine; to see: visible vision; to melt: fusible; to carry: portable, exportation; town: municipal; bed; clinical; life: vital; health: sanitary; death: mortal, mortality.

No difficulties of this kind arise in Latin for learners who speak any one of the Romance Languages.

III. The Claims of English.

There are strong reasons in favour of regarding English as the probable world-language of the future, not to the exclusion of national languages, but as the foreign language most likely to appeal to those who do not possess it as their mother tongue.

That English is more widely used than any other language appears even from the statistics usually quoted; yet these refer only to countries where English is the national language, and leave out of account the foreigners who have acquired a speaking or reading knowledge of it, or both. Of these, there were many, and the number

was increasing, even before the great War; during its course and since the armistice the spread of English has been quite remarkable. From every foreign country—including those recently at war with the Allies—comes the same story: English is being taught more widely in schools, private teachers of English are overwhelmed with eager students, there is a great demand for English books.

To what is this remarkable development due? It is not due to any propaganda. The attempt to force any foreign language upon the nations of the world would probably be resented, and jealousy might be aroused. The steady progress of English throughout the world is due to the growing recognition of the advantages that a knowledge of our language confers.

From the material point of view, the very fact that it is much more widely known than any other national language, and beyond comparison more widely than any artificial language, is, to many, a convincing argument in its favour. It gives English the highest value for purposes of international communication, whether in commerce or in learning. English possesses a vocabulary suitable for expressing all the requirements of civilised man, and, where necessary, new words and expressions are readily coined.

Other foreigners value a knowledge of English as the key to a vast storehouse of literature, in which—whatever their tastes—they will find much to satisfy them. There are some other literatures which, for wealth and variety, may be deemed comparable, but certainly no literature in any artificial language can bear comparison with it. There are translations of English works in many foreign languages; but in spite of this—or because of this—many learn the language because they desire to go to the original from which they can obtain that intimate appreciation which no translation secures.

The language in itself has features which render it particularly suitable for international use. As a result of its historical development it has progressed further than any other national language. This is seen in the simplification of its grammar, which presents little difficulty to the foreigner: a rough working knowledge of English is easier to acquire than that of any other natural language. The vocabulary is remarkably rich: words expressing most of the objects and actions met with in ordinary life are short and effective; and, owing to the introduction of words derived from the Romance languages and directly from Latin and Greek, the finest shades of meaning can be expressed by our vocabulary. It would be idle to deny that to acquire a perfect mastery of English is no easy task for a foreigner; but that is true of any foreign language; and not many can justly claim to be perfect masters even of their mother tongue.

It may be argued that the pronunciation and spelling of the English language stand in the way of its wide diffusion. Practical experience does not support this view as regards the pronunciation, considered apart from the spelling. English does not contain any sounds that present serious difficulty to the foreigner, if the pronunciation is taught by a scientific method, which is increasingly the case. The spelling is notoriously unsatisfactory, and a reform would be welcomed by many, on behalf of our own children, and not merely for the sake of the foreign learner. That, in spite of this rather serious handicap, English should be spreading so rapidly, is a tribute to its many merits.

If it be compared with Latin, it will be evident that English has a much simpler grammar; a vocabulary far richer, and better suited to express the many sides of modern life; and a finer and more varied literature.

If English be compared with Esperanto or any other artificial language, it has, indeed, some irregularities in its grammar, but it has a far richer vocabulary. (It should be borne in mind that the ease with which the Esperanto vocabulary is acquired by an Englishman is due to its Romance and Teutonic constituents; a Chinaman or a Zulu would find nothing in them that was familiar to him in his own language.)

As has been said above, the literature of an artificial language cannot compare with English literature.

The great facts, however, that cannot be blinked, are that, for every person who has a working knowledge of Latin or of any artificial language, there are one hundred who know English, and that English is spreading more rapidly throughout the world than any other language. It is a triumphal progress due to its intrinsic merits; it is the result of no propaganda, and no propaganda on behalf of any other language will check that progress.

IV. The Claims of Esperanto and Ido.

(*a*) ESPERANTO.

Granted the obvious need for a common auxiliary language, what are the proposed olutions of the problem ?

Latin or Greek ?—Who can make the dead bones live ? Latin (of any epoch) is incapable of expressing some of the commonest ideas of modern life. Its great difficulty forbids its use even in writing, except by the cultured few. If reformed into a dog-Latin it would please nobody—and Latin scholars least of all.

A National Language ?—Its difficulties of pronunciation, vocabulary, irregularity, idioms, etc., prevent its real mastery in other lands by all but a small minority. A 'national international language' is a contradiction in terms. The universal adoption of the language of any nation would give that nation a world-influence to which the others would not readily agree. French, Spanish, English, German, and other languages all have strong partisans. This solution is thus really an indefinite number of solutions, mutually destructive.

English ?—Not only does English suffer from all the drawbacks above named, but it is further handicapped by chaotic spelling. Its advocates, recognising this, usually postulate reformed spelling as a necessity, but are widely at variance as to what reforms should be adopted, and how to enforce them. Moreover, were English spelling made phonetic, this would render it for the average European still more difficult, because less international. At best, it takes twenty times as long to learn English imperfectly as to *master* Esperanto thoroughly. Why demand that, for our convenience, the greater part of mankind should study twenty times longer only to succeed in communicating with their fellows *less* perfectly ?

Requirements.—The international language, to be ideal, should be free from sounds difficult to pronounce, neutral for all nations, pleasing to the ear, phonetic, concise, easy to learn, flexible, exact and unambiguous, logical and regular, serviceable for all purposes, and triumphant in the severest continued tests of practical use on a large scale.

Esperanto the Solution.—Esperanto, which has been scientifically constructed for the purpose, possesses all these characteristics to a very high degree, and *it is the only language that does.* It is 'artificial' only as being the result of the conscious selection of the fittest material for the purpose, and as the railway, the telephone, and every convenience of modern civilisation are artificial. It is neutral and international in its elements, logical and regular in construction, and at least as euphonious as Italian, for which it is often mistaken. It is the easiest language in the world ; the principles of the grammar can be grasped in half-an-hour ; every rule is without exception ; the spelling is phonetic, and the dictionary incredibly slim. Nevertheless, it has unrivalled precision and flexibility, and can express the nicest subtleties of thought. It is not an untried project, but 'the living language of a living people' ; tested for over thirty years in every conceivable way, and never found wanting.

History and Organisation.—The first Esperanto grammar was published in 1887. Though progress at first was slow, there is now no country in which it has not gained a footing. There are numerous national propaganda associations, and a number of international associations with various specific objects.

Esperanto in its grammar and essentials is precisely the same now as it was at its inception, and texts written in 1887 are as legible now as then. On a fixed basis, however, there is full room for expansion in any direction, and with use, guided by a representative international committee of linguists, the language is steadily growing richer and more polished.

Esperanto Congresses.—Twelve annual congresses held each year in a different country, in which 1,000 to 4,000 persons of up to forty nationalities have met together for a week or a fortnight of business meetings carried on in Esperanto alone, have demonstrated how Esperanto annihilates the language difficulty. In these, all kinds of business and the discussion of the most varied subjects are carried on with perfect ease, without the slightest misunderstanding or failure to give adequate expression of thought.

Esperanto in the School.—The Committee has received a booklet containing reports from headmasters and inspectors of British schools in which Esperanto is taught.

These are invariably warmly favourable. The Board of Education gives grants for Esperanto classes to a large number of evening and commercial institutes.

In other lands, Esperanto has received far more official support than in this country. In the last few months, Esperanto has been introduced into schools in Geneva, Breslau, Chemnitz, Milan, and many other towns, and the educational authorities of Czecho-slovakia, Bulgaria, and Hesse have encouraged its instruction in the State schools. The Paris Chamber of Commerce is now introducing Esperanto into the commercial schools of Paris, and has published a long report on the reasons for its decision.

Literature.—The catalogue of any Esperanto publisher will show that a very large amount of literature of all kinds has been published in Esperanto—in prose and verse, both translated and original, much of which is of remarkably high literary merit. As a means of exact translation it is indeed unapproachable. The British and Foreign Bible Society has sold over 12,000 copies of the Esperanto New Testament, and will shortly publish the Old Testament. Some sixty Esperanto periodicals are now appearing.

Esperanto in Commerce.—As an example of the increasing use of Esperanto commercially, it may be mentioned that in the last twelve months the following international fairs have advertised largely, and with good results, in Esperanto :—Leipsic, Lyon, Basel (2), Padova (2), Paris (2), Helsingfors, Ghent, Frankfurt (3), Breslau, Reichenberg.

Official Support.—In the last year, resolutions in favour of Esperanto have been passed by the World Congress of the Union of International Associations, the National Institute for the Blind, the International Women's Suffrage Alliance, the Women's Co-operative Guild, the Women's International League, the Christian Internationale, the 10th International Congress of the Red Cross, by twenty-one members of the French Academy of Science, and a number of other international bodies. Among bodies that have recently used the language officially for their own purposes are the Religious Society of Friends (Quakers), the International Congress of Freethought, the Hungarian Academy of Science, the Czech Academy of Science. The Pope has just given the Apostolic Blessing to the Roman Catholic Esperanto organ. In 1920, the Finnish Parliament subsidised the Esperanto Institute of Finland to the extent of 5,000 marks, and in 1921 voted 25,000 marks for the propaganda and instruction of Esperanto in Finland.

A resolution in favour of Esperanto, presented by Senator Lafontaine, of Belgium, to the League of Nations during its meetings at the end of 1920, was subscribed by Lord Robert Cecil, and the representatives of Brazil, China, Chile, Colombia, India, Haiti, Italy, Persia, and Czechoslovakia. It was referred for consideration to a committee, which reported favourably. Unfortunately (the session being almost at an end), discussion was ruled out, and for the moment the matter remains in abeyance.

Reforms.—As Esperanto draws its elements from the principal European languages, many of its features are necessarily of the nature of compromises. The language, therefore, presents a temptation to experimenters who think that the adoption of some change in this or that detail would make it more in accordance with the principle of maximum internationality, or would render it more acceptable to this or that nation, or for use for scientific purposes. Hence, numerous projects for reforming Esperanto have been introduced to the public, some of them boldly using the principal features of Esperanto while adopting for themselves another name. Few of these projects ever reach completion, none has more than a handful of partisans, or any literature. These reforms often contradict one another ; some features of Esperanto which one rejects, another judges to be essential. They thus afford some justification for the maintenance of the language in its original form. Of these projects Ido is the best known, but, in spite of advertising and its attractiveness on a superficial view, it has met with comparatively little support. Many of its original adherents have become Esperantists or have published 'reformed' Idos, which though sometimes avoiding the more serious faults of Ido have difficulties of their own, find no following and hardly call for serious consideration as competitive projects, though containing interesting suggestions regarding details.

The claim that Ido was the creation of a competent and authoritative delegation, while Esperanto was the work of a single individual is, to say the least, misleading.

The Committee is referred to the detailed criticism of Ido already furnished to each of its members. The Esperantists stoutly resist any modification in the

fundamentals of their language at the present stage. They consider that the strength of their position lies in its stability, but they possess machinery by which any strongly desired reform that may be proposed can be carefully tested and experimented on, and, in due course, if justified, regularised by competent authority.

Conclusion.—The 'dead-language' and the 'national-language' solutions are alike inadmissible. For the rest, though various schemes for an 'artificial' language (mostly plagiaries of Esperanto) exist on paper, in the field of practice Esperanto stands alone. Unlike them, Esperanto is not an untried scheme to be experimented with and altered at pleasure *ad infinitum*, but a living language, which has stood triumphantly every possible practical test for 33 years. It is the only language which fulfils the conditions, and the more it is investigated, the stronger its claims are seen to be.

(*b*) IDO.

Advantages of Ido over Latin.—Ido can be learnt effectively even by the 'man-in-the-street' within three months. This is not the case with Latin. Most University graduates, who may be presumed to be Latin scholars, have never really effectively learnt Latin, even after years of study, otherwise Latin would be the language used at the many scientific congresses which meet from time to time, most of the delegates being graduates of their national Universities.

The impossibility of resuscitating a difficult dead language has thus been practically demonstrated.

An auxiliary language will not be a universal auxiliary language unless, like Ido, it is sufficiently simple to be learnt universally with as much ease as, for instance, phonography is learnt by people of very mediocre intelligence.

Advantages of Ido over English.—English-speaking people would all undoubtedly prefer English to be the universal auxiliary language.

Foreign-speaking people will object to such use of English (*a*) on political grounds, (*b*) because to acquire an effective use of English would require several years of study instead of the short time required to learn Ido.

While English is spoken by more people than French, French is already more used than English as a common language among people speaking different mother tongues.

In deciding upon a universal auxiliary language, one has to consider its acceptability to the great majority of mankind who do not speak English and who can save time by learning Ido instead of English, and feel that they have not conferred upon English-speaking people an undue political or business advantage to their own possible detriment.

Advantages of Ido over Esperanto.—Esperanto and Ido are stages in the development of the same idea. Ido is the later and more perfect stage. Esperanto is the invention of one man. Ido is Esperanto simplified and rendered scientific by improvements which have been made in it by international philologists working through a central body.

Ido has purged from the vocabulary of Esperanto all the words which were arbitrarily invented by the founder of Esperanto. The principle upon which the vocabulary of Ido is based is that of the maximum internationality. Each word has been devised from what can be found of common in words having an identical meaning in the existing natural languages. Practically every word selected is common in some slightly modified form to several European languages. Ido has not found it necessary to introduce a great number of arbitrarily invented words, such as are found in Esperanto. The Ido vocabulary is easier to learn and easier to use.

Ido, unlike Esperanto, has no accented letters, so that for printers to print, or typists to type, no special types of each of the thousands of founts in use have to be stocked, nor do typewriters have to be specially fitted. Esperanto can, of course, dispense with accents in the same way as the modification of German vowels can be effected by the addition of an 'e' instead of the use of letters furnished with a diæresis, but the appearance of such matter is as unnatural to the eye as the sound of the speech is to the ear.

Because of its natural derivation, printed and spoken Ido are pleasing to both eye and ear. Once convinced of the necessity for an international language, everyone in favour of Esperanto should be still more in favour of Ido, because Ido is really

Esperanto grammar simplified, improved, and complemented by a vocabulary devised by numerous philologists who have worked upon the same foundation as Esperanto during the past fifteen years.

The Idist Societies and Academy have, therefore, done in advance work which would have had to be performed in any other case in the event of the acceptance of Esperanto.

The spread of Esperanto was largely due to the enthusiastic reception it received in France, the intellectual centre which introduced it to the rest of the world. In France the foremost exponent of Esperanto and its propagandist in chief was the Marquis de Beaufront. There was a competent International Delegation which, after six years of preparatory study, met in Paris in 1907 and was recognised by Esperantists until it gave only a qualified approval to their project. This Committee deliberated in 1907 and finally decided to adopt Esperanto in principle modified in accordance with the suggestion of the Marquis de Beaufront whose competence is mentioned above, and whose revised Esperanto is named Ido. The word Ido is an Esperanto suffix meaning 'derived from,' *i.e.*, derived from Esperanto.

V. Observations and Recommendations.

A.—LATIN.

The main advantages and disadvantages of Latin as an I.A.L. appear to be as follows :—

As Latin is a neutral language, its adoption would not give an undue advantage to any one nation, although it would be more easily acquired by nations speaking a Romance language or a language containing Romance elements, such as English. Latin has already served, in the Middle Ages, as an I.A.L., and the cultured individuals who used it apparently found it quite satisfactory ; a modern form of Latin is now in general use in the Roman Catholic Church. Thoughts and emotions can be expressed as lucidly and as concisely in Latin as in any modern national language or any invented tongue. New words can be built up from Latin or Greek stems on well-established lines.

On the other hand, the acquirement of Latin is relatively difficult to the average man. Its general use as an I.A.L. has been abandoned ; its revival would be very difficult and would entail the coining of a very large number of new words. There would be great difficulty in securing the adoption of a uniform pronunciation.

The Committee is unanimous in its conclusion that the advantages of Latin as an I.A.L. are outweighed by its disadvantages. Although there is ample scope for research on the relative ease of acquirement of languages, there is no doubt that to the English-speaking peoples, at least, even a 'working knowledge' of Latin is more arduous to acquire than a corresponding knowledge of, say, Spanish, Italian, French, German, or Dutch, and incomparably more difficult than Esperanto or Ido. It may be that, by improved methods of instruction and with better teaching, Latin might be learnt in a much shorter time than it is now ; or that a simplified form, *e.g.*, Latin without inflexions, might be elaborated (which would probably lose in precision as it gained in simplicity) ; but these at present are little more than remote possibilities, and simplified Latin would belong essentially to the artificial languages and would be more properly considered with them. That Latin, as we know it, could be used as an I.A.L. to excellent advantage by the intellectual *élite*, appears very probable ; but that the average individual, including the Mongol, the Negro, etc., is capable of acquiring even a 'working knowledge' of it within a reasonable time, or that he could be induced to attempt to learn it, seems very improbable.

The second serious objection to Latin is the difficulty of pronunciation. It is sufficiently hard to obtain adequate uniformity and mutual intelligibility in the pronunciation of ancient Latin words ; but a very large proportion of the new words that would have to be coined, or taken over into the new Latin, would not be Latin at all, and the pronunciation given to these words would naturally be the same as that which the speaker habitually employs in his own language.

The lucidity and conciseness of good Latin are undeniable, but these attributes are not peculiar to Latin, and in any language they appear to depend as much upon the individual as upon the nature of the tongue he speaks or writes.

In submitting that the adoption of Latin is impracticable, the Committee is not unmindful of its greatness, of its high value as a means of culture, and of its utility as a mental discipline; but these considerations appear to be relatively unimportant from the standpoint of the I.A.L. problem, which is to provide a language as free as possible from difficulties of accidence and syntax, and one which will be readily understood and used by an immense number of individuals who are not scholars.

B.—ENGLISH.

The principal arguments in favour of the adoption of English are:—(1) It is already widely used and is expanding naturally; (2) its grammar is relatively simple and its vocabulary rich. *Per contra*, (*a*) its adoption would confer great political and certain other advantages on the English-speaking peoples and would arouse the jealousy of other nations; (*b*) both spelling and pronunciation are difficult; and (*c*) absence of inflexions, hyphens, etc. gives rise to ambiguity.

Although accurate and complete data are not available, there seems to be no doubt that the English language, including 'pidgin' English, is not only more widely spoken than any other national European tongue, but that its use is extending both rapidly and naturally. These considerations constitute a very strong argument in favour of its adoption—by general consent rather than by *laisser faire*, since the rate of progress by the latter method would be all too slow and subject to fluctuations in the political fortunes of the English-speaking nations. Early in its deliberations the Committee came to the conclusion that it would be undesirable for it, as a committee of Englishmen, to pronounce in favour of the English language, and accordingly it sought the opinions of foreigners (*v.i.*). The conclusion was, however, reached (with one dissentient—Professor Ripman) that objection (*a*) (*v.s.*) was sufficient in itself to preclude the recommendation of English or of any existing national language. Every such language is bound up with the outlook, world-place, and racial temperament of its people. The great international languages of the past, Greek, Latin, Arabic, French (in the East), and English at the present day have all borne the marks of imperial *prestige* which prevented them from being welcomed by alien races. To spread a national language by international effort would be, in effect, to extend the power of that nation or race, however impartial might be the intentions of the promoters.

It is scarcely necessary to state that, should English become the I.A.L., either by general consent or by natural means, the members of this Committee would one and all rejoice, but from a wide view of the possibilities and from the results of specific inquiries addressed to foreign Chambers of Commerce, it does not appear in the least degree probable that any such agreement would be reached. If English were adopted it would devolve upon the English-speaking peoples to introduce into their language certain reforms in spelling which would make it easier for international use. This would be a minor handicap compared with that of having to learn a new language.

Although, by reason of its simpler grammar, a 'working knowledge' of English is more easily acquired than one of Latin for use as a second language, neither can compare with Esperanto or Ido in this respect. The difficulties of English have been well stated in the section on the claims of Latin (page 398), but in actual practice, in the affairs of commerce and every-day life, they are not experienced to any inconvenient extent. As in the case of Latin, French, and some other languages, the fact that English is the key to a fine and vast literature appears to be of secondary importance from the point of view of an international *auxiliary* language.

C.—ESPERANTO AND IDO.

The outstanding advantage of these invented languages is their great simplicity; they are constructed on well thought-out scientific principles and are, therefore, adaptable to many diverse requirements. Further, they are neutral, and have already made considerable progress (especially Esperanto). *Per contra*, they excite antagonism in certain quarters because they are frankly artificial; the opposition between them tends to be destructive of both; and neither is used to an extent comparable with that of any existing great national language.

The great facility with which these languages are acquired is obvious to all who have studied them or who have had the opportunity to make first-hand observations. This facility is due primarily to the extreme simplicity of the grammar and the in-

genious method of word-building, *i.e.*, by adding prefixes and suffixes to key-words which convey general ideas; and to a lesser extent to the preponderance in them of Romance constituents. In some respects these languages resemble Latin stripped of all the exceptions and irregularities, which correspond to the friction of an obsolete machine obviated by the inventor of a new one. Evidence is lacking that this ease of acquirement applies equally well to Eastern peoples, *e.g.*, Chinese, Japanese, natives of India, Africa, etc., and the Committee would welcome information on this point. The principle of 'maximum internationality' of selected key-words as applied by the Idists works out very well for the Western peoples from the point of view of intelligibility, but has cognisance been taken of the 320,000,000 speaking Chinese? With the object of testing the ease of acquirement of Esperanto, Professor J. J. Findlay has recently conducted an experiment with a class of secondary school teachers at Manchester University, and his conclusions are, briefly, as follows:—(*a*) A practical knowledge of Esperanto can be acquired in a far shorter time than any natural speech, ancient or modern; three weeks of intensive study under a good instructor would enable a class of teachers to master the essentials and acquire a fluency which is seldom gained in French or German after twelve months' study abroad. (*b*) Its construction secures the intellectual interest of the learner and invites him to go forward and complete the mastery. (*c*) Methods of instruction are of capital importance.

Great as is the importance of ease of acquirement, it is not all; the easiest language is not necessarily the best, for it may be lacking in precision. In the course of the inquiry, certain passages from English texts were submitted to the Classical Association, the British Esperanto Association, and the International Language (Ido) Society of Great Britain for rendering into Latin, Esperanto, and Ido, and the short extracts from these translations which are given in the Appendix will enable the reader to judge the precision of each.

The objection that Esperanto and Ido are not real 'living' languages, but artificial 'dead' things, appears to be based on sentiment and inference rather than upon observation, and is no more valid than a similar argument would be against the practicability of shorthand, synthetic indigo, or artificial fertilisers. There is satisfactory evidence that Esperanto has made very considerable headway in spite of academic prejudice, limited resources, and nationalistic sentiment. Scepticism concerning the practicability of either of these languages vanishes when one attends a public meeting where one of these is spoken, or on witnessing students engaged in learning them; to the learner the process is no more 'foreign' than that of learning French or German. Therefore *a priori* arguments based upon the specific nature and evolution of language must yield to experience and demonstration.

The Committee has not undertaken to adjudicate upon the respective claims of Esperanto and Ido; for the present it is satisfied, from the evidence brought before it, that both are practicable, the differences being more matters of detail than of principle. However, it is obvious that there can be but one I.A.L., and it, therefore, appears to be of first importance that the schism which has arisen between the protagonists of these two languages should be healed as soon as possible. The Committee is gratified to have the assurance of the British Esperanto Association and of the International Language (Ido) Society of Great Britain that they are willing to accept changes in their languages which might be considered desirable by an impartial, expert and authoritative international body. In view of the fact that these languages have much in common, Ido being admittedly based upon Esperanto, it seems reasonable to hope that the efforts of such a body would meet with success. Unity is essential, and it would be lamentable if a project to unite the nations were to be shipwrecked by disunion within the ranks.

In the above connection it may be recalled that in a letter to Esperantists, dated January 18, 1908, Dr. Zamenhof (the inventor of Esperanto) himself foresaw the probability that the question of an I.A.L. would be taken up by some authoritative external agency whose decisions would be regarded as final:—'When the matter shall have been taken in hand by some great agency (*e.g.*, the governments of the great nations), which by reason of its power will be in a position to give us not merely worthless over-confident verbal promises, but full guarantee that it will carry our project to its goal more certainly than we, and that it will not lightly commit itself to any decisions before these shall have had the most competent and mature consideration, and have been subjected to definite and practical tests, then we can confidently transfer to this great agency the fate of our affair.'

From the evidence laid before it, the Committee (Professor Ripman dissenting) has come to the conclusion that a language of the type of Esperanto and Ido should be adopted as the International Auxiliary Language ; and also, that, whatever language be adopted, it should be placed under scientific control.

It will be recollected that in 1918 the Prime Minister's Committee on Modern Languages recommended, *inter alia*, 'that a Committee be appointed to inquire into the potentialities of artificial languages and of the desirability of encouraging the development and use of one'; and this Committee hopes that other responsible academic bodies, literary and scientific, as well as commercial organisations, will be stimulated to consider the question in all its bearings so that there may result an expression of truly representative opinion which will carry weight in the deliberations of any superior organisation that may be appointed to make the final decision. In the meantime there is a number of collateral questions and problems which would repay investigation, such as the acquirement of language, the value of an I.A.L. as an introduction to the study of foreign languages, its place in the school curriculum, the possibility of arriving at a standard pronunciation based upon a phonetic study of the chief natural languages, and the measures to be taken to stimulate and instruct public opinion. The ultimate success or failure of any I.A.L. will be determined by the attitude of the peoples ; learned societies, academic bodies, and private organisations may do extremely useful pioneer work, but, unless the public is brought to realise both the desirability and the practicability of such a project, their efforts will fall short of the desired result. *Tout ce qui multiplie les nœuds qui attachent l'homme à l'homme, le rend meilleur et plus heureux.*

* * *

Summary of Conclusions.

The views of the Committee may be summarised, very briefly, as follows :—

(1) Latin is too difficult to serve as an I.A.L.

(2) The adoption of any modern national language would confer undue advantages and excite jealousy.

(3) Therefore an invented language is best. Esperanto and Ido are suitable ; but the Committee is not prepared to decide between them.

Appendix.

Short extracts from specimen translations prepared by, or under the auspices of, the Classical Association, the British Esperanto Association, and the International Language (Ido) Society of Great Britain.

Letter from a Lead Merchant.

ENGLISH.

We enclose herewith four cardboard boxes containing thiosulphate of lead such as is used by match makers; will you submit these to any likely buyers and find out if they are disposed to purchase? Our price to-day is £90 per ton f.o.b. London, and the price will rise or fall with the price of pig lead, less $2\frac{1}{2}$ per cent. discount for prompt cash.

We also send by parcel post four sample tins of lead residues which we wish you to offer to lead smelters and especially to those who have blast furnaces. The residues are a mixed lot of slags, lead ashes, fumes, test bottoms and so forth, and will average 50 per cent. lead. We offer these at the price of pig lead on the date of delivery f.o.b. London less five units and £5 returning charge. Kindly wire on receipt of the samples. We have about 100 tons to offer you. If you are a buyer please arrange for cash against bill of lading. We will effect insurance through your shipping agents, and you should instruct your lightermen to store in a bonded warehouse.

On receipt of your cable of the 13th, we rang up Lloyds and got the policy underwritten. You have not sent us the charter-party and we cannot do anything further without it.

If you prefer it we will draw on you a three months' bill for the residues and get it discounted by bill brokers here.

LATIN.*

Cum hâc epistulâ simul inclusas mittimus 4 capsulas cartonaceas Thiosulphatum Plumbi continentes, quale ad fabricam flammiferorum usitatum est. Has ubicunque emptoribus placituras esse creditis, ut offeratis precamur. Pretium hodie quod postulamus erit nonaginta librae Sterlingae pro tonnâ Anglicâ mercis ad Londinium in navem impositae. Hoc tamen pretium augebitur minueturque unâ cum pretio Plumbi rudis; pro praesenti autem pecuniâ deductio fiet $2\frac{1}{2}$ p.c.

Praeterea per Postam Portitoriam quattuor capsulas stanneas mittimus exemplares, Residua plumbea continentes quae veno offerre volumus Plumbi fusoribus, iis praecipue qui fornaces flabiles habent. Haec Residua sunt mixta ex Scoriâ plumbi, cineribus et fumis, ut vocant, plumbicis, fractis crucibulorum fundis, aliis rebus eiusdem generis; per haec in universum aestimata plumbi proportio quinquaginta part. cent. erit. Haec vendimus Plumbi Rudis pretio quo die in navem ad Londinium imposita erunt, pretii tamen huius 5 p.c. retro-fusionis nomine remittimus, et 5 libras sterlingas. Receptis his exemplis, nobis telegraphice nuntietis precamur. Circa centum tonnas Anglicas venales habemus. Si emere voltis, praesentem pecuniam pro Catalogo Onerario solvendam vos curetis velimus. Periculi marini Securationem nos curabimus per proxenos vestros navicularios. In horreis anteportoriis merces deponendi negotium vestris scaphariis dandum est.

Nuntio vestro telegraphico diei 13mi accepto Societatem Lloyds certiorem fecimus per telephonam, periculique marini eius summae praedes constituimus. Contractum partiarium a vobis nondum accepimus, sine quo nihil amplius facere possumus.

Si vos mavoltis, pro Residuis illis trium mensium chirographum a vobis accipiemus idque inter chirographorum negotiatores venale proponemus.

[* The writers of the Latin version state that they thought it their business to express completely the full meaning of the English put before them, though they also thought it could have been condensed with advantage.]

Letter from a Lead Merchant.

ESPERANTO.

Ni sendas ĉi kune kvar kartonaĵojn, kiuj entenas plumban tiosulfaton kian uzas alumetfabrikistoj; bonvole proponu ilin al eblaj aĉetontoj kaj informiĝu, ĉu ili volas aĉeti. Nio prezo hodiaŭ estas £90 por tuno surŝipe el Londono, kaj la prezo altiĝos aŭ male laŭ la prezo de kruda plumbo, kun rabato de 2½% por tuja saldo.

Ni ankaŭ sendas specimene per pakaĵa poŝto kvar stanaĵojn da plumbrestaĵoj, kiujn ni petas vin proponi al plumbofandistoj, precipe al tiuj posedantaj blovfornojn. La restaĵoj estas miksaĵo de skorio, plumbcindro, fumaĵoj, kaj tiel plu, kiu enhavas proksimume 50% da plumbo. Ni proponas ĉi tion je la prezo de kruda plumbo en la dato de livero surŝipe en Londono, malpli 5 unuoj kaj £5 resenda kosto. Bonvole telegrafu tuj post ricevo de la specimenoj. Ni povas proponi al vi proksimume 500 tunojn. Se vi volos aĉeti, bonvole aranĝu por tuja pago kontraŭ ŝarĝatesto. Ni asekuros pere de viaj ŝipagentoj, kaj vi devos doni instrukcion al viaj elŝipigistoj meti la komercaĵon en doganan deponejon.

Ricevinte vian kablogramon de la 13a, ni telefonis al Lloyds kaj subskribigis la polison. Vi ne sendis al ni la ĉarton, kaj sen ĝi ni povas fari nenion plu.

Se vi tion preferos, ni tratos al vi per trimonata kambio por la restaĵoj, kaj diskontigos ĝin ĉe kambiistoj ĉi tie.

IDO.

Ni kune inkluzas quar karton-buzi kontenanta plombo tiosulfato tale quale uzesas da alumetifisti; kad vi voluntez ofrar ici ad irga probabla kompreri e saveskar kad li inklinesas komprar ol? Nia preco cadie esas £90 po tuno, afrankita ye bordo di London, e la preco augmentos o diminutos segungrade la merket-preco di plomba lingoti, kun rabato di 2½ po cent. ye pago kontanta.

Ni anke sendas per paket-posto quar specimeni, en lada buxi, de plomba rezidui quin ni deziras ke vi ofrez a plomb-fuzisti ed precipue a ti qui havas ventofornegi. La rezidui esas mixita loti de skorio, plomba cindri, precipitaji de fumuro plomboza, restaji de fornegi, edc., e havas mezvaloro di 50 po cent de plombo. Ni ofras ici segun la preco di lingoti de plombo ye la dato di livrado, afrankite ye bordo, minus 5 unaji e £5 kustumala rabato por fuzado-procedo. Komplezeme telegrafar ye la recevo di ica specimeni. Ni havas proximale 100 tuni por afrar a vi. Se vi esas kompreri, voluntez aranjar pagar pekunio kontre la konosmento. Ni asekurigos per vua mar-transport-agenti, e vi devas instruktar via gabareri depozar ol en la doganal-magazino.

Lor la recevo di via kablo-telegramo dil 13esma, ni telefonis a Lloyds' ed aranjis ke on signatizez la asekuro-kontrato. Vi ne ja sendis la freto-kontrato e ni nule povas agar pluse til ke ni havas ol.

Se vi preferas ni volas tratar vi per bileto pos tri monati por la rezidui, diskontenda da diskontisti hike.

From a Paper read before the Royal Society.

ENGLISH.

The Protoplasmic Factor in Photo-synthesis.

The centre of interest in problems of the photo-reduction of CO_2 in green photo-synthesising cells is shifting from the chlorophyll to the protoplasm. The quantitative control of photo-synthesis in the normal green cell is determined protoplasmically. This is illustrated by the temperature relations which are not those of a photo-chemical reaction, but of a dark reaction. The photo-synthetic activities of leaves of different varieties (green *v.* golden leaves) and at different stages of development show no relation to the amount of chlorophyll that they contain, as is brought out by the 'assimilation numbers' of Willstätter. The relation between chlorophyll development and photo-synthesis development, described in the next communication, furnishes another instance of the dominance of factors other than the pigment.

LATIN.

De vi Protoplasmatis in Photo-Synthesi.

In photo-reductione Carbonis Dioxidi, quae fit viridibus in cellis photo-synthesin efficientibus, protoplasma potius quam chlorophyllum hominum mentes nunc coepit occupare. Quanta enim fiat photo-synthesis in cellulâ viridi normali, ex protoplasmate potissimum pendet. Cuius rei id signum est quod temperaturae rationes non chemicae sed obscurae reactioni respondent. Neque enim photo-syntheticae potestates foliorum quae nec eodem genere sunt, *e.g.*, viridium et flavorum, nec ad eundem vitae gradum pervenerunt cum quantitate chlorophylli quod continent ullam proportionem exhibent—id quod plane declarant 'numeri assimilationis' Willstätteriani. Evolutio praeterea chlorophylli cum photo-synthesis evolutione comparata, quae in proximâ relatione describetur, docet in hâc re causas alias praeter pigmentum dominari.

From a Paper read before the Royal Society.

Esperanto.

La Protoplasma Faktoro en Fotosintezo.

La centro de intereso ĉe problemoj pri la foto-redukto de CO_2 (karbona dioksido) en verdaj fotosintezaj ĉeloj transiĝas de la klorofilo al la protoplasmo. La kvanta kontrolo de fotosintezo en normala verda ĉelo determiniĝas protoplasme. Tion ilustras la temperaturaj rilatoj, kiuj ne estas tiuj de fotokemia reakcio, sed de reakcio senluma. La fotosinteza aktiveco de folioj diversspecaj (verdaj kontraŭ oraj folioj) kaj ĉe diversaj stadioj de disvolviĝo ne montras rilaton al la kvanto de klorofilo entenata, kiel montras la 'asimilaj nombroj' de Willstätter. La rilato inter klorofila disvolviĝo kaj fotosinteza disvolviĝo, priskribota en la sekvanta komunikaĵo, donas alian ekzemplon de la superrego de faktoroj aliaj ol la pigmento.

Ido.

La Protoplasmala Faktoro en la Fotosintezo.

La intereso-centro en la problemi pri fotoredukto dil CO_2 en la verda celuli fotosintezanta, transfereskas de la klorofilo a la protoplasmo. La kontrolo quantesmala dil fotosintezo en la normala celuli verda esas determinita protoplasmale. To esas manifestebla per la kaloral relati, qui ne esas ti di fotokemial redukto, ma di reakto en obskureso. La fotosintezal funcioni di folii de diferanta varietati (verda kontre orea folii) ed en diversa gradi di developo, montras nula relato a la quanto de klorofilo quan ta folii kontenas. Ta fakto esas demonstrita da la 'asimilal nombri' di Willstätter. La relato inter la developo atribuebla al fotosintezo, deskriptita en la sequanta raporto, furnisas altra exemplo pri la influo di altra faktori kam la pigmento.

From a Critique of a Concert.

ENGLISH.

The Bach Festival.

The second concert of the Bach Festival at the Central Hall, Westminster, on Saturday, was devoted mainly to instrumental music, though there were two songs from church cantatas sung by Miss Dorothy Silk and Mr. Gervase Elwes. The Suite in B minor for Flute and Strings played by M. Fleury, with members of the London Symphony Orchestra, made a most genial opening, and was beautifully played. It was curious to find, however, that the solo flute stood out more clearly against the massed strings than when playing in company with a group of solo strings.

The concert ended with the Overture in D (No. 3) for orchestra with three trumpets and drums. Here a word of congratulation is due to Mr. Barr for the remarkable assurance and skill of his playing of the first trumpet part. The *ensemble* as a whole was not everywhere entirely satisfactory, possibly because the players had not had sufficient rehearsal to assimilate all Dr. Allen's ideas of balance and *tempi*. In the air, where the first violins alone were unmuted, the inner parts were too subdued, and in the gavotte and elsewhere the orchestra hurried the conductor, the reverse of what one would expect. These things, however, were only slight defects in a finely executed programme.

LATIN.

De Bachicis Festis.

Secunda de commissionibus Musicis, quae nunc Bachis nomine celebrantur, Saturni die habita est Westminsterii in Testudine Centrali. Musica autem plerumque fuit instrumentalis, sed duo carmina e 'Cantatis Ecclesiasticis' excerpta cecinerunt voce Domina Dorothea Silk et Dominus Gervase Elwes. 'Sequentia' quae in B minore est tibiae uni fidibusque pluribus accommodata, a Dno. Fleury unâ cum sociis Orchestrae Symphoniacae Londiniensis quamvis belle edita est—quod festivissimum fecit initium. At id notandum videtur quod Tibia Solicana clarior erat magisque eminebat fidibus universis simul cum eâ sonantibus quam paucis solicanis fidibus.

Commissionis huius finis fuit 'Apertura in 'D' (n. 3) ad Orchestram et tres tubas atque tympana accommodata. Et hic loci lubet nonnihil gratulari Dno. Barr, quod fidenter ac sollertissime primas partes tubae sustinuit. 'Concentus' autem 'universitas' hic illic non omni carebat offensione—et fieri potest ut ludentium chorus, parum crebris usus exercitationibus, quae de libratione temporibusque musicis praecepisset Doctor Allen, ea omnia penitus animo percipere non potuerit. Ad hoc in melodiâ, quâ soli violini primi libere (remotis ligulis eis quibus sonus obfuscatur) recinebant, 'partes interiores' nimis submisse sonabant, necnon et in Gavottâ et alibi Orchestra, contraquam exspectaris, velocius ire ac magistrum (*or* rectorem) quodam modo incitare videbatur. Sed tamen haec vix notanda erant vitia in optimâ serie carminum optime redditorum.

From a Critique of a Concert.

ESPERANTO.

La Bach Festo.

La duan koncerton de la Bach Festo en la Central Hall, Westminster, la laston sabaton, oni dediĉis plejparte al instrumenta muziko, krom du arioj el ekleziaj kantatoj, kiujn kantis F-ino Dorothy Silk kaj S-ro Gervase Elwes. La Suite en B minora por fluto kaj kordoj ludita de S-ro Fleury kaj anoj de la Londona Simfonia Orkestro faris tre agrablan malfermon, kaj estis bele ludata. Strange tamen estis konstati, ke la solfluto elstaris pli klare kontraŭ la kordoj amasigitaj, ol kiam ĝi ludiĝis kun grupo da solinstrumentoj.

La koncerto finiĝis per la Uverturo on D (No. 3) por orkestro kun tri trumpetoj kaj timpanoj. Gratulan vorton oni ŝuldas al S-ro Barr pro la rimarkinda aplombo kaj lerteco de lia ludo de la unua trumpeta partio. La *ensemble* ne ĉiam estis tute kontentiga, eble ĉar la ludantoj ne havis sufiĉan antaŭprovon por asimili ĉiun ideon de D-ro Allen pri proporcio kaj rapido. En la ario, kie nur la unuaj violonoj estis sensordinaj, la internaj partioj estis tro subigitaj, kaj en la gavoto kaj aliloke la orkestro rapidigis la kondukiston—male de tio, kion oni povus atendi. Ĉi tio tamen estis nur difektetoj en bele plenumita programo.

IDO.

La Festajo di Bach.

La duesma koncerto dil Festajo di Bach che Central Hall, Westminster, ye Saturdio, dedikesis precipue ad instrumental muziko, quankam esis du kansoni ek kirkal kantati quin Damzelo Dorothy Silk e Sioro Gervase Elwes kantis. La *Suite* (serio) en B minoro por la fluto e kord-instrumenti pleita da Sioro Fleury, kun membri dil London Simfoni-Orkestro, facis maxim simpatioza komenco ed esis bele exekutita. Remarkinda tamen esis observar ke la solo-fluto esis plu klare distingebla apud la amasigita kord-instrumenti kam kande on pleis ol kun grupo di solo-kord-instrumenti.

La koncerto finis per la Uverturo en D (No. 3) por orkestro kun tri trumpeti e tamburi. Hike paroli gratulanta debesas a Sioro Barr pro la remarkinda su-certeso e habileso di lua pleado dil parto por la unesma trumpeto. La ensemblo-peco, judikita kom totajo, ne esis omnaloke tote satisfaciva, posible pro ke la pleanti ne suffice subisis ol a anteaprobo por asimilar omna la idei di Dro. Allen pri equilibrigo e tempi. En la melodio ube la unesma violini sole nemutigesis, la interna parti esis tro quieta ed en la Gavoto ed altraloke la orkestro rapidigis la muzik-direktoro, quo esis la kontreajo a to quon on expektabus. Ica punti tamen esis mikra difektaji en bele exekutita programo.

SECTIONAL TRANSACTIONS.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 464.)

Thursday, September 8.

1. Prof. J. C. McLennan, F.R.S.—*Radiation and Absorption by Atoms with Modified Systems of Extra-nuclear Electrons.*

2. Prof. R. W. Wood, For. Mem. R.S.—*The Spectra of Hydrogen from Long Vacuum Tubes.*

The spectrum of hydrogen obtained in the laboratory exhibits a Balmer series of lines, of which only twelve members are found under ordinary conditions. Thirty of the lines have been recorded in the solar spectrum. The impossibility of obtaining the higher members of the series in the laboratory results, in part at least, from the presence of a continuous spectrum and the so-called secondary spectrum.

By employing vacuum tubes of unusual length it has been found that the secondary spectrum appears only at the ends, the central portion radiating a very pure Balmer spectrum. By this means it has been possible to record the series down to the twentieth line. Very remarkable phenomena have been observed with tubes of this type. Starting with a very feeble current, the secondary spectrum only is in the tube, the Balmer lines being absent. As the current is increased the Balmer lines appear and increase in intensity, while the secondary spectrum fades away, passing through a minimum value of about one-fiftieth of the intensity shown at the ends of the tube, after which it slowly increases in intensity.

If a heavy current is employed, there is an explosive flash of the secondary spectrum at the moment of closing the switch, the Balmer lines being feeble. In about $\frac{1}{60}$ sec. the secondary spectrum has nearly disappeared (reduced to one-fiftieth of its initial value) and the Balmer lines have risen to full intensity. These phenomena occur only when a trace of oxygen or water vapour is present. With pure hydrogen, continued operation of the tube eventually causes the complete disappearance of the Balmer series, the secondary spectrum remaining, and the colour changing from fiery purple to white. A new spectrum has been found which is much more complicated in structure than the secondary spectrum. This appears when the tube is in the condition best suited to the exhibition of the Balmer series.

3. Prof. R. W. Wood, For. Mem. R.S.—*The Time Interval between the Absorption and Emission of Light in cases of Fluorescence.*

In the case of mercury vapour, illuminated by the instantaneous flash of an aluminium spark, it has been found that the vapour remains non-luminous during the period of excitation and for about $\frac{1}{15000}$ sec. after, subsequently bursting out in a flash of green fluorescent light. This appears to be the first observation of a fluorescent or phosphorescent body remaining dark during the period of illumination. Other substances have been observed with a new type of phosphoroscope which records the phenomena of phosphorescence to $\frac{1}{400000}$ sec. Nothing analogous in its behaviour to mercury vapour has been found up to the present time, however.

It is only freshly formed mercury vapour which exhibits the phenomenon of fluorescence. No trace of visible luminescence is shown by mercury vapour at any density, or by any light stimulation, *unless metallic mercury is present* and liberating nascent molecules. It is believed that these are diatomic when they first leave the metal, subsequently breaking up into monatomic molecules

4. Prof. J. C. Kapteyn.—*First Attempt at a Theory of the Structure and Motion of the Stellar System.*

According to former results, slightly modified, the equidensity surfaces of the stellar system for that part (domain A), where the density exceeds one-hundredth part of the density near the sun, must be with some approximation concentric similar rotation ellipsoids, similarly situated. As a highly probable consequence the attraction of the whole of the system on a point within the domain A can be computed as soon as we know the average mass of a star.

Reasons were shown for assuming the whole of the system to be in a rotatory motion round the line from the centre towards the pole of the Milky Way as an axis, and for further assuming that to the stars along this axis we may apply the barometric formula for determination of heights in our atmosphere. The application leads to a determination of the average mass of a star, with the following results :

Distance from centre	Average mass
198 parsecs.	2·2 × mass of sun.
413 ,,	2·0 ,,
717 ,,	1·7 ,,
1114 ,,	1·5 ,,
1660 ,,	1·4 ,,

which agree surprisingly well with what was derived from binary stars, the change with distance being in the sense required by theory.

The same values of the masses are obtained for the stars in the plane of the Milky Way if to the gravitational forces we add definite centrifugal forces. The linear velocities implied by these centrifugal forces are :

Distance from centre	Linear Velocity
1010 parsecs.	13·0 kilometres per sec.
2106 ,,	19·5 ,,
3657 ,,	20·1 ,,
5675 ,,	19·4 ,,
8465 ,,	18·6 ,,

Assuming that part of the stars rotate one way and the rest in the opposite direction, there are two groups of stars which in the overwhelmingly greater part of the system have a relative motion of approximately $2 \times 19{\cdot}5 = 39$ kilometres per second. This velocity agrees almost perfectly with the relative velocity of the two star streams, so that we get an explanation of these streams by simply assuming that the sun is not at the centre but in this greater part of the system, which seems strongly probable *a priori.*

That no curvature was found for the streaming must be due to the fact that the extent of the domain for which star streaming was found is so small that the curvature must escape notice.

5. Dr. Crichton Mitchell.—*The Geophysical Observatory in the Shetlands.*

(*a*) Department of Physics and Mathematics.

6. Prof. E. T. Whittaker, F.R.S.—*Tubes of Force in Four-dimensional Physics.*

7. Mr. A. A. Campbell Swinton, F.R.S.—*The Reception of Wireless Waves on a Shielded Frame Aerial.*

The paper described experiments in receiving the spark emission from the Eiffel Tower on a small frame aerial placed inside a tube of wire network with open ends. It was hoped to obtain improved directional properties, but, though the presence of the tube weakened the signals, it was found that altering its direction did not affect them, nor were the signals further weakened by closing the open ends of the tube by wire grids.

The tube was next replaced by a sheet copper box with one open end. In this was placed, not only the frame, but also the amplifier and all the other apparatus, the telephone being listened to through a rubber pipe. Signals were

heard of equal strength with the open end of the box pointing towards, or directly away, from Paris; but ceased when the box was turned so that the open end faced at right angles to Paris, the frame still pointing to Paris; or when the open end was completely closed with a copper or tinfoil cover. In the latter case the signals were still audible unless the cover actually touched the box on all sides. For other positions of the box with the end open, signals could only be heard when the relative positions of the box and frame were such that a prolongation of the plane of the frame towards or away from Paris, no matter which, came out of the open end clear of the copper sides of the box.

8. Sir F. W. Dyson, F.R.S.—*Results with the 72-in. Reflector in British Columbia.*

9. Prof. H. Briggs.—*Prehensility: a Factor of Gaseous Adsorption.*

Prehensility is defined as the slope at the origin of an adsorbent isotherm.

A method of measuring prehensility is described, and results given for various adsorbents at liquid air temperature.

It is shown that from the prehensility the evacuating power of a substance may be calculated. In evacuating any given volume, the weight of charcoal required to yield a required reduction of pressure may be computed. The degree of vacuum obtained in a Dewar liquid air container is discussed.

The plum-stone charcoal used by the author had a higher evacuating power than coconut charcoal.

Reference is made to a colloidal silica of appreciable evacuating power, though, at -190° C., over four times as much of it is required (by volume) as of plum-stone charcoal to attain the same result, and it acts more slowly.

The very high degree of vacuum procurable by using a succession of charcoal bulbs is discussed, and it is shown that with a given weight of charcoal the reduction of pressure obtainable by division of the mass among a number of bulbs does not indefinitely increase with that number, but eventually reaches a maximum.

(*b*) Department of Cosmical Physics.

10. Dr. H. Jeffreys.—*The Cause of Cyclones.*

11. Capt. C. K. M. Douglas.—*Some Remarks on Bjerkne's Theory of Cyclones and Anticyclones.*

The following demonstrations were given during the afternoon:—

12. Mr. J. J. Dowling.—*Demonstration of the Recording Ultra-micrometer and some of its Applications.*

(1) Theory.—Variation of the capacity in an oscillating valve circuit causes variation of the plate circuit current. (2) A potential balancing device enables a sensitive galvanometer to be employed to record these changes. (3) Extremely minute changes in capacity due to relative displacements of plates (10^{-9} cm. or less) are readily detectable. (4) The sensitivity is independent of the condenser plate distance over large ranges (up to 1 mm.). (5) The device is quite stable and consistent in operation. (6) The following applications were exhibited:—

(*a*) Micrometer arrangement for demonstration.
(*b*) As applied to a seismograph; specimen records.
(*c*) Micro-pressure manometer.
(*d*) Rapid action sensitive balance.
(*e*) Application to recording of plant growth.

(7) Other applications are under investigation, including the measurement of variation of gravity—it is quite easy to detect the diminution in gravity due to a height of one metre.

13. Mr. W. L. Balls.—*Demonstration of Simple Harmonic Analyser and Periodoscope.*

The apparatus consists of a set of equidistant strings under equal tension radiating from a flexible yoke, the movements of which are observed by an optical lever. The angle of divergence of the strings is small, so that they may be considered as being parallel.

The curve to be investigated is made into a template and pushed under the strings at a point corresponding to the interval which it is desired to test as being the suspected length of the period. The resulting movement of all the strings is averaged out into the movement of the spot of light from the optical lever. Both the length and amplitude of the period may thus be determined with fair accuracy. The outstanding advantage of the device is its simple construction and the rapidity with which it may be used, once the template has been prepared.

14. Dr. Dawson Turner and Mr. D. M. R. Crombie.—*Demonstration of the Behaviour of an Electrified Pith Ball in an Ionised Atmosphere.*

The experiments included those showing the effectiveness of various sources of ionisation, such as an arc light, burning magnesium wire, incandescent platinum wire, Nernst filament, a Bunsen flame, X rays, and radium rays. The directive influence of the charged Leyden jar was demonstrated by the concentration of the ions along a line joining the centre rod of the jar and the source of ionisation; for unless the pith ball be in this line it appears to be unaffected. A comparison between the sensitiveness of the charged pith ball and an electroscope as indicators of the presence of an ionised atmosphere was attempted. The ions tend to be carried upwards by convection currents. The effect upon the pith ball appears to be independent of the nature of its charge.

During the afternoon, also, an exhibition of physical apparatus of historical interest, arranged by Dr. Carse, was on view. In addition, Prof. Whittaker invited members of the Section to see the Mathematical Laboratory.

Friday, September 9.

15. Presidential Address by Prof. O. W. Richardson, F.R.S., on *Problems of Physics.* See p. 25.

16. Joint Discussion with Section B on *The Structure of Molecules.* See p. 468.

In the afternoon a visit to the Royal Observatory took place.

Monday, September 12.

17. Discussion on *The Quantum Theory.* See p. 473.

(*a*) Department of Cosmical Physics.

18. Mr. Carle Salter.—*The Drought in England during the Summer* 1921.

19. Mr. W. H. Dines, F.R.S.—*The Discontinuity of Temperature at the top of the Troposphere.*

20. Prof. A. Kriloff.—*The Magnetic Anomaly in the District of Kursk.*

(*b*) Department of Physics.

21. Prof. C. G. Barkla, F.R.S.—*The Energy of X Radiation.*

22. Dr. Hans Pettersson.—*Internal Movements in the Sea.*

The stratification of the sea-water, consisting in a superposition of water-layers different in salinity, temperature, and density, different also in biological respects, is a normal condition always to be found where large masses of fresh or brackish water are being carried out to sea. The Baltic, the Kattegat, and the Skagerak are typical examples, and so, according to the results of Johan Hjort, is the Gulf of St. Lawrence and the Newfoundland waters.

The study of the stratification of coastal waters has been carried on for nearly half a century by Swedish oceanographers, and important relationships between the displacements of the water-layers and the migration of the herrings and other food-fishes have been discovered in the course of this work. The dynamics of these movements, and notably the vertical displacements of the boundary surfaces, which occur on a vast scale along the west coast of Sweden, have been made the subject of continuous observations by means of instruments specially designed for this purpose, some of which instruments were shown by the lecturer. Making use of the correlation method, the author has been able to prove that a distinct influence is exerted by the wind on these internal movements, whereas the air-pressure appears to be void of any similar effect. The influence of cosmical factors, again, has been thoroughly investigated by Otto Pettersson.

The importance of these internal movements on the hydrography of the Baltic is very great. The bottom water in its deeper basins being too dense to become ever able to rise to the surface through a surface-sheet of low-salinity water forty to sixty mètres thick, its supply of oxygen would gradually become exhausted and the water made uninhabitable for its marine fauna, if there was not an intermittent inflow of freshly saturated sea-water in the shape of an undercurrent through the straits taking place at intervals largely ruled by the occurrence of the internal movements before mentioned (experiment). These invasions of the undercurrent make the impression as of submarine waves sending gigantic 'breakers' over the thresholds to the Baltic and gliding down the slopes leading to the deeper basins like submarine streams or rapids.

(c) Department of Mathematics.

23. Rev. J. Cullen.—*The Identity* $4X = Y^2 - (-1)^{\frac{1}{2}(p-1)}p\ Z^2$.

If p be any odd prime and $X = (x^p - 1)/(x - 1)$ it is known that $4X$ can be expressed as above. Legendre, in his 'Théorie des Nombres,' stated erroneously that Y may be determined by expanding $2(x-1)^{\frac{1}{2}(p-1)}$ by the binomial theorem, and reducing each coefficient to its absolutely least residue (mod. p). He afterwards, however, corrected his mistake.

From the correspondence in *Nature*, of June 9 and July 7, 1921, together with results given in Mathews' 'Theory of Numbers,' p. 218, it appears that all odd primes $p < 41$ conform to Legendre's rule, while those from 41 to 61 inclusive fail. It seems therefore that the extent of failure has not yet been completed, and the object of the present paper is to effect this.

Let $p = 2p^1 + 1$ and $Y = 2x^{p1} + c_1x^{p1-1} + \dots + c_nx^{p1-n} + \dots$ then $3.2^7c_5 = 189 + (90 + 36e_2 + 32e_3 + 32e_2e_3)e_1p + (1 + 4e_2)p^2$, where $e_1 = (-1)^p$, ${}^1e_2 = (2/p)$, $e_3 = (3/p)$ (Mathews, p. 217). The condition in this case for the failure of Legendre's rule is $/c_5/ > \frac{1}{2}(p-1)$ and if $p = \alpha + 24t$ all primes > 3 are included in this form. Where $\alpha = 1, 5, 7, 11, 13, 17, 19, 23$, the e's are determined by the α's, and the result of substituting for p is 8 simple quadratic expressions in t for c_5. I find the condition for failure $/c_5/ > \frac{1}{2}(\alpha - 1) + 12t$ is satisfied if t exceeds any of the values below :—

$$\begin{array}{l} \alpha = 1\ ,\ 5\ ,\ 7\ ,\ 11\ ,\ 13\ ,\ 17\ ,\ 19\ ,\ 23. \\ t > 1\ ,\ 3\ ,\ 1\ ,\ \ 1\ ,\ \ 2\ ,\ \ 0\ ,\ \ 1\ ,\ \ 2. \end{array}$$

The only prime unaccounted for is 71, and on working out Y in this case, I find the term $58x^{29}$. Hence, Legendre's rule fails for all primes $p > 37$.

24. Dr. F. E. Hackett.—*The Relativity Contraction in a Rotating Shaft moving with Uniform Speed along its Axis.*

This problem was considered in terms of a fixed ether and the Fitzgerald-Lorentz contraction (A). The restricted principle of relativity (B) was applied to observations made by a fixed and a moving observer. The validity of expressing the results of their observations in terms of Euclidean geometry was assumed throughout the paper (C).

A hypothetical modification of Fizeau's method for measuring the velocity of light was considered—a rotating shaft carrying two discs with apertures which correspond to the toothed wheel in Fizeau's experiment. It follows readily that when a rotating shaft is moving with uniform velocity along its

own axis, to a stationary observer it appears twisted in the opposite sense to the rotation. This effect has been pointed out by R. W. Wood.

The arrangement may act as a clock. It measures time on the same principle as the ideal clock, consisting of a beam of light reflected between two mirrors, with the addition that a disc fixed on the shaft at any cross-section, and rotating with it, can indicate the local time there.

In the latter part of the paper, the contraction in the shaft due to the motion of translation and the twist were considered as a strain-displacement in a thin tube or solid circular cylinder. One of the principal axes of strain is assumed to be the direction of resultant velocity $\sqrt{v^2 + u^2}$ (D). The principal contraction in the latter direction is found to be $\sqrt{1-(v^2+u^2)/c^2}$.

Since the twist and longitudinal contraction are independent of the form of the shaft, the values of the contractions obtained for a thin tube hold also for a solid circular cylinder. In the limiting case of a disc rotating without any motion of translation, the circumferential contraction is equal to that of a rotating ring, viz. $\sqrt{1-u^2/c^2}$, where u is the velocity at the rim. It follows that the contraction in the radius is of the same magnitude.

At the end of the paper an application of the analysis was made to the Wiedemann effect. For the small strains of magneto-striction the formula agrees with that given by Knott, which has been experimentally verified.

25. Prof. D'Arcy Thompson.—*Note on the Tetrakaidekahedron.*

26. Prof. A. S. Eddington, F.R.S.—Lecture on *Einstein's Theory of Relativity.*

Tuesday, September 13.

27. Joint Discussion with Sections C, D, and K on *The Age of the Earth.*

Rt. Hon. Lord Rayleigh, F.R.S.—In view of the past history of this subject it seems particularly important to keep our eyes open to all possibilities, and to welcome evidence from any quarter. Lord Kelvin in the last generation attempted to set a limit of time to the duration of the sun's heat. And also from consideration of the earth's internal heat he argued back to the time when the surface was too hot for the presence of living beings.

As regards the earth's heat, it is now generally known that the premises of Lord Kelvin's calculations, carefully particularised by him, are upset by the discovery of radioactive substances in the earth. In 1906 I made a determination of the amount of radium in the superficial parts of the earth which are alone accessible. From the radium analysis we can calculate the amount of uranium and other associated substances, and the thermal output from them. The result is to show that if we suppose the same radium content to extend to a depth of some twenty miles, the whole output of heat would be accounted for, without assuming that any of it comes from primeval store as postulated by Lord Kelvin. It is surprising, in fact, that the output is not greater. We are puzzled at the present time to account for the existing state of things, and cannot use it as a firm basis from which to explore the past.

Next, as to the sun's heat. Lord Kelvin's argument was that we knew of no source at all adequate to supply the existing output of solar energy except secular contraction; and even this was not enough to account for more than 20 million years of solar heat in the past. Although we still do not know definitely of such a source, yet we are now compelled to admit that it must exist. Some of the stars (the giant red stars) are radiating energy at something like 1,000 times the rate that the sun does. They ought, according to the contraction theory, to have expended an appreciable fraction of their total energy in historical times. No one will maintain that this has occurred, and if not, there must be some source of supply other than contraction. If this is admitted, Lord Kelvin's argument from the sun's heat fails.

Modern knowledge in radioactivity, on the other hand, seems to give a firm basis for the estimation of geological time. Uranium, for example, goes

through a series of changes (radium is one of the stages in its progress), changing eventually into an isotope of lead—that is, an element chemically indistinguishable from lead except a slight difference in atomic weight, and inseparable from ordinary lead by chemical means if once mixed with it. The isotope in question has probably an atomic weight of 206 exactly, as contrasted with 207.1 for ordinary lead, which is doubtless to some extent a mixture of isotopes. Thus the product has a much less atomic weight than uranium (238.5), and the difference represents approximately the weight of helium atoms, which are the débris shed at the various stages of the transformation.

Further, it is well established that a gram of uranium as found along with its products in rocks and minerals is now changing at a rate represented by the production of 1.88×10^{-11} grams of helium and 1.22×10^{10} grams of lead isotope per annum. There is every reason to believe that this is also the rate at which 1 gram of uranium has changed in the past, since the rate is unaffected by any change of temperature or pressure which we can apply.

Minerals containing uranium are always found to contain helium and lead. The helium may safely be treated as wholly a radioactive product. It would be difficult to account for its presence, retained mechanically in the mineral, in any other way. The lead in some cases conforms itself to the expected atomic weight of 206, about one unit lower than common lead, and in such cases we may safely regard the whole of it as a product of uranium disintegration.

Thus, take the broggerite found in the pre-Cambrian rocks at Moss, Norway. The lead in this monad has an atomic weight of 206.06 as determined by Hönigschmid and Fräulein St. Horovitz. The ratio of lead to uranium is .113. Taking the lead as all produced by uranium at the rate above given, we get an age of 925 million years. Some minerals from other archæan rocks in Norway give a rather longer age. A determination of the amount of helium in minerals gives an alternative method of estimating time. But helium, unlike lead, is liable to leak away, hence the estimate gives a minimum only. I have found in this way ages which, speaking generally, are about one-third of the values which estimates of lead have given, and are therefore generally confirmatory, having regard to leakage of helium. This method can be applied to material found in the younger formations. Thus the helium in an eocene iron ore indicated 30 million years at least.

H. N. Russel has recently applied the argument from accumulation of lead to the earth's crust as a whole. He takes the uranium as 7×10^{-6} of the whole, the lead as 22×10^{-6} of the whole. If all the lead were uranium lead, and had been generated since formation of the crust, the time required would be 11×10^{9} years. This is certainly too great. Allowing for the production of some of the lead by uranium, Russel finds 8×10^{9} years as the upper limit. This is about six times the age indicated by the oldest individual radioactive minerals that have been examined.

The upshot is that radioactive methods of estimation indicate a moderate multiple of 1,000 million years as the possible and probable duration of the earth's crust as suitable for the habitation of living beings, and that no other considerations from the side of physics or astronomy afford any definite presumption against this estimate. The argument from geology and biology I must leave to our colleagues from other sections. May I venture to say that I for one consider the topics with which they will deal as not less interesting and important than those which it has been my privilege to lay before you?

Prof. J. W. Gregory, F.R.S.—The claim that geological time must be restricted within a score or a few score million years was regarded by most geologists with incredulity, since a score million years was of little more use to geology than the seven days of the Pentateuch. Now that physical evidence allows the age of the earth to be counted by the thousand million years the problem is of less concern to the geologist, except from the hope that the uranium-lead ratio may fix geological dates in years, and from the interest of reconciling the conflicting results of the different methods.

The geological estimates to which most weight has been attached are based on the saltness of the sea. The salinity argument has been widely accepted as sound in principle; the estimates varied from 70 to 150 million years, and some intermediate length was regarded as inevitable. Allowances were made

for various factors; but they added only a few per cent. to the total, and did not multiply it by 10 or more.

The validity of the salinity argument may be tested by two checks—the supply of chlorine, and the denudation required to account for the amount of sodium; and as shown by Dr. A. Holmes, each of these indicates a much longer period than the sodium.

The supply of chlorine in igneous rocks is quite inadequate to convert their sodium into chloride. Most of the sodium chloride in river water is probably marine in origin, and only the sodium in the bicarbonate and sulphate is a fresh addition to the sea. On this ground the salinity estimate should be approximately doubled. Again, to obtain all the sodium in the sea from igneous rocks would involve the denudation of improbable volumes of them, and, at the rate usually accepted, the age of the earth should be multiplied three- or four-fold.

The fundamental objections to the salinity argument are against (1) its assumption that the sea was originally fresh, which palæontological evidence renders improbable. The oldest fauna, the Cambrian, has the characteristics of a marine fauna, and the contrast between the freshwater and marine faunas was as sharp in Palæozoic times as it is to-day. (2) Its omission to allow for the large supplies of sodium chloride raised from beneath the earth's surface by magmatic waters. (3) Its assumption of uniform denudation. The earth has probably undergone deformations that led to alternate periods of quick and slow crustal movement; during the times of repose the surface would have been planed down, rivers would have become sluggish and denudation slow. As the earth is now under the influence of a time of quick movement, denudation is faster than the average. A multiplication of the earth's age five-fold for this difference would not be excessive.

During quick crustal movement volcanic action would be more powerful, the discharge of hydrochloric acid and sodium in hot springs would be increased; and as denudation is now acting on land in which sodium chloride has been produced in unusual quantities by volcanic action the estimated age of the earth must be again extended. The rhythmic acceleration of geological processes lengthens the estimates based on sedimentation, but would affect the biological argument inversely, since at periods of rapid physical change biological change would have been quickened, and thus the occasional abrupt introduction of a new fauna does not necessitate so long an interval as has been thought.

The best-known geological estimates of the age of the earth require to be multiplied ten- or twenty-fold in order to agree with the physical estimates, but this increase is consistent with the geological evidence.

Prof. A. S. Eddington, F.R.S.—A study of the Cepheid variable stars affords strong evidence that the stars have other sources of energy besides that furnished by gravitational contraction. The rate of radiation by δ Cephei is such that it would be necessary for the density to increase 1 per cent. in 40 years in order to provide the required energy. The light-change of δ Cephei is believed to be due to a periodicity intrinsic in the star (*e.g.* pulsation); it is clear that such an intrinsic period cannot remain unaltered whilst the density changes so rapidly. But the observed change of period of δ Cephei is only $0^s.08$ per annum, or 1 per cent. in 58,000 years. The condition of δ Cephei is thus changing at a rate very much slower than that required by the contraction theory. The figures suggest that Lord Kelvin's time-scale should be lengthened 500-fold—at least during this stage of the evolution.

Prof. Sollas, F.R.S., also took part in the discussion.

28. Prof. R. A. Sampson, F.R.S.—*The Microchronograph.*

The instrument described is in actual use at the Royal Observatory, Edinburgh, for registering clock times of any signal accurately to 0.001 sec., for the purpose of examining short-period changes in the relative rates of two clocks, or the lag of a controlled clock, or of one part of a piece of apparatus with respect to another. It has many other applications. The means employed are an adaptation of the oscillograph; this instrument can be so constructed as to be completely free from noxious or variable lag of its own. A detector

is placed between the poles of a powerful electro-magnet. This detector consists of a short loop of fine platinoid wire, under tension, and spanned by a small plane mirror. The clock signal or other signal being given as an electric current of a few milliampères is recorded photographically on a moving film by help of a minute rotation of the mirror. The rate of motion of the film cannot be trusted for fine measurement over an interval of one second; therefore the standard is taken from an interrupter which cuts off the light falling on the mirror at intervals of approximately 0.1 sec. This interrupter is a simple steel tongue, set in vibration and not maintained in any way, and carrying a wire which occults a slit through which the light passes.

The scale employed can be varied within wide limits. That used most frequently at present gives a motion of the film of about 3·3 cm. per second, with a lateral displacement of 0.13 mm. per milliamp., corresponding to a magnification of the movement of the wire loop of the detector by about 1,000.

Exhaustive tests have shown that the instrument is completely reliable.

29. Rev. A. L. Cortie.—*The Magnetic Storms of the Present Solar Cycle.*

A comparative table of magnetic and solar phenomena, including sun-spots, bright prominences, H_α absorption markings, and calcium flocculi, has been constructed for the years 1913-1921 (June) from the records of the Stonyhurst, the Tortosa, and the Kodaikanal Observatories. Sunspots attained their their maximum in 1917; bright prominences their maximum profile-area in 1915 and in 1917; bright calcium flocculi and dark H_α absorption markings their maximum area in 1920. The greatest number of magnetic disturbances, g., and v.g., took place in 1919. A g. disturbance is one in which the range exceeds the mean daily range of the five quietest days, in D and H combined, by 15′. If the excess is 20′ and over it is marked v.g.

1. The general magnetic activity in this solar cycle would appear to coincide with the mean daily area of the calcium flocculi, and the hydrogen absorption markings.

2. Consistently with the results for past solar cycles, the magnetic phenomena increase in activity, as the sun-spots and the flocculi decrease in mean latitude, after the sun-spot maximum has been attained. This is due to the fact that, on the whole, disturbed areas on the sun are more effective when they are situated in or near the heliographic latitude of the earth.

3. There have been four outstanding magnetic storms of great violence during the present cycle: (*a*) 1917, August 9, 10, renewed on August 13, 14; (*b*) 1919, August 11, 12; (*c*) 1920, March 22, 23; (*d*) 1921, May 12-21. In the cases of (*a*), (*c*), and (*d*) the magnetic storms were coincident with the passage of very great sun-spots across the sun's disc. The most active period of sun-spots was 1917, August 6-16, and a big active group, lat. $+16°$, maximum area 3,444 on August 9, was near the central meridian when the series of magnetic disturbances began. In 1920 there was but one greatly disturbed area, mean latitude $-6°$, which extended in longitude 36°, and which was active from 1919, December 27, to 1920, May 16. Its maximum area was 3,652 on March 23, when a series of synodical magnetic disturbances culminated in a violent storm. It was also near the central meridian, and in latitude $-5°$. Similarly the protracted storm of 1921 coincided in its maximum phase with the central meridian passage of a very large sun-spot group, area 3,300, on May 14.

These cases support the view that the action of a sun-spot area, as the necessary condition of a magnetic storm, is by the projection of a set of divergent rays proceeding fan-wise from the area, and probably diffused into a cloud-belt. For some of the series of magnetic storms, accompanying the passage of the sun-spot area, occur when the sun-spot is far removed from the central meridian.

But with regard to the violent magnetic storm of 1919, August 11, 12, the largest sun-spot group of the year, a triple equatorial group, extending 20° in longitude, was approaching the sun's E limb on August 11. This very large group formed a procession with another, spectroscopically very active, group that had preceded it, so that the same region of the sun was disturbed from August 4 to August 24. The violent magnetic storm might have been connected with the greatly disturbed area which passed the central meridian

August 9. But if, consistently with the three other violent storms of this cycle, it was connected with the large disturbed area, the case is decisive against a radial discharge of electrical particles. It is not inconsistent with the hypothesis of a wide diffusion of electrical particles, by mutual repulsion, in the form of a belt of clouds round the sun.

30. Prof. G. Forbes.—*Radial Velocity of Stars.*

31. *Report of the Seismology Committee.* See p. 206.

32. *Report of the Committee on Tides.* See p. 217.

SECTION B.—CHEMISTRY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 464.)

Thursday, September 8.

1. Presidential Address by Dr. M. O. Forster, F.R.S., on *The Laboratory of the Living Organism.* See p. 36.

2. Prof. R. Robinson, F.R.S.—*The Genesis of Plant Pigments and Related Substances.*

Two types of naturally occurring substances containing a C_{15} nucleus can be distinguished : those related to the sesquiterpene series and probably derived by polymerisation of three isoprene molecules, and those which are most easily regarded as condensation products of carbohydrates. Santonin, for example, clearly belongs to the former class, whilst the greater number of plant pigments belong to the latter. The molecules of the flavones, flavonols, anthocyans and related substances contain two aromatic nuclei (A and B) which in different members of the series exhibit a varying state of oxidation. Nucleus A is normally present as a derivative of trihydroxybenzene, whilst the normal condition of B is that represented by a dihydroxybenzene. This is the natural consequence of an hypothesis the main feature of which is that each nucleus is derived from a hexose, and that these are connected by aldol-condensations with glycerose (dihydroxyacetone). The details of the molecular changes required in order to reach individual flavones, flavonols, &c., involve no unusual assumptions. Brasilin and hæmatoxylin are derived by the introduction of a molecule of formaldehyde to the complex, and the relation is very similar to that between the papaverine and berberine groups. The theory is extended to plant pigments of the naphthalene and anthracene series.

Friday, September 9.

3. Prof. H. E. Fierz.—*The Modern Dyestuff Industry.*

Figures are given indicating the relative importance of the artificial dyestuff industry and of other principal industries. Whilst in 1913 95 per cent. of this industry was in the hands of Germany, it is now widely distributed. Its relation to the manufacture of other classes of chemical substances is shown, and it is maintained that the future existence of a self-contained dyestuff industry is impossible, and that it must form an integral part of a much larger organisation.

4. Joint Discussion with Section A on *The Structure of Molecules.* See p. 468.

Monday, September 12.

5. Joint Discussion with Section I on *Biochemistry.*

Prof. W. Gowland Hopkins, F.R.S.—*Oxidations and Oxidative Mechanisms in Living Tissues.*

The reactions and mechanisms involved in the oxidation of foodstuffs with special reference to the following : The β-oxidation of fatty acids ; the equilibrium between dextrose and lactic acid, and the oxidation of the latter ; general aspects of the oxidation of amino-acids, and the particular

cases of the rupture of the benzene and indol rings in tyrosine and tryptophane respectively. Mechanisms of oxidation at the temperature and reaction of the animal body; activisation of oxygen; the probable importance in physiological oxidations of the activisation and transport of hydrogen; Wieland's views; the factors involved in the mobilisation of hydrogen atoms.

6. Prof. E. C. C. Baly, F.R.S., Prof. I. M. Heilbron, and W. F. Barker.—*The Synthesis of Formaldehyde and Carbohydrates from Carbon Dioxide and Water.*

7. Prof. F. M. Jaeger.—*The Decomposition of Simple Organic Acids by Ultra-Violet Radiation.*

The author describes experiments on the action of the ultra-violet rays from a mercury arc on aqueous solutions of simple organic acids and their salts. It is shown that the course of the reaction for each substance under given conditions of radiation is highly dependent on the presence or absence of special ions, which appear to act as directing catalysts.

8. Prof. G. Barger, F.R.S.—*Demonstration of Micro-Analysis of Compounds containing Carbon, Hydrogen, and Nitrogen.*

9. Reports of Research Committees. See pp. 243-250.

Tuesday, September 13.

10. Prof. F. M. Jaeger.—*The Measurement of Surface Tension over a wide range of Temperature.*

A method is described and illustrated which allows of the accurate determination of surface tensions between $-80°$ and $+1625°$. A slow current of nitrogen enters the liquid through a platinum capillary immersed to a known depth, so that small gas bubbles are formed at the sharp edge of the tube, and the minimum gas pressure needed to cause the gas bubbles to burst is measured. This pressure is connected in a known way with the radius of the tube, the density of the liquid at the temperature given, and the surface tension sought. The method has been applied to many organic substances and inorganic salts, and the results are reviewed.

11. Mr. Cosmo Johns.—*The Surface of Liquid Steel.*

12. Prof. C. H. Desch.—*Surface Tension in the Solidification of Metals.*

The forms of the grains in solidified metals are examined and compared with those of foam cells. A close correspondence is found, indicating that the form of the grain boundaries is determined by surface tension.

13. Dr. J. S. Owens.—*Suspended Impurities in City Air.*

This paper describes experiments carried out for the Advisory Committee on Atmospheric Pollution. The quantity of suspended matter in London air has been determined by means of automatic apparatus at South Kensington, Kew Observatory, and Westminster for several months, and curves are given. A new and simple method of dust measurement is described. A measured volume is drawn through a small cell, in which the dust is deposited on a cover glass, which can be removed, mounted for examination, and preserved for reference.

14. Mr. W. Thomson.—*Determinations of the Smoke Impurities in the Air of Manchester.*

Afternoon visits were arranged to the Chemical Laboratories of the University, Liberton, and of the Heriot-Watt College; to Messrs. Wm. Younger & Co., Abbey Brewery; Messrs. Duncan, Flockhart & Co., Manufacturing Chemists; The North British Rubber Co., Castle Mills; and The Broxburn Oil Co.

SECTION C.—GEOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 464.)

Thursday, September 8.

1. Prof. T. J. Jehu.—*The Geology of the Edinburgh District.*

2. Prof. L. W. Collet.—*On the Origin of the Swiss Lakes.*

3. Presidential Address by Dr. J. S. Flett, F.R.S., on *Experimental Geology.* See p. 56.

Friday, September 9.

4. Joint Meeting with Section K. Discussion on *The Oldest Land Flora* opened by

Prof. W. H. Lang, F.R.S.—*The Flora of the Rhynie Chert Bed.*

(1) List of Plants.

Vascular Cryptogams. *Rhynia Gwynne-Vaughani, R. major, Hornea Lignieri, Asteroxylon Mackiei.*

Fungi. Numerous saprophytic forms; hyphæ usually non-septate, bearing vesicles or resting spores.

Algæ. *Algites* (*Palæonitella*) *Cranii*; the vegetative organs suggest comparisons with Characeæ.

Schizophyta. *Archæothrix oscillatoriformis* and probably unicellular bacteria.

Incertæ sedis. *Nematophyton Taiti.*

(2) The ascertained succession of the plants throughout a vertical section of the chert bed.

(3) Indications of conditions of accumulation of the deposit.

(4) Interest of the vascular plants. Comparative morphology and affinities. Physiological anatomy; probable habitat and mode of life.

(5) Interest of the occurrence of *Nematophyton* in the deposit.

5. Prof. L. W. Collet.—*Alpine Tectonics.* Followed by a discussion on *Alpine and Scottish Tectonics.*

Monday, September 12.

6. Mr. H. M. Cadell.—*Evidence from recent Bores in the Carboniferous Rocks of Scotland.* Followed by a discussion on *The Search for Oil in Scotland.*

7. Joint Meeting with Section G. Discussion on *The Mid-Scotland Canal.*

Tuesday, September 13.

8. Joint Meeting with Sections A, D, and K. Discussion on *The Age of the Earth.* See p. 413.

9. Mr. F. Dixey.—*The Magnesian Group of Igneous Rocks.*

Rocks rich in magnesium are abundantly developed amongst the older rocks of the Earth's crust, and it is proposed in this paper to distinguish them as a Magnesian Group of Igneous Rocks. The rocks of this group are characterised mainly by richness in magnesium and iron, especially in the less siliceous varieties, and by wide variation in silica content. Hypersthene is usually in large amount, and in many cases garnet is a common constituent. These rocks are developed in a number of Magnesian Provinces, of which Southern India may be regarded as a typical example. Rocks belonging to the Magnesian Group were erupted much more extensively and more frequently in pre-Cambrian than

in later times. The group includes, for example, the 'Charnockite-Anorthosite Series' of Rosenbusch, and also the large magnesian intrusives commonly associated with this series.

10. Mr. H. H. Read.—*The Contaminated Magmas of Aberdeenshire.*

SECTION D.—ZOOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 465.)

Thursday, September 8.

1. Presidential Address by Prof. E. S. Goodrich, *F.R.S.* See p. 75.

2. Joint Meeting with Section K. Discussion on *Forest Insect Problems.* See p. 451.

3. Dr. F. A. E. Crew.—*The Mechanism of Sex-reversal in Frogs as illustrated by the Recorded Cases of Abnormality of the Reproductive System and by the Results of a Breeding Experiment.*

The cases are arranged so that they form a complete series beginning with an individual which in primary and secondary sexual characters and in accessory sexual apparatus is an almost perfect female, and ending with one which is an almost perfect male. It is shown that the process is initiated by the appearance upon the inner border of what hitherto has been a typical and functioning ovary of a nodule of spermatic tissue. In such an ovo-testis the ovarian tissue degenerates and becomes removed, whilst the spermatic thrives until the gonad assumes the form and structure of a typical testis. Simultaneously the individual develops the secondary sexual characters and accessory apparatus of the male, and the only female character which persists is the Müllerian ducts of a size equal to that of the oviduct of the adult female. Such an individual functions as a male. One such fertilised the eggs of a normal female, and every one of the offspring reared to an age at which the sex could be identified proved to be a female. This is what one would expect if the chromosome constitution of the frog is of the XY type, for an XX ♂ mated with an XX ♀ would produce a generation all XX in chromosome constitution.

4. Ruth C. Bamber (Mrs. Bisbee).—*The Male Tortoiseshell Cat and a Suggestion on Sex-determination.*

Male tortoiseshell cat.—No case of confluence of blood-vessels found in embryo cats. Two records of tortoiseshell males from otherwise female families. Thus parallel with freemartin, suggested by Doncaster, seems improbable. But tortoiseshell male may be case of sex reversal at fertilisation. *Cf.* Riddle's pigeons, Goldschmidt's and Harrison's moths. (I believe that Dakin thinks tortoiseshell male comparable with Goldschmidt's intersexes.) Difference between yellow male and tortoiseshell female possibly due only to sex physiology; if tortoiseshell male an example of sex reversal physiology probably incompletely male (*cf.* usual sterility), then tortoiseshell coloration expected.

Sex-determination.—Suggestion that sex due to metabolic rhythm, male and female metabolic conditions tending to alternate; *cf.* chemical rhythms. At fertilisation metabolism turned in either direction according to relative 'strengths' of germ cells. Chromosome content, and sex, possibly parallel results of metabolic condition. Avoids split in living universe made necessary by X chromosome hypothesis.

5. Prof. J. Arthur Thomson.—*Are there 'Modification Species'?*

An Antipatharian colony with which a Polychæt worm is associated departs altogether from its ordinary mode of growth and forms a basket-work tunnel in which the worm lives. Similarly, in a Primnoid colony, the presence of a

Polychæt is associated with drastic changes in the shape and size of the spicules forming the tunnel. The question arises whether other environmental peculiarities may not result in 'Modification Species' in many corners of the animal kingdom.

6. Prof. J. Arthur Thomson.—*A Long-lost Alcyonarian.*

In 1882 G. von Koch briefly described from near Naples *Gorgonella bianci*, n.sp. His note was compressed into a few lines. In his famous Naples Monograph (1887) he remarked that he had not seen it again. A few years ago a specimen (dated 1879) was received from Naples which corresponded entirely with the notes made by von Koch, and showed that he was right in establishing a new species. It is quite distinct from other Mediterranean Gorgonids and deserves more complete diagnosis. Its name should be *Leptogorgia bianci*, von Koch.

7. Prof. J. Cossar Ewart, F.R.S.—*The Structure, Development, and Origin of Feathers.*

It has been asserted that if we assume birds are descended from reptilian ancestors, we may assume feathers are modified scales. Evidence of the descent of birds from reptiles we have in *Archæopteryx*, but the geological record affords no evidence in support of the assumption that feathers are modified scales. Our only chance, in the absence of fossils, of ascertaining how feathers were evolved is by studying the nestling feathers (*prepennæ*) which precede the true feathers (*pennæ*). This study suggests (1) that feathers were not originally acquired to enable birds to fly, but to prevent the loss of heat; (2) that when birds were in the making the coat consisted for a time of simple umbels, like those forming the first (protoptile) coat of penguins; (3) that when a cold, dry period was succeeded by a cold, wet period the original downy coat was succeeded by a fur-like coat like the second (mesoptile) plumage of penguins; (4) that in most modern birds this second (mesoptile) coat has been completely, or all but completely, suppressed; (5) that feathers, like hairs, were formed, not out of scales, but out of the skin lying under and between scales.

Believers in the scale origin of feathers have hitherto assumed that a perfect true feather consists of a single shaft or blade—that, *e.g.*, the long aftershaft of the emu feather is a secondarily acquired feature. The developing feathers of the emu, grouse, and many other birds conclusively prove, however, that complete true feathers (*pennæ*), like true down feathers (*plumulæ*), consist of two shafts which are practically alike in origin and structure.

Friday, September 9.

8. Prof. D. M. S. Watson.—*Dry Land and the Origin of the Bony Vertebrates.*

The bony vertebrates differ from all others in the presence in all of a lung or air bladder, and in the Dipnoi, Amphibia, and Polypterus of larval external gills. The air bladder is clearly homologous with the lung. It is actually used as a lung in the relatively primitive Polypterus, Lepidosteus, and Amia. It is not possible to conceive of its origin primarily as a hydrostatic organ. Hence it arose as a lung. A marine fish has no obvious need of accessory respiratory organs of the type of a lung if it lives a normal life; therefore the lung presumably arose in a fresh-water fish. The conclusion of Joseph Barrell that it develops first in fish living in the intermittently running streams of an arid district with seasonal rainfall is adopted. It hence follows that all known bony fishes are derived originally from fresh-water forms. External gills do not occur in larval Neoceratodus, whose eggs are laid separately amongst the Valisneria in large deep pools where the aeration is comparatively good. They do occur in Lepidosiren and Protopterus, which lay large masses of eggs in small definite nests where the conditions of aeration are shown to be very bad by the development of functional external gills by teleost larvæ. It is hence natural to believe that external gills first arose under the same conditions as lungs in fish which remained under these conditions after the Actinopterygii had left them for more normal regions. The external gills may have been originally developed for the purpose for which they are actually used by

Protopterus, to carry the embryo over the difficult period before the internal gills and lung become functional. The systematic position of Polypterus is quite uncertain. It is certainly not a direct descendant of the Osteolepids, but appears to be of Actinopterygian origin. If so, it is not improbable that its external gill, borne on the hyoid arch instead of the branchial arches as in Amphibia and Dipnoi, may have been independently acquired under the pressure of similar circumstances.

9. Prof. J. F. VAN BEMMELEN.—*The Colour-markings of Mimetic Butterflies.*

The author has made a study of the colour-markings of mimetic butterflies from a strictly morphological standpoint. He concludes that the resemblances between the mimetic forms and their so-called models may be interpreted as due to the retention of a primitive type of coloration, the mimetic forms being more, and not less, primitive than the non-mimetic members of the same genera. The advantage of protection gained by their resemblance to the models may be a result but is not a cause of the resemblance.

10. Mr. D. WARD CUTLER.—*Recent Investigations on Soil Protozoa.*

The investigations of Russell and Hutchinson on partial sterilisation of the soil led to the view that in normal soil the increase in bacterial numbers was inhibited by a biological factor provisionally regarded as the soil protozoa. The first counting methods used were unsatisfactory, since only the total numbers were found; no distinction was drawn between the active and cystic conditions. A satisfactory method has recently been devised. Since July 5, 1920, consecutive daily soil samples have been taken for 365 days, and, by the new method, the active and cystic numbers found for fourteen species of protozoa. The data obtained show that an inverse relationship exists between the bacterial and active amœbic numbers; when the latter are high the former are low. The bacteria are not greatly affected by the flagellates, but in one species (*Oicomonas termo*, Martin) the active numbers show a periodicity which also obtains in artificial cultures.

11. Dr. NELSON ANNANDALE.—*The Biological Aspect of Taxonomy.*

The paper was a plea for a broader concept of Taxonomy. The author, while recognising the importance of type-specimens, protested against the tendency to base identifications, and thence systems of classification, solely or mainly on the comparison of other specimens with the types of species. He insisted on the importance of examining large series of individuals of precisely known *provenance*, of discriminating between morphological and adaptive characters, and of the closest possible connection between field observations and museum work. Practical suggestions were offered for the carrying out of these reforms, such as frequent exchange of officers between British biological institutions and those in the Colonies and India; the introduction into British museums of the colonial principle of the 'Sabbatical Year,' and the preparation of local 'faunas' locally.

12. Reports of Committees. See p. 260.

13. Sir SIDNEY F. HARMER, K.B.E., F.R.S.—*Modern Whaling.*

The examination of reports by Government officials, supplemented by voluminous statistics which have been furnished by the whaling companies, at South Georgia and elsewhere, to the British Museum (Natural History), has shown that there are indications of a serious diminution in the number of whales, in an industry which commenced so recently as the end of 1904. In the case of the humpback, a marked decrease in the number of individuals captured commenced after the end of the season 1911-12; while there is reason to fear that a similar decline in the numbers of blue whales commenced after the end of the season 1917-18.

The study of the records of the lengths of fœtuses obtained principally at South Georgia in the South, and from various localities in the north, has led to the conclusion that in each of the three species (humpback, fin whale, blue whale) principally hunted there is a period during which pairing takes place with maximum frequency. In the southern whales the periods in question

appear to occur approximately from July to September, and in the northern whales approximately from January to April. If these conclusions are correct the principal breeding periods, in each hemisphere, are in winter and early spring, when the majority of the whales have left the localities in which they are mostly hunted. Fœtal specimens of a given length are thus most numerous during periods about six months apart in the two hemispheres. The duration of pregnancy does not seem to exceed twelve months; and evidence has been obtained that in the fin whale the rate of growth in the fœtus is about two feet a month, during a considerable part of the period of gestation.

14. Dr. JAMES RITCHIE.—*Giant Squids on the Scottish Coast.*

Records of the occurrence of Giant Squids on the shores of the British Isles are exceedingly sparse, a fact scarcely to be wondered at, since the headquarters of the group appear to be in the cold waters bordering the Arctic regions. Of the previous records the most interesting hail from the West Coast of Ireland; but a few years ago the writer examined near Dunbar a large individual of *Architeuthis harveyi*, having tentacular arms 14 feet long, and since that time a series of interesting occurrences have been noted. These include, on the east coast, a living example of *Sthenoteuthis pteropus* recently cast ashore at North Berwick, and a specimen of *Architeuthis harveyi* in Caithness, while the west coast has afforded a single specimen of the former species. Representative portions of these individuals are preserved in the Royal Scottish Museum.

The Section was entertained at the Scottish Zoological Park by the President and Council of the Zoological Society of Scotland.

Saturday, September 10.

An excursion to Dunkeld took place under the leadership of Dr. STEWART MACDOUGALL.

Monday, September 12.

15. Prof. J. W. VAN WIGHE.—*Demonstration of Wax Model of the Skull of Acanthias Embryo* (39.5 *mm. long*).

16. Joint Meeting with Section J. Discussion on *Instinctive Behaviour.*

Dr. J. DREVER.—The psychologist maintains that no adequate scientific account of instinct is possible without taking into consideration psychological factors. Behaviour implies the total response of an organism to a situation. According to common usage, so far as this response is not determined by what has happened to the organism in its individual past history, it is said to be instinctive. This usage requires some qualification, and also limitation and elucidation, if it is to be adopted for scientific purposes. To take a particular instance, we require to find some way of marking off instinctive behaviour from the unconditioned reflex. It is at some such point that the psychological account would appear to come in. Those responses of the organism which are instinctive involve processes which, regarding them from the inner standpoint we as continuous beings are capable of taking, we denominate 'experience.' The charge of anthropomorphism urged against this method of interpretation is not so well founded as it appears at first sight. If we adopt a 'hylomorphic' method of interpretation, there are certain aspects of what might be called the 'experience phase' of behaviour which must be simply ignored by science. Even if McDougall's view be accepted, that the emotion is not a secondary development or a disorder of an instinct, this difficulty still remains.

Prof. E. S. GOODRICH, F.R.S.—The word instinct has been used with so many different meanings that I shall restrict myself to the discussion, not of instinct,

but of instinctive behaviour, or, in other words, instinctive response to a stimulus or complex of stimuli.

Can we distinguish instinctive from other behaviour? Most psychologists seem to draw a distinction, sometimes a sharp one, between instinctive and intelligent behaviour. But I have looked in vain for any definition of the one which will exclude the other.

The most generally accepted view seems to be that instinctive behaviour depends on an inherited mechanism (variously called innate dispositions, congenital prearrangements, and the like), while intelligent behaviour depends on experience acquired during the lifetime of the individual. Instinctive behaviour is said to be the working of a preformed mechanism set going by an appropriate releasing stimulus, and therefore to be definite, relatively invariable, inherited. Intelligent behaviour is contrasted as indefinite, variable, not inherited but acquired.

But all behaviour is made up of responses, and these, like structural responses or character, all depend, on the one hand, on transmitted factors of inheritance, and, on the other, on the moulding conditions of the environment. As I tried to show in my Presidential Address, a response can never be transmitted in inheritance as such, nor can it occur unless both the necessary conditions and the transmitted factors are present. Instinctive response is neither more nor less acquired than intelligent response. What is the innate mechanism, said to be inherited, but the result of reactions to internal as well as external environmental conditions; the product of the past experience of the individual in which it is developed (using the word 'experience' as a biologist and not as a metaphysician)? There is no hard and fast line to be drawn between intelligent and instinctive behaviour. In so far as it depends on a mechanism yielding a closely interlocked series of reactions specialised and adapted to respond in one direction to a particular evocative stimulus, behaviour may be said to be instinctive. In so far as it depends on a mechanism delicately balanced so as to respond in a variety of ways to environmental stimuli differing perhaps only slightly in nature and intensity, behaviour may be said to be intelligent. Instinctive behaviour corresponds to what we call constancy of structure in bodily characters; intelligent behaviour to modifiability or individual adaptability.

Prof. J. ARTHUR THOMSON.—(1) Is it possible to distinguish different grades of instinctive behaviour, e.g., from bee to bird? (2) If instinctive behaviour, physiologically regarded, is made up of a succession of reflex actions, what evidence can be adduced in support of the view that there is sometimes a subjective aspect of appreciative awareness and endeavour? (3) May one venture to suggest a provisional diagram of the inclined plane of animal behaviour?

17. Mr. F. A. POTTS.—*The Work of the Carnegie Institution of Washington in the Pacific.*

The Department of Marine Biology of the above institution has, apart from its essential work in the West Indies, conducted a series of expeditions in the Pacific. In 1913 Dr. Mayer made an intensive study of Murray Island in Torres Straits. During the last few years the work, in which a number of specialists have engaged, has centred round the island of Tutuila in the Samoan group. The Samoan reefs are not so rich as the Great Barrier Reef, but in the harbour of Pago Pago, Tutuila, the best conditions occur for the study of coral reefs. Exhaustive experiments on the rate of growth of corals and the influence of varying conditions have been made. The reefs have been explored by diving, and the habits and colouration of the fish fauna studied by this method. The reef has been drilled in several places and a great deal of attention has been devoted to the geological history and the botany.

18. Prof. GEORGE H. CARPENTER.—*Warble-flies: a Study in Development and Adaptation.*

The later stages in the life-history of the Warble-flies (*Hypoderma*) of cattle have been well known since the researches of Bracy Clark early last century. Only during the last few years, however, has it been shown—by Hadwen in Canada, Gläser in Germany, and the writer and his colleagues in

Ireland—that the minute spinose maggots with powerful mouth-hooks, after hatching from the eggs laid on the host's hairs, bore directly into the skin, and that the presence of second-stage maggots in the gullet wall of the host indicates a resting-place in the migrations of the larvæ to their final position beneath the skin of the back. The changes in structure undergone by the maggots—notably the reduction in the mouth armature through the successive stages, afford interesting examples in adaptation for specialised parasitic life relations. The presence of a series of six pairs of vestigial lateral spiracles in the fourth stage larva is a feature of morphological importance in connection with the degenerative modification shown by muscoid maggots generally. Attempts to exterminate these parasites in definite geographical areas show that the task is more difficult than has been hitherto supposed, as treatment for destroying the 'ripe' maggots must be continued through a period of five months.

19. Mr. F. Balfour Browne.—*Sapyga 5-punctata, a Fossorial Wasp Parasitic upon Bees.*

The relationship between the parasite and the Blue Osmia, *O. ænea* (*cærulescens*) was investigated and various experiments were made. It was found that larvæ hatched in the cells of the Red Osmia (*O. rufa*) never survived, but experiment showed that this was only because the pollen paste stored by the latter bee is drier than that stored by the Blue Osmia, and if this dry paste was moistened with treacle or honey the food was quite suitable for the Sapyga. Experiments in feeding the Sapyga larvæ, either on egg-food alone or upon pollen paste alone were inconclusive owing to scarcity of material, but they seemed to indicate that the egg of the bee was necessary as a preliminary to the pollen-paste diet. As to the origin of this case of parasitism, there are difficulties in the way of regarding it as a 'mutation,' and it is suggested that the wasp originally oviposited in bee-cells containing full-grown larvæ, animal food being the normal diet of wasp larvæ; that in the course of time the wasp appeared earlier in the season—or the bee later—and the wasp larva completed its development on the pollen-paste diet, until now it only requires the egg of its host to enable it to pass through its first stage, after which the pollen paste supplies the necessary nourishment.

The afternoon session was followed by a visit to Roslin Glen. Leader: Dr. Walter Ritchie.

Tuesday, September 13.

20. Mr. T. A. Stephenson.—*Zoanthactiniaria: a Study in Classificatory Method.*

Among Actiniaria classificatory work must at present deal with genera and families, species being too little understood. A search for a satisfactory method of classification reveals that no system of unit-characters is reliable, the most adequate basis for work being the sum of the more important characters; this method resulting in a scheme representing relationships and evolution of whole animals and not of isolated aspects of their structure. Actiniaria may be traced from a small eight-rayed plankton-swimmer, the main evolutionary line dividing into anemones and corals and leaving curiosities behind it, the anemone branch, after shedding out further minority forms, itself dividing into two main lines of tendency. It is suggested that the mode of classification advocated for anemones might be applied to other animal groups, and that this might be a step in the direction of realising some fuller conception of classification, the need for such a conception being even now foreshadowed.

21. Joint Meeting with Sections A, C, and K. Discussion on *The Age of the Earth.* See p. 413.

22. Dr. John Rennie.—*Acarine Disease in Hive Bees.*

The paper gives an account of the researches of Harvey, White, and Rennie. The term 'Isle of Wight Disease' is now no longer possible as a designation for adult bee disease in this country, since it has been found to cover the

condition known as microsporidiosis or Nosema disease; May sickness, a temporary malady of bee colonies, claimed by Turesson to be caused by moulds in pollen or honey; and Acarine disease, resulting from the invasion of the tracheal system by the mite *Tarsonemus woodi*, n.sp. The overwhelming majority of cases of adult bee disease in this country are cases of Acarine disease. *Tarsonemus woodi*, a member of the family Tarsonemidæ, is a heterostigmatic mite, only the females possessing a tracheate respiratory system. It invades the anterior thoracic tracheal system, breeding and feeding in this situation. Only adults, the males rarely, are found outside the bee. Inflammatory reactions result in the chitinisation of the tracheal wall, the oxygen supply is vitiated and restricted, and there is evidence of muscular degeneration. The disease is chronic; bees may live a long time even after they are unable to fly, but they cease to share in the co-operative work of the colony. The course of the disease within a colony varies according to the size of the colony, the age and fertility of the queen, the type of original infection, whether multiple or not, and possibly the breed of the bee.

23. Prof. J. F. Gemmill.—*On the Bionomics of the Wheat-bulb Disease Fly, Lepto-hylemyia coarctata, Fall.*

The larva causes widespread damage to young wheat. In Scotland there is only one generation in each year. The eggs are laid on bare soil, preferably on potato fields between the plants, in autumn. The larvæ, which hatch out in early spring, pupate in early summer, and the flies emerge towards the end of June. Newly hatched larvæ seek out and bore into young wheat plants near the bulb, destroying the inner tissue, and migrating when necessary to other shoots. Couch-grass, rye, barley, and a garden ribbon grass are highly susceptible and can nourish the larvæ from emergence up to normal pupation. Larvæ taken from any of them can complete their growth in wheat, and *vice versa*. Oats and many field and sand grasses are immune. In nature, couch-grass appears to serve as the main 'reservoir' for the insect, apart from wheat. The influence on the eggs and larvæ of cold, moisture, depth of ploughing, and various manures and disinfectants, is outlined. Remedial measures should aim at prevention and the courses to be adopted follow from the life-history.

24. Major F. W. Cragg, M.D.—*Observations on the Organ of Berlese in Cimex and on the Behaviour of the Spermatozoa.*

The author drew attention to the special features of the reproductive system of the female *Cimex hemiptera*, Fabricius, pointing out that the sperms pass into an asymmetrical organ (organ of Berlese) through minute pores (organ of Ribaga). Later they pass through its walls into the body cavity, then into the spermathecæ, and into the walls of the oviduct, up which they pass to the follicular epithelium, penetrating it to fertilise the eggs. The development of the embryo begins while the egg is in the ovariole. A number of sections demonstrating these appearances were shown.

25. Dr. Nelson Annandale.—*Convergence in Shell-sculpture in the Viviparidæ.*

The Viviparidæ, a family of Gastropod Molluscs of ancient origin and of wide range in all geographical regions except the Neotropical, have, as a rule, smooth shells. At certain places and at certain geological periods the shells of at any rate a large proportion of the species have become sculptured in a peculiar manner. The most conspicuous instances are the late tertiary Viviparidæ of Slavonia, those of the Greek Archipelago, the living and sub-fossil *Margaryæ* of the lakes of Yunnan, and the living and sub-fossil *Taiæ* of the Inlé basin in the Southern Shan States. The differences in shell-structure between the various series were discussed and the anatomical basis for the sculpture described.

26. Major W. S. Patton.—*The Diptera which cause Myiasis in Man and Animals in India.*

Recently, with the help of a grant from the Indian Research Fund Association, an appeal was issued to all medical and veterinary officers in India, Assam, and Burma for living Dipterous larvæ from cases of myiasis. By breeding out

the flies from the hundreds of larvæ sent it was discovered that *Chrysomyia bezziana* Villeneuve caused all forms of cutaneous and wound myiasis, as well as oral, aural, nasal, ocular, and vaginal myiasis. This species is well known in Africa, where it only attacks animals. *Chrysomyia megacephala* Fabricius (*dux* Esch; *flaviceps* Macquart) and *Lucilia argyricephala* Macquart (*serenissima* Walker) only cause cutaneous myiasis in their larval stages in animals. *Aphiochæta xanthina* Speiser (*ferruginea* Brunetti) causes cutaneous and intestinal myiasis, and *Musca* (*Philæmatomyia*) *crassirostris* Stein may cause intestinal myiasis in man. A species of *Sarcophaga*, which it is not possible to identify at present, occasionally causes cutaneous and intestinal myiasis in man.

27. Prof. MATTHAI.—*Some Observations on the Marine Biology of Karachi and Tuticorin.*

28. Prof. W. C. MCINTOSH, F.R.S.—*A Century of Zoology in Edinburgh.*

29. Prof. E. HÉROUARD.—*Suite à la communication sur* Hydra tuba *faite par Sir J. G. Dalyell au meeting de la British Association tenu à Edinburgh en* 1834. (Communication taken as read.)

Sir J. G. Dalyell dans sons beau mémoire intitulé *Rare and remarkable animals of Scotland*, paru en 1847 et dans lequel il expose les observations qu'il a poursuivies pendant dix années sur la vie de *Hydra tuba*, rappelle qu'il fit une communication orale en 1834 au meeting d'Edinburgh sur ce sujet, devançant ainsi Sars, qui ne fit paraître son travail sur le Scyphistome qu'en 1838. Dans ce mémoire Sir John expose avec une très grande précision le bourgeonnement stolonial normal de *Hydra tuba* et la formation des Ephyra et, en outre, il relate un certain nombre d'observations qui restèrent pour lui énigmatiques.

Pseudoplanula tentaculaires.—Sir J. Dalyell dit avoir observé dans ses cultures des 'white fleshy corpusculum in motion' dont il ignorait la provenance et la destination. Ces corpuscules sont ce que j'ai appelé des pseudoplanula tentaculaires. On constate, en effet, que des polypes surabondam-

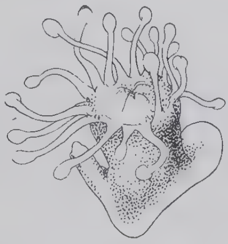


FIG. 1.

ment nourris tombent en dépression (au sens de Hertwig). Quand cette dépression est peu accentuée, le polype, après être resté un certain temps dans son état d'affaissement, recouvre sa tonicité et reprend sa vie normale. Mais, si au contraire l'état de dépression est accentué, on voit les tentacules se transformer en organes rhopaliformes, puis devenir capités et bientôt ces extrémités renflées se détachent du polype, tombent au fond du vase et se mettent à tourner sous l'action de leurs cils vibratiles. Ce sont là les 'white fleshy corpusculum in motion' observés par Dalyell. Après quelques jours de rotation ils s'arrêtent, se fixent au fond du vase et finalement reforment un Polype. C'est en somme un bourgeonnement multiple spécial, pouvant donner une vingtaine de polypes simultanément. La dépression et ses causes avait été indiquée par Dalyell, car il dit (p. 88) qu'un animal repu est aplati, 'as if incapable of sustaining itself upright': nous venons d'indiquer le rapport qui existe entre cet état et le 'white fleshy corpusculum in motion.'

Kyste pédieux.—J'ai montré que le Scyphistome forme pendant la belle saison au dessous de son disque pédieux, des kystes chitineux auxquels certaines figures des planches de Dalyell semblent se rapporter. Ces kystes contiennent des pyramides vitellines et un œuf parténogénétique qui est capable de garder une vie latente pendant de longues années.

Fixation au sol.—Le Scyphistome adhère à son substratum d'une façon remarquable : s'il est fixé sur une roche nue, aucun courant quelque violent soit-il est incapable de l'en détacher. Dalyell, sans rien affirmer, avait estimé que "probably adhesion is spontaneous as with the polypus of the fresh waters and the actinia of the seas." J'ai montré que le premier acte de la fixation consiste à revêtir le sol d'un enduit chitineux continu qui en moule toutes les aspérités en y adhérant intimement. Puis l'ectoderme du disque se résorbe en formant des tonofibrilles soudées par leur extrémité distale à la lame chitineuse et par leur extrémité proximale à la mésoglée. De telle sorte,

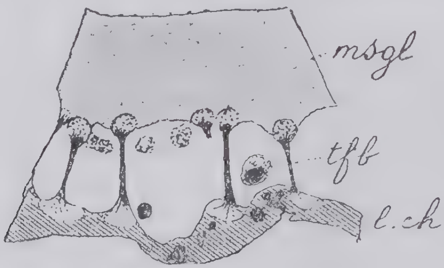


FIG. 2.

que le polype stationnaire ne repose sur le sol que par le bord libre du cylindre ectodermique qui entoure la colonne, tandis que le sac mésogléen est nu dans le champ du disque pédieux et n'est en rapport avec le sol que par ses tonofibrilles dressées comme des échasses et le polype ne peut se détacher qu'en brisant ces échasses.

Elevage.—Dalyell entretenait ses cultures par un fréquent renouvellement de l'eau qui les contenait, mais il suffit de disposer au fond du vase une couche de Mélobésies et de ne laisser pénétrer qu'une lumière tamisée pour obtenir le même résultat. C'est là un procédé qu'on ne peut trop recommander pour maints élevages, aux laboratoires d'enseignement qui sont éloignés de la mer. Depuis quinze ans j'obtiens ainsi chaque année des Ephyra, nées à la Sorbonne et provenant de lignées Parisiennes.

Ces faits joints à ceux que Sars et Dalyell nous avaient déjà fait connaître sur le Scyphistome (*Hydra tuba*) montrent dans le cycle évolutif des Acraspèdes l'exemple le plus complet, qui se puisse rencontrer, des différents modes de reproduction chez une même espèce.

En terminant, je tiens à remercier la British Association de l'honneur qu'elle m'a fait en m'invitant à assister à ses réunions et de m'avoir ainsi permis d'évoquer le souvenir du grand naturaliste écossais Sir J. G. Dalyell dans sa ville d'Edinburgh.

At appropriate times during the Meeting exhibits illustrating several of the papers were set out in the Zoological Laboratory, and there were also exhibitions of the following items:—

(*a*) *Growths and Transformations of Echinospira, a Molluscan Larva with Two Shells*, by Prof. W. GARSTANG.

(*b*) *Multipolar Spindles in the Oocytes of a Guinea-pig*, by Mr. G. LESLIE PURSER.

(*c*) *A New Genus and several New Species of Polychæte Worms*, by Prof. W. C. MCINTOSH, F.R.S.

(*d*) *Living specimens of* Pedalion mirum, *from the neighbourhood of Cardiff*, by Prof. D'Arcy Thompson, C.B., F.R.S., on behalf of Mr. A. E. Harris, Cardiff.

(*e*) Miss Nelson exhibited at her house living sea-anemones, some of which had been living in aquaria for fifty-nine years.

SECTION E.—GEOGRAPHY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 465.)

Thursday, September 8.

1. Lieut.-Col. E. F. W. Lees, D.S.O.—*Aeronautical Maps.*

The provisions of the International Aeronautical Convention of October 13, 1919, with special reference to the new system of co-ordinate reckoning, and the adoption of Mercator's Projection for the general series—index scheme for general series—British proposals for design of maps—non-acceptance of certain of these proposals by the French and Belgians—the design finally agreed upon—conventional signs—abbreviations for the characteristics of aerial and marine lights.

2. Presidential Address by Dr. D. G. Hogarth, C.M.G., on *Applied Geography.* See p. 86.

3. Miss A. M. B. Gillett.—*Historical Geography of the Black Earth Region of Central Russia.*

Physical aspects : space relations : physical features : distribution of black earth : historical aspects of the vegetation—historical geography : prehistoric period : period of great migrations to ninth century : period of Kievan Rus : Mongol-Tartar invasions (1200-1480) : period of the growth of the Muscovite State—Black Earth region as a base for the organisation of the great Russian power : the steppe advance : rise of the Cossacks.

4. Captain L. V. S. Blacker.—*Fresh Ground in Turkistan and Khorasan*, 1918-1920.

Over the Pamirs—Turkistan in 1918 : Cossacks and Bolsheviks—a dash from the Chinese Pamir to Yarkand—a winter journey southward over the Pamir—from Baluchistan to Merv—a visit to the forbidden fortress of Kelat-i-Nadiri—unexplored country north-east of the Kara Dagh and how it was mapped—nine months in the Kurd country of the south-east Caspian region.

Friday, September 9.

5. Joint Discussion with Section H on *The Origin of the Scottish People.* See p. 439.

6. Joint Discussion with Section L on *The Teaching of Geography.*

Mr. G. G. Chisholm.—Frequency of the discussion of the nature of the subject of geography—causes of this—agreement with Sir Halford Mackinder in regarding geography as 'essentially a mode of thought'—importance of arriving at a clear agreement as to what that mode of thought is—signs of approximation to such an agreement in the work now done under the head of geography—much might be done towards bringing about such agreement by the preparation of a physical geography in which the main stress should be laid on physical agencies, not as changing the face of nature, but as influencing human activities.

In the afternoon Messrs. John Bartholomew & Sons, Ltd., received a party at the Edinburgh Geographical Institute, Duncan Street, and Messrs. W. & A. K. Johnston, Ltd., received a party at their cartographical works in Easter Road.

Monday, September 12.

7. Mr. C. B. Fawcett.—*Note on the recent Census Reports in relation to the large Towns of Great Britain.*

8. Discussion on *The Geography of Edinburgh and District: Past, Present, and Future Outlook.* Opened by Mr. F. C. Mears.

9. *Primitive Maps of Britain and Early Plans of Edinburgh.* Demonstration and explanation by Mr. H. R. G. Inglis and Mr. W. Cowan of the collection lent by the Royal Scottish Geographical Society.

In the afternoon an excursion took place to Leith Docks, by invitation of the Dock Commissioners.

Tuesday, September 13.

10. Dr. Marion I. Newbigin.—*The Mediterranean City-State in Dalmatia.*

The conditions determining the origin and growth of Mediterranean city-states in general—the influence of the trade with the East—the early Dalmatian cities : their origin, growth, and destruction at the Slav invasion—the rise of the mediæval cities : their relations to the hinterland and to Venice—the conflict between Venice and Hungary in relation to the Dalmatian cities—the coming of the Turk and its influence on Dalmatia—Ragusa in Turkish times—the modern problem.

11. Lieut.-Col. H. S. Winterbotham, C.M.G., D.S.O.—*The Present Position of the International* 1 : 1,000,000 *Map.*

Historical outline—conventional signs and layer tints—the position at the outbreak of the Great War—the extension of mapping on the scale of 1 : 1,000,000 and 1 : 2,000,000 due to the War : the War Office Provisional Series, 1 : 1,000,000 : the Anglo-French 1 : 2,000,000 series of Africa : maps for the Peace Conference : international aeronautical maps—the present position, illustrated by slides of several maps—other maps on the same scale.

12. Miss R. M. Fleming.—*Geographic Aspects of Tradition.*

The influence of general geographic environment on traditional tales—trade routes and old tales—geographic distribution of certain traditional themes.

13. Mr. H. M. Spink.—*Distribution of Commercial Timber on the Pacific Coast of North America.*

Dependence upon climatic factors and topography : topographic divisions—distribution of the forest types : economic importance of the distribution—economic questions arising out of the distribution : migration of the lumber industry : effects of migration upon lumber costs : competition for markets of the middle west : the growing importance of the Pacific forest in these markets and overseas—'life' of the forest area.

14. Mr. G. W. Grabham.—*A Journey from Kake Tana to Roseires.*

The Royal Scottish Geographical Society arranged an exhibition of geographical books, wall maps, atlases, globes, &c., at the rooms of the Society (Synod Hall, Castle Terrace). The exhibition was open daily to members of the Association.

A unique collection of early maps of Scotland and Britain, together with an extensive collection of early plans of Edinburgh, lent by the Royal Scottish Geographical Society, was on view daily throughout the meeting in the rooms of Section E.

SECTION F.—ECONOMIC SCIENCE & STATISTICS.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 465.)

Thursday, September 8.

1. Miss G. Jebb.—*Cost-of-Living Sliding Scales.*

What is the effect on distribution of the automatic adjustment of wages to changes in the cost of living?

It is argued that this depends mainly on the cause of the change in price level.

When a rise in prices is due to currency or credit expansion, or a fall in prices is due to currency or credit contraction, the cost-of-living sliding scale tends to prevent arbitrary changes in the distribution of real income.

When, however, price changes are the result of changes in the volume of production the effect is reversed.

If prices are rising because goods are diminishing the purchasing power of wages can only be maintained at the expense of other incomes.

Conversely, if prices are falling because goods are increasing the automatic reduction of wages to keep pace with prices lessens the relative share of the wage-earner in the goods income.

2. Joint Meeting with Sections J and L. Discussion on *Vocational Training and Tests.* See p. 455.

3. Mr. A. A. Mitchell.—*The Breakdown of the Minimum Wage.*

In all the recent wages controversies, it seems to have been assumed as an axiom that wages must conform to a predetermined standard of living. This conception seems to find little or no place in economic textbooks. On the other hand, there seems to have been little or no audible protest on the part of economists. According to the subsistence theory of wages, it is impossible for general wages to fall below what is required to keep not each individual workman but the supply of workmen in existence. That purports to be a true economic law, a corollary from the laws of supply and demand, population and diminishing returns. A wage based on an arbitrary standard of life beyond actual subsistence is not based on economic law, though of course it may be made a matter of legal or moral obligation. Minimum wage is inconsistent with the nature of wage which is a payment for a service. No one is compelled to employ at all, and no one can be compelled to employ at a loss. Even the rulers of a socialist state could give their workmen no more than an equal share of the total national product, which might very well be less than the desired standard. We are coming near, or perhaps have reached, the point where the entire wealth of the country is insufficient to pay the wages that are demanded. A wage based on standard of living, not on the value or selling price of the product, tends to (*a*) unemployment, (*b*) inefficiency. Reference was made to recent wage controversies.

Friday, September 9.

4. Presidential Address by Mr. W. L. Hichens, on *The Principles by which Wages are Determined.* See p. 95.

5. Prof. A. W. Kirkaldy.—*The Wages System and Possible Developments.*

Theory and practice in wages. How the present system arose; attempts at improvement; bonus systems; profit sharing; co-partnership—advantages and disadvantages. Influence of Trade Unions. Collective bargaining. Time and piece rates. The effects of the flat rate. Speeding up and restriction. Wage rate and labour cost. Scientific management and the employment of supermen; social and industrial effects; consequences of wide application. It is suggested that the British policy should be grading. An early example of grading; the grading authority. Danger of placing responsibility of grading on employers. The ideal authority. Gradual development of Trade Union

functions. Co-operation with Whitley or other Industrial Councils. What is the labour force? How can harmony be obtained amongst all sections? The ideal system—a grading of the whole human effort, whether of brain or hand, required for the work of production, including transport services.

6. Mrs. B. Wootton.—*Self-supporting Industries: an Inquiry into the Principle of Regulating Wages and Provision against Unemployment in Accordance with Industrial Capacity.*

Satisfactory regulation of wages according to what industry will bear is impeded by (1) the absence of any consistent definition of an industry, (2) inequalities in the strength of different industries, in respect of value of per head output, stability, etc. Since weak industries can only bear comparatively low wages, an ethical as well as a commercial element must be introduced into wage determinations. The effort to bear high wages may, however, cause unemployment. Industries that are self-supporting in respect of wages ought, therefore, to assume some responsibility for the unemployment which their wage policy may create. The possibility of this was illustrated by (1) drafts of special schemes under the Unemployment Insurance Act, (2) proposals of the National Transport Workers' Federation and of the Building Industry.

7. Sir Josiah Stamp, K.B.E.—*The Taxable Capacity of a Country.*

In an examination of the conclusions recently arrived at by Professor Oldham that the capacity of Ireland is one thirty-second part of that of the United Kingdom, and that the 'over-taxation' of Ireland amounted to 18½ million pounds in 1919-20, it is shown that the methods approved by the Commission in 1896 can no longer be relied upon (1) because fundamental ideas as to taxpaying ability have considerably developed, (2) because the aggregate capacity of individuals resident in Ireland is now intended, and not merely the productivity of Ireland, and (3) because, through changes in income tax methods and graduation, the technical measure of capacity has itself altered in character. The shrinkage in taxable capacity, as set out by Professor Oldham, is shown to be largely illusory, and his final results unproven. 'Over-taxation' is shown to be a misnomer, and to result from ignoring the capacity of the population below the income tax level, who bear indirect taxes. An index of capacity derived entirely from statistics of direct taxation, but applied to contributions through indirect taxes, is bound to give inconclusive and unsatisfactory results.

Monday, September 12.

8. Mr. A. H. Gibson.—*An International Stable Standard of Value.*

This paper outlined a scheme for the formation of an international bank of issue, the notes, jointly guaranteed by the participating States, being primarily intended for use as a constituent of bank cash reserves, cover for home currency paper issues and settlement for international differences, and the interest charge for use of same being regulated and varied from time to time according to the course of an index-number based on the prices of certain basic commodities. By the issue of such notes, internationally guaranteed, it was contended that by the operation of a variable interest charge for use of same, the general course of commodity prices may be approximately stabilised at an agreed index-number.

9. Prof. J. Shield Nicholson.—*Deflation.*

10. *Report of Sub-Committee on The Currency and the Gold Standard.*

11. Dr. Mary T. Rankin.—*The Element of Compulsory Arbitration in Recent Industrial Legislation.*

The legislation dealt with was the Trade Boards Act, 1918, and the Industrial Courts Act, 1919. These Acts were passed for the purpose of giving effect to certain proposals for the reorganisation of industry contained in the Whitley Reports, and must therefore be considered from this point of view. The Whitley Reports, while professedly aiming at the self-government of industry, advocate the principle that State 'assistance' should vary inversely with the degree of organisation in each industry (cf. second Whitley Report). The Trade Boards Act, 1918, was the method adopted for providing this 'assistance.' The

Act may be applied to any trade or part of a trade merely at the discretion of the Minister of Labour. State regulation may thus supersede and must conflict with the principle of self-government. Similarly, the Industrial Courts Act, like the Whitley Report, contemplates and encourages a vast increase in appeals to Government arbitration in trade disputes. No Act of such a nature can retain a purely voluntary character.

Tuesday, September 13.

12. Prof. D. H. Macgregor.—*Trusts.*

Trusts in relation to large and small businesses. The question of economic evolution. The historical development of trusts, their changes of form, and the problem of their control. The nature of this development in England, Germany, and the U.S.A. Limited or localised control of industry as affected by (*a*) certain given conditions, (*b*) certain conditions which are created by the trusts themselves. The evidence of the British inquiries under the Profiteering Act. Trusts in relation to the 'era of competition.' The criterion of 'fair' competition.

13. Prof. Edwin Cannan.—*The Application of the Theoretical Apparatus of Supply and Demand to Units of Currency.*

Currency being like machinery and houses rather than milk and newspapers, the supply of it is rightly thought of as depending at any moment upon the stock rather than the rate of production : the stock consists of currency outstanding, and cannot include the clearing-house returns for a year or any other period of time, nor the amounts due to customers from banks, nor the debts of other institutions and persons. The demand for currency is not to be thought of as unlimited nor as provided by 'the aggregate of goods,' but as provided by the willingness and ability of individuals and institutions to sacrifice goods and services for the sake of holding such a stock of currency as each thinks convenient or profitable. When rapid changes in supply take place, it is more difficult than usual to decide what is convenient and profitable : and as the mistakes in each direction do not entirely cancel each other, fluctuations of demand ensue which at one time alleviate and at another aggravate the changes in purchasing power caused by the alterations in supply.

14. *Report of Committee on Credit, Currency, etc.* See p. 268.

15. Miss E. F. Stevenson.—*The Economic Theory of Public Expenditure.*

The economic theory of consumption is generally regarded as being based on an individualistic principle of maximum satisfaction. In public expenditure the conception of social well-being is predominant; but the hedonistic principle still applies if it is understood to mean that economic activity is directed towards such an adjustment of costs and utilities as may be expected to achieve the greatest satisfaction possible under existing conditions. This formula is equally applicable to the private consumer, to consumer groups, and to the State. Whenever the consumer ceases to act as an isolated individual the process of balancing cost against utility must be, to some extent, a social process. It is making a false distinction to say that private expenditure is individual and public expenditure social in its nature. Both conceptions—the individual and the social—are implied in the economic theory of consumption.

SECTION G.—ENGINEERING.

(For references to the publication elsewhere of communications entered in the following list of transactions, see pp. 465–6.)

Thursday, September 8.

1. Prof. T. Hudson Beare.—*An Inquiry into the Suitability of Scottish-grown Timber for Aeroplanes and Pit Props.*

A series of experiments was carried out for the Ministry of Munitions in connection with the suitability of Scottish-grown timber for aeroplane work. The samples sent were tested as beams on a 20-inch span, the load being so

arranged that there was no shear stress on this central 20 inches. From the unstressed ends of every beam specimens were cut and turned for compression tests. In the case of the beams a deflectometer was used in order to determine carefully the limit of elasticity. The various results obtained in both sets of tests are given in a series of tables. After the beams were broken, tests were made of the percentage of moisture present, measurements were made of the width of the annual rings and of the nature of the growth of the wood. The tests of the wood for pit-prop purposes were carried out for the Forestry Commission. The props were tested just as received, the bark was left on, and the ends were carefully squared; the load was applied by means of compression blocks with spherical backs. The results obtained were given in a series of comparative tables.

2. Prof. Alex. R. Horne, O.B.E.—*Experiments on the Mechanical Properties of Scots Pine.*

The investigations relate to the cross-breaking strength and the elasticity of Scots Pine, as determined from experiments made upon about 300 test specimens, taken from 18 trees from 6 forests in the north of Scotland. The specimens were of a size such as is used in constructional work. Laws are established giving the relations between breaking strength, elasticity, and dry density. Information has been obtained regarding the percentage of moisture in the trees as cut, and its distribution. The rate of loss of moisture during seasoning has been investigated; and the period of seasoning desirable in practice has been determined. A note was added regarding the 'blueing' of timber.

3. Dr. B. C. Laws.—*Stresses in Ships' Plating Due to Fluid Pressure.*

This paper referred to means used for determining stress values in plates of rectangular form fixed at their boundaries and subject to uniformly distributed loads, and indicated the effect of fluid pressure on the ultimate stress in mild steel plates used in ship construction when considered jointly with the stresses induced in the material on account of local or structural bending of the vessel. Later the paper dealt with the question of stiffened or reinforced plates, and, in the absence of complete experimental investigation, an endeavour was made to trace the effect of the reinforcement in bringing about a redistribution of stress in the material.

4. Dr. E. M. Horsburgh.—*The Fracture of Wire in Steel Ropes.*

Forces in Rope-wires.—Tension approximately constant throughout the length of a rope-wire, when the rope hangs vertically, and supports a load.

Rope Modulus.—The modulus of elasticity of a rope. The load a continuous function of the extension. Shape of the strain-stress rope curve. Possible analytical representation. Rope modulus as a gradient. Its maximum value. Small values.

Efficiency of a Steel Rope.—Its strength compared with the sum of the strengths of the wires. Difficulties which arise.

Tension in a Rope-wire.—The cup and cone fracture. A family of slip surfaces. Combined tension and bending.

Torsion of a Rope-wire.—Susceptibility to torsion tests. Combined tension and torsion.

Spring Effect in Winding Ropes.—Flexibility test.

Some Effects of Wear.—Abrasion. Denting.

Some Age Effects in Rope-wire.—Loss of toughness after prolonged use. Hardness. The representation of fatigue curves.

5. Prof. T. Hudson Beare and Mr. Wm. Gordon.—*The Influence of the Width of the Specimen upon the Results of Tensile Tests of Mild Steel and Rolled Copper.*

The research originated in an attempt to re-establish the critical ratio of width to thickness which Barba has shown to give a maximum elongation, but the scope of the investigation ultimately became much wider. The paper dealt

with the influence of width on the apparent strength and ductility of test-bars cut from $\frac{1}{4}$-inch and $\frac{1}{8}$-inch mild steel plates and from $\frac{1}{8}$-inch copper plate. The range of the ratio $\frac{\text{width}}{\text{thickness}}$ in the thinner plates is approximately from 2 : 1 to 32 : 1.

In the afternoon visits took place to Messrs. Bruce Peebles & Co., Edinburgh (electrical engineering), to Messrs. Ramage & Ferguson, Leith (shipbuilding), and to Leith Docks.

Friday, September 9.

6. Presidential Address by Prof. A. H. Gibson on *Water Power*. See p. 110.

7. Prof. F. G. Baily.—*The Linking up of the Smaller Water-Powers in Scotland.*

8. Prof. F. C. Lea.—*The Utilisation of Tidal Power, with special reference to the Severn Estuary.*

The daily and monthly variation of head in tidal estuaries and the consequent variation in the energy per day available. The difficulty arising in utilising power as developed; the necessity for large storage reservoirs, and the consequent increase in plant necessary and loss of efficiency in the system; the probable power available in the estuary of the Severn; types of turbines and pumps that might be used and the particular turbine for a known output; capacity of the storage reservoir necessary to obtain a given daily energy supply; some points to be considered in preparing the scheme.

In the afternoon a visit took place to the wire mills of Messrs. Brunton, Musselburgh.

Monday, September 12.

9. Squadron-leader A. J. Miley.—*Seaplanes.*

10. Joint Meeting with Section C. Discussion on *The Mid-Scotland Canal.*

In the afternoon a visit took place to the Laboratories of Heriot-Watt College.

Tuesday, September 13.

11. Mr. Sydney B. Donkin.—*Some Notes on the New Electricity Supply Station of the Corporation of Edinburgh for the Supply in Bulk to the City and to the Lothians.*

(1) Short historical sketch of the Edinburgh electricity undertaking. (2) General description of the new power station in process of construction, with special reference to its economic operation, and the efficient use of available fuel. (3) Suggested area of supply. (4) Details of some novel features in the design, such as the means proposed for obtaining condensing water from the sea, the degasification of feed water, scientific boiler-room control, etc.

12. *Report of Committee on Complex Stresses.* See p. 291.

13. Dr. S. P. Smith.—*Large Electric Units.*

Large manufacturing firms in different countries were invited to supply information on recent machines used for the production of alternating- and continuous-current energy. The abundant supply of information deals with prime movers, turbo-, water-wheel, and slow-speed alternators, continuous-current generators, rotary and motor converters and rectifiers. A selection was

made to include as far as possible the most interesting points in recent design—the first half of the lecture dealt with the production of a.c. energy, the second part with c.c. energy.

14. Mr. John Scott-Taggart.—*Two New Negative Resistance Devices for use in Wireless Telegraphy.*

The paper opened with a short account of the uses of negative resistance, the chief being the production of continuous oscillations for use in wireless telegraphy. The author then described two new thermionic valve devices by means of which negative resistance characteristics may be obtained. The first device, the 'Negatron,' has attained commercial importance as a generator of continuous oscillations for use in wireless telegraphy. The principle involved is the redistribution of electrons passing between a cathode and two anodes. By increasing the potential of one anode the electron current is diverted by means of an electrostatic field towards the other anode. The first anode current, therefore, decreases. In the second device two three-electrode valves are used and the circuital arrangments are such that when a positive potential is applied to the anode of one of the valves a negative potential is given to the grid, thus giving negative resistance characteristics.

15. Dr. T. F. Wall.—*The Long-distance Transmission of Electrical Energy Generated by Means of Tidal Power.*

The problem of the generation and transmission of electrical energy obtained from tidal power involves serious difficulties on account of the widely varying speeds of the turbines working under a variable head of water. For example, the proposals of the Ministry of Transport for the Severn scheme comprise the use of special direct-current generators driven by the turbines, the direct current being then converted into alternating current by means of rotary converters and then transformed into high-tension current for transmission. Such a scheme involves a very large number of machines and tends to make the proposal prohibitive from the point of view of capital outlay. The purpose of this paper was to describe a scheme in which the difficulty due to the variable speed of the turbines is obviated, the scheme being based on the characteristic phenomena of quarter-wave transmission.

Wednesday, September 14.

16. Dr. S. F. Barclay.—*Modern High-speed Centrifugal Pumps.*

The fourteenth-century records of the French Academy contain a reference to a primitive form of centrifugal pump, and it is probable that the principle involved is quite an ancient discovery. The foundation of the modern form of turbine pump was laid in 1875, when Osborne Reynolds invented the stationary guide vanes. The present-day development is due mainly to the industrial evolution of the electric motor with its demand for high angular speeds. The efficiency of the centrifugal pump attains a high value only when worked at the full-rated output. The advantages, however, of the compactness, simplicity, and low first cost of pumps of this kind as compared with displacement pumps are so marked that they are often employed for intermittent work with which high efficiency is not possible. Centrifugal pumps are suitable for use under a very wide range of conditions; they are used for lifting water against a head of only a few feet or against a head of several thousand feet, and for small or large volumes. Centrifugal pumps are constructed that require only a fraction of a horse-power to drive them, and at the other extreme there are pumps that require several thousand horse-power.

17. Dr. J. S. Owens.—*Experiments on Air-Lift Pumping.*

This paper described experiments on some of the factors which govern the efficiency of air-lift pumps; it dealt especially with so-called slippage losses arising from relative motion of air-bubbles through the water; with the relation of size of bubbles to velocity of rise; the conditions governing size of bubbles; the effect of diameter of orifice delivering air upon the size of bubbles produced;

and eddy formation. The 'coefficient of friction' of a mixture of air and water flowing through pipes was discussed. An experimental air-lift pump, dealing with acid mine liquors and delivering five hundred to six hundred gallons per minute in two lifts of about a hundred feet each, with a pump efficiency of 60 per cent., was described. The cause of pulsating flow in air-lift pumps was discussed, also the most efficient design of foot-piece for delivering air.

18. Dr. David Ellis.—*Iron Bacteria in Relation to the Incrustation of Pipes.*

The connection between the activities of iron bacteria and the incrustation of pipes is so close in many cases that it is necessary to know the life histories and the physiology of these bacteria. The change that the deposition of iron causes in the appearance of some of the organisms is so great that identities are sometimes difficult to establish. The iron bacteria that are found in water reservoirs and give trouble to the water engineer are the following: *Leptothrix ochracea* (Kützing), *Gallionella ferruginea* (Ehrenberg), *Crenothrix polyspora* (Cohn), *Cladothrix dichotoma* (Cohn), *Spirophyllum ferrugineum* (Ellis). Iron bacteria must have organic matter to maintain life, so that the examination of organic matter in the water in which the pipes are immersed is of cardinal importance in solving troubles brought about by the incrustation of pipes. Trouble in the pipes may take one or more of the following forms: (1) *Slimy streamers.*—These owe their existence to the activities of iron bacteria and other similar organisms. The reproductive cells of bacteria fasten to the walls and are helped by slime from other micro-organisms. Organisms are usually *Gallionella* and *Spirophyllum.* (2) *Tubercular incrustations.*—Tubercles often form in the absence of organisms, but if present they exert an accelerating action on the formation of the tubercles. (3) *Iron incrustations on non-ferruginous surfaces.*—These result from the activities of iron bacteria with possible help from other organisms, as the walls are not corroded. (4) *Spongy disease of iron.*—The iron bacteria play no part. The remedy is to be found by ascertaining in each case where iron bacteria are concerned the amount of organic matter that is present in the water and then taking means to eliminate it by oxidation in some form or other. This can be done by direct oxidation or indirectly by the introduction of nitrifying organisms by a system of filtration. The acidity of the water must be reckoned and the mineral constituents of the water altered so as to be unsuitable for iron bacteria.

19. Prof. C. E. Inglis.—*Two-dimensional Stresses in Rectangular Plates.*

20. Mr. J. D. Watson.—*The Utilisation of Sewage Gas for Power Purposes.*

21. Prof. Timoshenko.—*Impulsive Stresses in Rails and Girders.*

22. Prof. Timoshenko.—*The Vibration of Bridges.*

23. Prof. Henry Briggs.—*Two New Forms of Rescue Apparatus for Use in Mines.*

The communication described two forms of rescue apparatus, namely, the improved Aerophor (liquid air) apparatus and the Briggs (compressed oxygen) apparatus. These appliances have been designed to comply with the more stringent requirements as to breathing apparatus in mines now stipulated by official regulations. The improved Aerophor has been evolved by the chief officers of the Newcastle and Mansfield groups of rescue stations and possesses improvements in the receptacle for holding the charge of liquid air, in the purifier for abstracting carbon dioxide from the air, and in the valves and in the general arrangement and construction of the appliance. The Briggs apparatus is described in full in the Second Report, Mine Rescue Apparatus Research Committee (Department of Scientific and Industrial Research), but since the latter report was written it has undergone further improvement. The

oxygen is in this case contained under pressure in a single cylinder and is supplied to the breathing circuit of the apparatus through a valve gear of special design. The headdress, mouthpiece, flexible tubes, and breathing bags are the same in both apparatus.

In the afternoon a visit to Rosyth took place.

SECTION H.—ANTHROPOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 466.)

Thursday, September 8.

1. Discussion on *An Imperial School of Anthropology*, opened by Sir Richard C. Temple, Bart., C.I.E. Speakers: Sir Everard im Thurn, K.C.M.G., K.B.E., C.B., Sir William Ridgeway, F.B.A.

2. Mr. Alex. O. Curle.—*The Fourth Century Silver Hoard found on Traprain Law.*

Traprain Law lies some 20 miles east of Edinburgh. It is a conspicuous mass nearly half a mile in length, but with an altitude from base to summit of only 350 feet. It has possessed in ancient times great defensive advantages, increased by the erection of a massive earthwork. The *enceinte* is an area of some 30 acres. The surface shows numerous evidences of long occupation. Excavation commenced by the Society of Antiquaries of Scotland in 1914, and, with a break of three years, continued since, has revealed the fact that the hill was under occupation more or less continuously from the Bronze Age period till the commencement of the fifth century. Numerous relics have been found, generally of Celtic character, but including a hoard of fourth-century Roman silver plate weighing over 770 oz. troy and associated with coins of Honorius. This is believed to have been pillaged from Gaul by Saxon pirates.

3. Prof. G. Baldwin Brown.—*The Bearing of Recent Discoveries in the Domain of Palæolithic Art on the Question of the Origin and Early History of Art in Relation to Human Nature.*

The bearing of recent anthropological study on the theory of art has not been systematically explored. The large part that certain forms of art, such as the dance and personal decoration, play in the life of savages is a remarkable phenomenon, and bears on current theories of art in that these activities are of practical use to the individual and the race, and so are in a sense forced upon them. This fact modifies the crude idea of the freedom of artistic activity, the corner stone of current artistic theory. The remote palæolithic cave dwellers were proficient in certain forms of art such as the delineation of animals, and these were of use, in a kind of make-believe, through their supposed magical influence. Hence they too were forced on the people of the time and were not in the conventional sense free activities. At the same time the work was to a great extent truly artistic and preserved to this extent its characteristic of freedom. The explanation of this apparent paradox casts a light on the general question of the place of art in the human economy.

4. Mr. Miles Burkitt.—*A Recent Important Discovery in Upper Palæolithic Cave Art.*

It is a good many years now since the discovery of the first Palæolithic Cave painting. We are fairly sure that these extremely well-drawn engravings and paintings (that are not all of the same age, but fall into four phases corresponding to four successive epochs) were made for magical purposes to obtain a good catch of game for these early hunters, who knew nothing of metal, of agriculture, or of domestic animals. A new find in a Pyrenean cave has disclosed a frieze of engraved animals (comprising horse, bison, lion, reindeer—in all attitudes—bear, rhinoceros, and mammoth) above which occurs a dominating

figure of a man masked, with stag's antler on his head and a tail. Was this only the sorcerer of the cave or does it show a method used for catching stag? (Compare ostrich hunt of Bushmen.) A certain very early Gallic god was always shown masked as a stag, as also a Cretan god of Minoan times. Are we to see any cultural connection between these representations of masking as stags?

5. Mr. Leslie Armstrong.—*Engravings on Flint-Crust from Grime's Graves.*

The finding at Grime's Graves of fragments of flint-crust bearing incised lines and irregular forms, indicated that definite engravings might be discovered if carefully searched for. This was realised in September 1920. An extensive new chipping zone, consisting of three distinct living levels, was located on hitherto undisturbed ground. The uppermost level was of Bronze Age date, the intermediate level a typical Grime's Graves floor, and the lowest, at 2 feet 8 inches to 3 feet below ground level, resting upon and partially embedded in decalcified boulder clay, probably represents the earliest phase of the industry. Careful scrutiny of every piece of crust recovered resulted in the discovery, on the lowest floor, of several inscribed pieces, including two important engravings. One of these represents a stag, or elk, disturbed whilst browsing, with head held erect and raised fore-leg. The second has an animal's head upon it. Both are executed upon the outer crust of floor-flint. Associated with the engravings were flint implements of Mousterian type, bone tools and pottery.

Friday, September 9.

6. Joint Discussion with Section E on *The Origin of the Scottish People.*

Prof. Sir Arthur Keith, F.R.S.—The inhabitants of the Highlands and western parts of Scotland and the inhabitants of the inland parts of Scandinavia are branches of the same racial (Nordic) stock. Seventy years ago Anders Retzius in Sweden and Sir Daniel Wilson in Scotland maintained that the Highland or Celtic Scot and the central Scandinavian showed the same type of head and the same form of body. A comparison of the results of more recent investigations carried on in Scotland by Beddoe, Turner, Tocher, Gray, Reid, Macdonell, and Young with inquiries conducted in Sweden and Norway by Gustav Retzius, Fürst, Arbo, and Bryn makes it certain that Scot and Scandinavian are traceable to a common source. Prof. Carl Fürst of Lund maintains that the inland inhabitants of Scandinavia are the descendants of the people who settled in Norway and Sweden on the retreat of the last ice sheet. All the evidence favours the opinion that the modern Highlander is the lineal descendant of the people who reached Scotland at a corresponding period.

Scandinavian geologists estimate the beginning of the emergence of Scandinavia and of Scotland from ice to a period of about 11000 B.C. The North Sea was then an estuary or bay, open to the north, with a western shore leading up to Scotland, an eastern leading to Scandinavia. On the Danish, as also on the Scottish, coasts are found the shell heaps of the 'harpoon' folk—the earliest inhabitants in the north-western outskirts of Europe in post-glacial times. The culture of this people is to be traced to countries in the south-west of Europe, and although their remains have not been found we may safely infer that they arose from the long and big-headed type of man found in South England and on the Continent at the close of the Ice Age. It is thus maintained that Scot and Scandinavian are descendants of the late palæolithic men of South-West Europe.

The accepted opinion that the late palæolithic races of South Europe had dark hair, eyes, and complexions is probably well founded. Fair hair, light eyes, and clear complexions, which find their fullest expression in the inhabitants of Baltic lands, are best regarded as characters recently evolved. The darker hair and eyes of the modern Scot, as compared with his Scandinavian cousin, may not be due to a later Mediterranean admixture, but to his retaining to a greater degree the complexion of his palæolithic ancestor. The evidence gathered

in all countries round the North Sea points to an increase in stature amounting to 3 or 4 inches since neolithic times—showing that evolution as well as admixture had been at work.

If we suppose that the northward drift of the 'harpoon' people took place at the beginning of the neolithic period—some 6000 or 7000 B.C.—we have to leave about 4,000 or 5,000 years as a blank in the history of the Scottish people, for it is not until the beginning of the second millennium B.C. that we have reliable facts to guide us. At the beginning of this period we find Scotland in free communication with Europe by two portals. Through her eastern coasts she was open to the opposite shorelands of the North Sea and to Central Europe. From John o' Groats to Berwick we find graves of this period containing the remains of a peculiar and round-headed people. Lord Abercromby has traced the designs of their pottery to the upper reaches of the Rhine. In late neolithic times this same race of round-heads was invading the coast lands of Sweden, Norway, Denmark, and England. To this day the effects of the round-head invasion can be traced in the population of the eastern counties of Scotland and of the coast lands of Norway and Sweden.

There seems only one possible explanation of the westward spread of round-heads during the fourth, third, and second millenia B.C. These people must have had not only superior means of offence and defence, but must also have been in possession of an art which gave them an immense superiority over their neighbours. The only art which could give such a power is agriculture—the knowledge of how to make a piece of land carry a score of families which, by its natural produce, could not support a single soul. It is not necessary to say where or when agriculture first came to be practised as an art. If we suppose that, at the time when the harpoon people were drifting northwards, the round-headed natives in lands lying to the east and south of the Caspian Sea had already learned the art of the crofter, then we can explain the western expansion of the round-heads and the distribution of kindred tongues from India to Ireland. It is quite feasible that the Celtic and Teutonic tongues may be modifications of the speech which reached western Europe in the mouths of the round-head neolithic invaders.

Having thus seen the kind of visitors which Scotland was receiving through her eastern or front door in the earlier part of the second millennium B.C., we turn to see what was taking place at her western or back door. At this time, as Prof. T. H. Bryce has shown, the pulse of south-western Europe—of the Mediterranean—was beating on Scotland, along what may be called the Celtic sea-passage—St. George's Channel, the Irish Sea, the western shores, to Orkney and to Norway. Along the shores of this route can be traced at least three consecutive fashions in stone graves of a southern prototype; by this route Ireland and Wales received new settlers from south-western countries of Europe; but did they reach Scotland? Prof. Bryce is of opinion that the people buried in the western megalithic tombs of Scotland represent invaders of the Mediterranean type. They may equally well be considered as the native Nordic people of Scotland; indeed, in such skulls as retain the face there are certain features which suggest a northern origin.

There is no definite evidence of any great invasion of Scotland from the second millennium to the arrival of the Romans. The remains found near Gullane by Dr. Edward Ewart, and probably of the late Celtic period, are of the Nordic type. The Roman invasion left no appreciable mark on the Scottish people. But in the fifth century, when the Romans were gone, both eastern and western doorways became again open and busy with visitors. The Dalriad Scots from the North of Ireland entered by the western portal; they may have brought a tongue which was new to Scotland, but they brought no new physical type, for we have every right to presume that Ireland was originally peopled by the same race as settled in Scotland. From the fifth century onwards, for a period of 500 years, Scotland received at her eastern doorway settlers from the coast lands on the opposite side of the North Sea. They came from lands which, like Scotland, were first settled by the 'harpoon' people. They brought Teutonic dialects to Scotland, other manners, traditions, and arts, but no physical type of manhood which was new to Scotland. The difference between Celt and Saxon is a difference produced in the same race by separation in time and space; the distinction between them is a political, not an anatomical, one.

Who were the Picts? The people of Aberdeenshire were Picts in the ninth century; there is no reason to question that the bulk of the present population of that county are their children. The characters of Aberdeenshire people are well known from the investigations made by Dr. James Tocher and the late Mr. John Gray. An Aberdeenshire man cannot be recognised from another native of Scotland except by his speech. The Picts, Celts, and Saxons of Scotland are all of one breed—the descendants of the pioneer race which settled North-West Europe when the last ice-sheet lifted. There has been only one intrusive element—the round-head of late neolithic introduction.

Prof. Thomas H. Bryce.—We now know that Scotland was inhabited as far back as Azilian times. We have no direct evidence regarding the physical characters of these early inhabitants, but there is a strong presumption that the primitive basis of the population was Nordic in character. Superimposed on this, came, first, in late neolithic times, the men of the chambered cairns, second, the Beaker folk of the Bronze Age. These three elements, blended in different proportions, made up the population of pre-Roman times, since when it has been altered only by the intrusion of similar elements and reassemblage. In S.E. Scotland there are traces of a new element in the Iron Age with a late La Tène culture. It resembles the Gaulish, but the interments are native, not Gaulish. The distribution of the chambered cairns and burials of the Bronze Age indicate a grouping of the elements which, taken in conjunction with the movements of historical times, explains the well-known features of the present-day population.

In the afternoon an excursion took place to Traprain Law, etc. The excavations on Traprain Law were explained by Mr. J. E. Cree. A Picts' house visited on the journey was explained by Mr. D. MacRitchie.

Monday, September 12.

7. Sir William Ridgeway.—*Totemism.*

For more than half a century three theories have greatly influenced anthropological and classical studies—the Sun Myth, the Tree Spirit, and Totemism. The Sun Myth was the product of German philologists, who assumed that the primitive Aryans used a language consisting of abstract roots. Though Littledale and Lyall dealt deadly blows to this theory, for some years past it has again reared its head in alliance with Mannhardt's Tree Spirit and the manifold speculations that arose from J. F. McLennan's famous papers on 'The Worship of Animals and Plants' (1869-70). Practically all writers have held, and hold, that Vegetation spirits and the phenomena embraced under the term Totemism are independent of the belief in the existence of the human soul after the death of the body, and consequently independent of Ancestor-worship. But I had already maintained (1911) that Vegetation spirits and Totemic beliefs are merely Secondary phenomena, depending on the Primary belief of mankind in the continued existence of the soul after the death of the body and of its transmigration. As the authors of the latest theory of the origin of Tragedy laid under contribution Tree spirits and Totemism, in my 'Dramas and Dramatic Dances of Non-European Races' (1915) I had to test the truth or falsity of our respective theories, and a long series of inductions led me to conclude that the sacredness of certain rocks, trees, plants, etc., on investigation turns out to be due to the belief that they are the abode of the spirits of those who were buried under or near them, or were killed at or near them, and that in all regions where Totemism is known it arises from a like belief in the continued existence of spirits and in their transmigration. It must be borne in mind that in many, if not all, regions where Totemism exists the bodies of the dead were, or still are, left to be devoured by wild beasts, whilst in ancient Hyrcania chiefs kept large dogs to be their own tombs. The Burmese hold that the souls of those eaten by tigers or crocodiles inhabit their devourers, whilst the Papuans, etc., have a similar belief, but it is not merely the spirits of those eaten that migrate into animals. The ancient Egyptians, who were certainly Totemists, believed in transmigration, a doctrine also held by the primitive Greeks, who, if not actually Totemists, had beliefs closely resembling Totemism. Some Indonesians venerate the crocodile, considering it

beneficent, and men look forward to becoming crocodiles after death, whilst Sumatrans worship tigers, terming them 'ancestors.' In the Solomon Islands a dying man will tell his relatives the particular animal or tree into which he means to migrate (the fruit of which henceforth his kin may not eat, confirming my view that trees become sacred by the supposed indwelling of a human soul). In Florida a man may elect to become a shark. As both Papuans and Solomon Islanders are Totemists there are good grounds for believing that reverence for certain trees, animals, etc., depends on the primary belief in the immortality of the soul. It can be shown that the same holds good for the Totemists of Australia, West Africa, and North and South America.

8. Mr. J. Whatmough.—*Rehtia, the Venetic Goddess of Healing.*

The functions of Rehtia, who was worshipped at Este (15 miles S.W. of Padua), were similar to those of Juno at Rome as *Lucina, Februa, Fluonia,* etc., *i.e.* she was a goddess of motherhood and childbirth. An important group of offerings made to Rehtia, the so-called 'nails' and 'wedges,' are to be explained as a specialised votive type of hairpin with pendant axe-shaped talismans of a form well known from Early Iron Age deposits. Compare Artemis Orthia at Sparta.

9. Mr. Stanley Casson.—*Recent Excavations in Macedonia.*

These excavations were undertaken in the course of a journey of investigation made under the auspices of a Committee of the Association. An examination was made of parts of the coast area of Thrace and Macedonia, from Enos on the river Maritsa in the east to Kavalla, and of the inland regions near Lake Doiran and Ardjani and between Ostrovo and Vodena. At Chauchitsa an excavation was made on an Iron Age site lying on the main Vardar valley route, some 50 miles north of Salonika. This site proved to be a cemetery, and some fifteen graves were uncovered. Large quantities of bronze ornaments and pottery were found, mainly of Northern types. The pottery, however, shows Hellenic affinities in its later stages, though at present no adequate parallels can be adduced. The bronze ornaments are entirely of Northern and 'Geometric' types. Iron was found, but was not common; it was used chiefly for small knives and pins.

The main problems of this site still await solution.

10. Miss Margaret Murray.—*Recent Excavations in Malta.*

11. Dr. T. Ashby.—*Recent Archæological Discoveries in Italy.*

Our brief survey of recent discoveries in Italy may begin with the city of Rome itself, where we may notice further investigations of the remains of the temple of Jupiter Capitolinus, and the excavation of an interesting subterranean tomb in the Viale Manzoni, with frescoes of the end of the second century A.D. At Ostia, the port of Rome, work is still being actively continued, and a considerable portion of the main street has already been laid bare. Much new light has been thrown on Roman domestic architecture, and in this respect the discoveries which are being made at Pompeii are also of the highest importance.

Among the other discoveries we may notice especially a portion of the ancient city wall of Arezzo, of the end of the fourth century B.C., built of brick. Some fine terracottas were also found; but the richest harvest comes from the temenos of Demeter Malophoros at Selinus, in Sicily, where a large number of votive terracottas have been dug up.

Attention may also be called to the important discoveries which are being made by the French archæological authorities in the Roman cities of Tunisia, notably at Dougga, Sbeitla, and Gigthis.

Tuesday, September 13.

12. Dr. W. H. R. Rivers, F.R.S.—*Melanesian Land-Tenure.*

13. Mr. T. F. McIlwraith.—*Egyptian Influence on African Death-rites.*

In modern Africa preservation of the body has a sporadic distribution in the Congo and West Africa, where it is limited to chiefs. As its discovery in Africa seems improbable, a reasonable supposition is that the chiefs are

descendants of immigrants from a land where preservation was practised. The occasional use of canoe-shaped coffins, and one example of a box with cover carved to represent the deceased, suggests Egyptian influence. The anthropomorphic figure is common in the Congo and West Africa. Representations of the deceased are sometimes set up during funeral ceremonies, and in at least one case the spirit of the dead person is believed to animate the figure. If these customs are due to Egyptian influence their presence in West Africa and their absence in the pastoral regions may be due either to the influence of a centre established by sea-farers on the West Coast, or to a diffusion by land which was wiped out in East and North Africa by the arrival of the pastorals.

14. Rev. J. Roscoe.—*Death Ceremonies as practised among the Tribes of the Lake Region of Uganda.*

The Bagesu are ceremonial cannibals and eat their dead lest the ghost do harm to the young people of the family. Among the pastoral tribes of Ankole royal persons are thought to transmigrate into animals and reptiles, but ordinary persons are buried with ceremony in the dung-heap in the kraal and the ghost is given cows, whose milk is laid before it daily at a shrine in the heir's house. In Bunyoro the king is deified and human sacrifices are offered to him. The ordinary person is buried with ceremonies similar to those in Ankole. In Buganda attention is paid to a higher rank of gods, but these have been proved to have originated in ghosts, and the king is regularly deified. The ordinary man becomes a powerful ghost, and all sorts of things are given to him to gain his favour, but after a time he is reincarnated. The ghost is the only supernatural being the native understands, and it is on the power of the ghost that not only religion but laws and social customs are based.

15. Mr. F. W. H. Migeod.—*Ceremonial and Mystic Avoidance of Contact with the Ground.*

In Africa there are dancers who are completely covered so that no part of the body is visible, and the feet are also covered so that, though the dancing is incommoded, the ground is not touched. Other dancers, when resting before starting again, are only allowed to sit on another person, and when doing so the feet may not touch the ground. Others again are carried on the shoulders of their friends in the intervals. Some fetish women of high repute may never be seen walking, but ride on a special attendant's shoulders. Brides have to be carried by the bridegroom or a friend, and widows similarly after the husband's funeral. In some initiation fetish ceremonies the initiate meets the head of the Order lying in the path, and has to walk on his body. Further, in a certain cattle tribe the newly elected chief has to walk on a man's corpse after cattle have been driven over the man and trampled him to death.

It is unlikely that there is one general underlying reason applicable to all the many ceremonial occasions on which contact is avoided, as the occasions and attendant circumstances are so diverse. The reason seems usually to be unknown to the performers.

16. Mr. M. W. Hilton-Simpson.—*The Modern Use of the 'Water-clock' in Algerian Irrigation.*

The irrigation of the Aurès is carried out by deflecting the meagre streams into a network of 'seggias,' or miniature canals. Before the arrival of the French in Algeria disputes between tribes or villages arising from irrigation questions were frequent. The 'water-clock' is still in use among the Shawia in order peaceably to divide among the owners of gardens in a single village the periods of irrigation from a 'seggia' to which each has become entitled by purchase or bequest. The 'clock' consists of a circular bowl, about six inches in diameter and two and a-quarter inches deep, the bottom of which is perforated with a minute hole. As the waters of a 'seggia' are turned into a garden the bowl is floated in a vessel of water and immediately refloated when it sinks, each owner being entitled to a definite number of 'sinkings' before the water is deflected from his land. The bowls are now often made of zinc, but older specimens are of hammered copper. A possible clue to the origin of the latter is suggested by the Roman 'seggias,' hewn in the solid rock, portions of which are in use to-day in the Aurès.

17. Rev. J. Roscoe.—*The Mackie Ethnological Expedition to the Uganda Protectorate.*

Among the pastoral tribes of Ankole and Bunyoro the milk diet is closely adhered to. The pastoral peoples are entirely different from the negro stock and are evidently the descendants of later immigrants. There is every probability that it may be possible to prove their connection with the Galla people and, through them, with ancient Egypt. Wherever they are found these pastoral tribes are the dominant race, having subdued and enslaved the aborigines. In some districts they have avoided intermarriage, and the two clearly differentiated races are found side by side.

The expedition was also successful in discovering a line of demarcation between the pastoral people of the Lake region and the Somali, Masai, and Nandi tribes, who are of the same origin. The latter perform at puberty certain initiation ceremonies involving circumcision, while the former avoid circumcision and any mutilation of the genital organs of their women.

18. Dr. Nelson Annandale.—*On a Collection of Anthropological Photographs from Calcutta.*

The communication was a demonstration rather than an original paper. The photographs shown on the screen and as prints form part of a large collection recently made in the laboratories of the Zoological Survey of India in the Indian Museum. They have been taken with a fixed camera and represent the whole body, in three positions, of living men belonging to a number of races and combinations of races. The preparation of similar series of photographs in other centres was advocated and the application of the figures obtained was illustrated, in reference particularly to the form of the trunk among the Malays and to head-form in the Armenians.

Wednesday, September 14.

19. Miss R. M. Fleming.—*Sex and Growth Features in Racial Analysis.*

Standards of race distinction need to be modified in the case of women because they tend as a sex to have a higher cephalic index, a deeper degree of pigmentation, and less marked bony development of the skull. The cephalic index varies with growth, and in both sexes there is a marked tendency towards greater growth in width of skull than in length, *i.e.* towards gradual increase of cephalic index. There is, however, a sex difference as to the ages at which decided alterations in skull form, in shape of forehead, and in coloration take place.

20. Mr. Alex. Sutherland.—*The Brock of Cogle, Watten, Caithness.*

Animal remains found in the Brock have been submitted to Prof. T. H. Bryce and identified by him as being bones of ox, sheep, red deer, roe-deer (?), dog, and pig.

21. Canon J. A. MacCulloch.—*The Mingling of Fairy and Witch Beliefs in Sixteenth and Seventeenth Century Scotland.*

Although the fairy and the witchcraft superstitions have, on the whole, separate sources, they had many things in common. Both were also regarded by official and orthodox ecclesiasticism as connected with the devil and the kingdom of darkness. The folk gradually accepted this view, at least with regard to witchcraft. The common aspects of the two beliefs, and the common ban under which both were placed, would inevitably tend to mix them up together. In Scotland, where persecution for witchcraft had seldom occurred before the Reformation period, this mingling of the two beliefs together with that in phantasms of the dead was thorough, and there is clear evidence of it during the sixteenth and seventeenth centuries. The evidence is found (1) in certain poems of the Reformation period; (2) in King James VI.'s *Dæmonologie*; (3) in the records of witch trials. It shows that this curious mingling of fairydom and the kingdom of darkness was not confined to one district, but occurred in the Lowlands, in Perth-, Moray-, and Aberdeenshires, in the

Western Isles, in Orkney and Shetland. Some of the accused witches who claimed to have their powers from elf-land were mere healers or 'spae-wives'; others were accused or accused themselves of the more sinister aspects of sorcery, but they also had dealings with the fairy folk.

22. Mr. Donald A. Mackenzie.—*Features of Scottish Folk-lore.*

Scottish folk-lore has distinctive features. The treatment of the pig is in sharp contrast to the Irish treatment. A Scottish pork taboo, at one time widespread, still persists in localities. It has no connection with totemism. In Ireland pork was eaten freely from the dawn of its history. This Scottish taboo is non-Celtic and non-Teutonic. Another contrast between Ireland and Scotland is presented by the Fomorians. In Scotland these are hill giants who engage constantly in single combats; in Ireland they wage war against the Danann deities. There are no Dananns in Scotland. In Scottish folk-lore are fragments of ancient mythological systems and evidence of culture drifting.

23. Mr. Lewis Spence.—*The Study of Mexican Religion.*

Present position of the study.—Its chief modern protagonists.—The modern literature which deals with it.—Obscurity of its most valuable sources.—Few of these accessible for general study.—The less reliable sources almost habitually employed by writers on folklore.—True sources of our knowledge of the subject.—Difficulties confronting the student.—The cults of Tlaloc, and of Quetzalcoatl, alien cults.—The Obsidian cult.—The elements of Mexican Religion at the period of the Spanish Conquest.—Conclusion.

SECTION I.—PHYSIOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 466.)

Thursday, September 8.

1. Presidential Address by Sir Walter M. Fletcher, K.B.E., F.R.S., on *The Aims and Boundaries of Physiology* (see p. 125); followed by discussion, opened by Prof. Sir E. Sharpey Schafer, F.R.S.

2. Dr. R. K. S. Lim.—*Demonstration of the Mucoid Cells of the Stomach.*

Friday, September 9.

3. Discussion of the *Results of Kidney Deficiency Tests in Relation to the Views of the Functions of the Kidney.* Opened by Prof. A. R. Cushny, F.R.S.

4. Dr. A. Krogh.—*An Apparatus for Recording the Oxygen Consumption of Man and Animals.*

The apparatus is a modification, or rather a simplification, of one used by Krogh and Lindhard. It consists of a recording spirometer, with a CO_2 absorbing system built into it, and connected by inlet and outlet pipes with respiration valves and a suitable mouthpiece, mask, or tracheal cannula, the whole forming a closed system. The respiration of the subject is recorded on a drum revolving at a constant rate, and the CO_2 being absorbed the respiration curve will show a steady decline due to the absorption of oxygen. Oxygen must, of course, be added at the beginning, so that the O_2 percentage in the apparatus does not sink below that of the atmosphere. When the O_2 absorption is uniform a straight line can be drawn joining the tops of all normal expirations, and from this the O_2 consumption can be measured in a few moments with a high degree of precision.

In order to express the results accurately in terms of calories the R.Q. must be known and the protein metabolism must be low. This is easily attained by

regulating the diet of the subject for one or two days before the determination. Control experiments have shown that the R.Q. can be fixed in this way within ± 0·05.

5. Dr. J. C. Drummond.—*Vitamines in Relation to Public Health.*

6. Prof. W. D. Halliburton, F.R.S.—Lecture on *Giants.*

7. Dr. R. J. S. McDowall.—*The Independence of the Pulmonary Circulation as shown by the Action of Pituitary Extracts.*

In a series of over sixty experiments it was shown that pituitary extracts caused a rise of pulmonary pressure—obtained by the method of Sharpey Schafer—which had no relation to any simultaneous rise or fall in the aortic pressure. This independence was further borne out by perfusion experiments of the surviving lung, in which both contraction and dilatation were obtained.

8. Prof. J. B. Haycraft.—*An Electric Light Sphygmograph.*

9. Dr. W. W. Payne and Dr. E. P. Poulton.—*Epigastric Pain.*

Monday, September 12.

10. Joint Discussion with Section B on *Biochemistry.* See p. 417.
Visit to Clinical Laboratory, Royal Infirmary.

11. Prof. J. Meakins.—(*a*) *Effects of Resistance on Breathing* (1) *at rest,* (2) *on work,* (3) *with varying percentage of Oxygen inspired.* (*b*) *Effects of restricted volume of Breathing, showing relief by adding Oxygen.*

12. Dr. E. P. Poulton and Dr. W. W. Payne.—*Demonstration of Peristalsis in the Œsophagus.*

13. Mr. McClure.—*Demonstration of Psychogalvanic Reflex.*

Tuesday, September 13.

14. Discussion on the *Physiology of Heavy Muscular Work,* opened by Prof. E. P. Cathcart, F.R.S. The following papers were taken as part of the discussion:—

Note by Prof. Henry Briggs, *D.Sc., Ph.D., A.R.S.M.*

This communication described the work of a Test Station set up by the War Office under the Scottish Command in 1918 for measuring in a quantitative manner the fitness and stamina of men sent in from various units as cases of doubtful physical capability. The test was based on the respiratory performance of the subject doing physical work of gradually increasing intensity on a Martin's ergometer. The subject was required to work first while breathing air and then while breathing oxygen. Rapid methods of collecting and analysing the expired products were evolved, and results were charted as the test proceeded. The graphs so obtained enabled the percentage fitness and stamina to be estimated.

The physiological principle involved has already been dealt with by the author (*Journ. Physiol.* 1920, p. 292, and elsewhere).

Specimen charts were shown illustrating the results obtained with men of different physical characteristics.

Dr. J. S. Haldane, F.R.S., and Dr. C. G. Douglas.—*Experiments on the Regulation of the Circulation in Man.*

Prof. A. D. Waller, F.R.S., and Miss G. De Decker.—*The Physiological Cost of Muscular Work (with special reference to the cost of Marching).*

Mr. E. Farmer.—*The Economy of Human Effort in Industry.*

15. Dr. F. W. Edridge-Green, C.B.E.—*The Change of Hue caused by the Addition of White Light to Spectral Colours.*

The apparatus used in these experiments was that described in the Proceedings of the Royal Society, B, Vol. 92, page 232 (1921).

Various spectral colours were isolated on a screen coated with magnesium oxide and definite proportions of white light, taken from the source, which was a 1,000-candle power Tantalum Arc, added. The scale of white light is arbitrary; the maximum amount of light it is possible to add being 100 divisions.

A comparison white light taken from the source was used. Each colour became less saturated on adding white light. Red first became orange, then yellow. Orange became yellower, λ 585 $\mu\mu$., pure yellow did not change in hue. Orange-yellow and yellow-green became yellow. Green became yellow-green. Blue λ 480 $\mu\mu$. became white, the comparison white appearing yellow. The violet end of spectrum from λ 480 $\mu\mu$., making a blue on the screen, changed to violet on adding 33 divisions of white light, light purple on adding 100 divisions.

Wave-length 585, the point where the addition of white light produces no change of hue, is also the centre point of pure yellow and the apex of the luminosity curve.

The result of these experiments shows that the component part of white light which has the greatest luminosity effect is the hue to which all colours tend on the addition of white light.

16. Dr. J. H. Shaxby.—*The Testing of Colour Discrimination.*

SECTION J.—PSYCHOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 466.)

Thursday, September 8.

1. Joint Meeting with Sections F and L. Discussion on *Vocational Training and Tests.* See p. 455.

2. Dr. H. S. Langfeld.—*The Study of Personality.*

Laboratory experiments, whether positive or negative in the principal results, usually yield individual differences, which in themselves may have considerable value both for the solution of theoretical problems and for applied psychology. An attempt is being made at present to correlate the individual differences which occur in the various researches of the psychological laboratory with the results of vocational and so-called intelligence tests. It is hoped in this way to discover the fundamental characteristics which constitute personality and to devise methods of accurately measuring such characteristics. The results so far obtained give promise of securing a 'profile' of the intellectual and emotional life of the individual. The problem involves a general consideration of experimental methods.

3. Miss M. McFarlane.—*Sex Differences in Tests of Constructive Ability.*

An investigation into the nature of 'Practical Ability' showed that in the construction test used, viz., fitting together a sectional wheelbarrow, boys scored much better than girls.

A further series of tests was planned to determine whether this superiority can be accounted for by:

(*a*) The relation of the object to be made to existing interests.
(*b*) Familiarity with this particular type of material.

Friday, September 9.

4. Presidential Address by Prof. C. Lloyd Morgan, F.R.S., on *Consciousness and the Unconscious*. See p. 143.

5. Dr. C. S. Myers, F.R.S.—*The Evolution of Feeling.*

(1) Four varieties of affective tone are distinguishable, characterised by (*a*) strain and (*b*) relaxation in response to a favourable situation, and by (*c*) strain and (*d*) relaxation in response to one unfavourable. Exhilaration, gladness and interest arise from (*a*); ease, bliss and contentment from (*b*); uneasiness, distress and repugnance from (*c*); depression, sadness and apathy from (*d*).

(2) Affective tone is due to (i) the organic harmony or discord induced by the environment; this evokes (ii) innately purposive patterns of out-going locomotor and organic activity, partly self-controlled and producing organic sensations; the latter in turn induce (iii) organic harmony or discord. Self-activity is 'affected' by (i), (ii), and (iii). An innate basis is afforded by (i) and (ii) for affective tone which is completely developed by (iii) derived from actual expression.

(3) Instincts are integrated from different higher and lower reflexes, emotions from different instincts, sentiments from different emotions, organised within higher systems and subjected to control and inhibition which are important determinants of the accompanying feeling. In the lowest reflexes the self is affected only by (iii). The higher reflexes are accompanied by affective tone evoked as described above (in 2). Instincts, emotions and sentiments are accompanied by their special feelings depending on the integration of dispositions to lower feelings and on (ii) and (iii).

6. Dr. W. Brown.—*Psychoanalysis and Suggestion.*

From a theoretical, no less than from a practical, point of view, it is most important to determine the exact relation of suggestion to psychoanalysis. Freud himself has admitted that the factor of *transference* (Uebertragung) is essential in any course of psychoanalysis which is to lead to cure or amelioration of the patient's condition. He also says that suggestion is a form of transference. It is proposed to take this position as the starting-point for discussion. Suggestion and hypnosis. Auto-suggestion. 'Unconscious' suggestion. Laws of suggestion. Practical use of suggestion in psychotherapy. Suggestion and psychoanalysis in so-called conversion hysteria. Criticism of this term. Criticism of certain psychoanalytical doctrines in relation to the subject of suggestion. Conclusions.

7. Dr. R. G. Gordon.—*Some Suggestions as to a Common Ground between Freudian and Behaviourist Psychology.*

8. Prof. T. H. Pear.—*A Neglected Aspect of Forgetting.*

9. Dr. Ernest Jones.—*The Psychology of the Herd Instinct.*

10. Dr. J. Drever.—*Appetition and Reaction.*

Freud's distinction between the Pleasure Principle and the Reality Principle appears to be psychologically to all intents and purposes identical with the distinction the writer has drawn between Appetitive and Reactive Tendencies. An earlier psychologist had found the fundamental psychical phenomena in Belief and Desire. This is apparently the same distinction. Freud's distinction is so radical for his psychological theory—it is not negligible in the practice based on that theory—that it is imperative for the psychologist, whether Freudian or non-Freudian, to come to some definite conclusion concerning the validity of the distinction, or at least to examine carefully the phenomena on which the distinction is based, and that in the interest of the science of psychology itself.

In the case of adult human behaviour the facts seem clearly to support the view that some such distinction is valid. Action may be initiated and determined by the agreeable or disagreeable in experience. Agreeable experiences may be sought, disagreeable shunned, with reference to no end beyond the

affective. This is appetition. On the other hand, action may be determined with reference to an external object or end independently of any immediate agreeableness or disagreeableness in the experience which the action involves. This is reaction. Further, it seems to be agreed among psychologists that there are tendencies, both native and acquired, which are characterically appetitive, in the sense in which we are understanding appetition. Popular thought and popular speech indicate the same recognition.

The first important question which suggests itself is: Are the two types of behaviour primitive? Three answers are possible. It may be maintained that appetition only is primitive, and reaction secondary. That is apparently Freud's contention. It is a contention which leads to many difficulties, and which it is almost impossible to adhere to. Or we may take the view that reaction is primitive, appetition secondary. If one had to choose between these two answers the writer would at any rate prefer this, which can easily be supported from the general biological, if not from the psychological, point of view. The safest view for the psychologist to take is probably that both are equally primitive.

A second important question is: What is the relation between the two biologically and psychologically? Again, Freud has given an answer which is not easily justified on theoretical grounds, but is nevertheless of some practical value and significance. The general answer to this question would appear to follow from the fact that the fundamental psychological function of pleasure and unpleasure in the normal case is not to initiate action, but to guide and regulate, in spite of the apparent contradiction presented by the natural appetites. The possibility of progress and development in the individual seems to be bound up with this function. Or rather the existence of reactive as opposed to appetitive tendencies seems to be a condition essential for human progress and development. In the case of the reactive tendencies pleasure and unpleasure function normally.

Monday, September 12.

11. Joint Meeting with Section D. Discussion on *Instinctive Behaviour*. See p. 423.

12. Mr. F. B. Kirkman.—*The Psychological Difficulties of a Naturalist.*

13. Dr. W. H. R. Rivers, F.R.S.—*The Instinct of Acquisition.*

14. Mr. J. C. Flugel.—*Social Progress and Psychological Understanding.*

Tuesday, September 13.

15. Dr. C. W. Kimmins.—*An Investigation of the Sense of Humour in School Children.*

From the analyses of a large number of funny stories and jokes recorded by children of different ages much interesting information has been obtained with regard to the varying nature of the elements most provocative of laughter and amusement from year to year, and the differences in this respect between boys and girls. The greatest changes are associated with periods of rapid growth.

Illustrations of the types of funny story and humorous incident of universal appeal. The long life of a good joke and the reasons for its survival. Theories of laughter.

The consideration of—(1) the element of surprise; (2) the feeling of superiority; (3) the misfortunes of others; (4) the play upon words; (5) riddles; and (6) boisterous fun, as factors in humorous situations.

The child's library of funny books. The important place taken by the fairy story.

16. Miss E. L. G. Ross.—*The Estimation of Vocational Fitness among Mental Defectives.*

In view of the increasing importance being attached to the provision of industrial colonies for the permanent care of the feeble minded, an investigation was undertaken to estimate the reliability of various forms of tests which seemed to measure abilities making for efficiency in those occupations usually provided for defectives. About fifty institution cases of various ages and grades were taken as subjects and their standing in the various tests compared with their known industrial ability and learning capacity. Those temperamental factors which go towards social efficiency were studied on the basis of the Porteus Social Ratings Scale.

17. Mr. F. Watts.—*The Present Condition of Industrial Psychology.*

18. Dr. A. R. Abelson.—*A Plea for the Psychological Treatment of the Delinquent Child.*

There is an accumulating amount of reliable evidence that wrongdoing is largely bound up with morbid conditions of health. An important percentage of delinquents are mentally deficient. Insanity and epilepsy are important factors. It is found that a large number of delinquent children possess a 'neurotic' constitution and psychological measures have succeeded in effecting considerable improvement where all other methods of treatment had more or less completely failed. One must also look for physiological irregularities, *e.g.* adenoids, tonsils, endocrinal insufficiency, but even in these cases psychological treatment should be used in conjunction with the other remedial measures.

By far the best method of treatment is by psychological analysis.

19. Miss E. M. Bickersteth.—*Coloured Thinking.*

20. Mr. J. G. Taylor.—*The Use of 'Retinal Rivalry' for a Test of Colour Fatigue.*

The experiments described in this paper were carried out in connection with the study of colour-fatigue in industries such as paper-making, where accurate discrimination of colour-tints and shades may be required. It was known that prolonged stimulation of one retina or any colour affected retinal rivalry by reducing the proportion of that colour to its 'rival' in the alternating fields. Preliminary experiments showed that the proportion of red to blue after stimulation of one retina for three minutes with red varied in different subjects from .76 to .25 of the 'normal' proportion.

In later experiments records of retinal rivalry were taken for thirty periods of one minute, separated by intervals of one minute. The colours used were red and blue, and the records were made by closing a key when red was dominant; except in a series of control experiments, when blue took the place of red in this respect. In the study of the records each half-minute was measured as well as each minute.

The main argument of this paper was that a decrease in the rate of fluctuation may be taken as a measure of fatigue. The rate of fluctuation decreases from the first half of a minute to the second half, from the beginning to the end of an hour, from forenoon to afternoon, and sometimes from day to day. Introspection shows that slower fluctuation is accompanied subjectively by a feeling of fatigue and also by a decrease in the apparent purity of the colours.

While change in the rate of fluctuation seems to be due mainly to fatigue, the total time per minute during which each colour is dominant seems to vary, partly with attention, partly with fatigue. Thus while the rate of fluctuation usually falls steadily towards the end of each hour the total amount of red per minute rises and falls over periods varying from twenty-five minutes to forty minutes—which may be referred perhaps to the fluctuation of attention. In the control experiments the total amount of red per minute was much lower

than in the previous experiments, and was subject to wide fluctuations until the adaptation of attention, acquired in the previous experiments, was overcome.

Illustrations were given of records, graphs, &c.

21. Demonstration by Mr. D. KENNEDY FRASER of Psychological Tests (Pedagogical Laboratory, The Training College).

SECTION K.—BOTANY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see pp. 466–7.)

Thursday, September 8.

1. Mr. J. SUTHERLAND.—*Forestry in National and Economic Aspects.*

State forests now being created, but encouragement should also be offered to private enterprise. It is desirable that the total proportion of land to be devoted to forestry should be considered. Forestry will : (1) produce a national asset ; (2) keep capital in country ; (3) develop subsidiary industries ; (4) increase the rural population ; (5) create an additional source of revenue ; (6) improve our security in war ; (7) improve climatic and agricultural conditions and national health.

2. Dr. BORTHWICK.—*Forest Protection.*

(1) By selection of home-grown seed and of favourable locations and the control of imported seed.

(2) By prevention of damage by : (A) Man and inorganic agents. The cure is education and training of forester, landowner, and public.

(B) Organic agents—fungi and insects. Immediate attention required for cleaning of débris on ground. Extension of research on origin and spread of disease. Passing of forest laws.

3. Joint Meeting with Section D. Discussion on *Forest Insect Problems.* The following papers were taken:

(*a*) Prof. A. HENRY.—*Forest Protection.*

Attacks of fungi, insects, and rabbits on woodlands, plantations, and nurseries. Suggested measures of protection, with some account of methods hitherto adopted in other countries.

(*b*) Dr. M. WILSON.—*The Phomopsis Disease of Douglas Fir and Larch.*

(*c*) Dr. W. RITCHIE.—*The Larvæ of the Genus Rhagium F. and their Economic Importance.*

A genus of Longhorn beetles whose larvæ are wood borers. Three species in genus. Distribution in Britain. Description of larva of *R. bifasciatum* F. The distinguishing characters of the other two species. Habits of the three species of larvæ. Economic importance of the genus.

(*d*) Mr. W. J. MUNRO.—*Some Forest Insect Problems.*

(1) Position of forest entomology. (2) Some important injurious groups of insects. (3) General distribution of pests in Britain. (4) Means of dispersal (specific instances). (*a*) Natural. Flight, wind. (*b*) Caused by Man. Dispersal in seed—in nursery stock—in timber. (5) Remedial and control measures. (*a*) Silvicultural ; (*b*) biological ; (*c*) mechanical ; (*d*) legislative.

4. Prof. E. P. STEBBING.—*Indian Forestry.*

Indian forests exploited up to 1850. In 1850 a committee of the British Association was formed at the instigation of Dr. Cleghorn with the following

reference : 'To consider the probable effects from an economic and physical point of view of the destruction of tropical forests in India.' In collaboration with Dr. Cleghorn the lines of a forestry administration were laid down and the nucleus of a Forest Department was established as a definite branch of the State service. The result of the efficient management thus introduced has been : (1) Plentiful supply of forest products with the protective co-operation of the people; (2) a considerable annual revenue.

5. Prof. D. and Mrs. Thoday.—*Lantern Demonstration of some Aspects of South African Bush, especially in relation to Moisture Conditions.*

6. Dr. Kidston, F.R.S., and Prof. W. H. Lang, F.R.S.—*Exhibition of Rhynie Material* (in the Botanical Laboratory of the Royal Botanic Gardens).

In the evening a visit took place to Arthur's Seat and Duddingston.

Friday, September 9.

7. Presidential Address by Dr. D. H. Scott, F.R.S., on *The Present Position of the Theory of Descent in Relation to the Early History of Plants.* See p. 170.

Followed by joint discussion with Section C, with special reference to the Rhynie fossil plants. (See p. 419.)

In the afternoon an excursion to Gullane took place.

Saturday, September 10.

An excursion to Nurthley and Dunkeld took place.

Monday, September 12.

8. Mr. Matthews.—*The Distribution of Certain Elements of the British Flora.*

9. Prof. McLean Thompson.—*The Bearing of the Floral Morphology of the Cannon Ball Tree (Couropita) on the Floral Morphology of the Myrtales.*

The development of the remarkable lopsidedness of the flower was shown to be correlated with cellular gigantism. Comparison was made with the Lecythidaceæ.

10. Mr. H. H. Thomas.—*On a New Group of Angiospermous Fruits from the Middle Jurassic of Yorkshire.*

The specimens fall into two genera, *Caytonia* and *Gristhorpia*, of which most study has been given to *Caytonia*. They consist of small bunches of stalked fruits which are inverted and show traces of what may be a stigma. Special microtome methods have been applied to the material. Each fruit contains about eight small seeds, covered with a double fibrous integument. They seem to constitute a group of Angiosperms of which there are no living representatives, and they may perhaps be regarded as forming a link between Pteridosperms and flowering plants.

11. Miss L. Batten.—*Organs of Attachment in Polysiphonia.*

The form of the attachment organ varies with the species and is influenced by the nature of the substratum. Four types may be distinguished :—

1. Rhizoids formed from longitudinal proliferation of siphons.
2. Elementary aggregations to form a disc.
3. Largely formed of stunted lateral branches.
4. Corticate forms which show a pseudo-tissue in disc.

12. Miss E. R. Saunders.—*On Some Anatomical and Physiological Relations in the Dicotyledon Shoot.*

The appearance of certain superficial anatomical characters exhibited by the seedlings and developed shoots of many Dicotyledons leads to the following conclusions : (1) that the Dicotyledon hypocotyl must be regarded as a compound structure consisting of an axial core clothed in a cotyledon skin; (2) that the epicotyl shoot is built up in the same manner, the vegetative axis being covered with the extended bases of the foliage leaves, the flower stalk with those of bracts or sepals. The view taken of the construction of the Dicotyledon shoot differs from the earlier conceptions of Hofmeister and Nägeli and from the Pericaulom theory recently brought forward by Potonié in two respects. It is based neither upon evolutionary theory nor on palæontological argument, but is deduced from patent characters in the living plant. It is put forward only in regard to the most superficial layer (or layers) of the stem, since only here do we appear to have clear evidence of the existence of boundaries.

13. Major C. C. Hurst.—*Origin of the Moss Rose.*

Only differs from the old Cabbage Rose which has been cultivated in Europe more than 2,000 years in multiple branching of its gland structures; nevertheless twelve authors gave it a specific rank. The sterility of the old Cabbage and old Moss Rose makes genetic investigation difficult, but bud variation of Moss and Cabbage Rose seems to throw light on their constitution, and if so gives a new value to bud mutation.

14. Prof. R. C. McLean.—*On the Behaviour of the Somatic Nucleus in Development.*

Discovery of the binucleate phase in the developing soma cell. Ontogenetic origin of the condition. Evidence for subsequent reduction by fusion. Occurrence of amitosis? Bearing of this process on somatic senescence, normal histogenesis, and somatic segregation of characters.

15. Prof. R. H. Yapp and Miss Una C. Slane.—*The Water Content of Developing Leaves.*

16. Prof. R. H. Yapp and Miss Nora Boycott.—*On Cell Form and Size in Vicia Faba under Varying Conditions of Illumination.*

17. Prof. F. E. Weiss.—Popular Lecture on *Graft Hybrids.*

Tuesday, September 13.

18. Joint Meeting with Sections A, C, and D. Discussion on *The Age of the Earth.* See p. 413.

19. Discussion on *Size and Form.* Prof. F. O. Bower, F.R.S., Prof. J. H. Priestley, Dr. D. H. Scott, F.R.S.

20. Dr. J. P. Lotsy.—*Factors of Evolution.*

To trace the course of evolution by the genealogy of species is erroneous. The species concept remains an abstraction; Nature consists of individuals; similar individuals form syngameons, and these have been mistaken for species. As the gamete takes precedence over the individual, the course of evolution should be traced by the genealogy of the gametes. The fundamental question of evolution is : can a gamete vary by itself without loss of chromosomes, and are such variants transmissible? The only transmissible changes proved to occur are the results of crossing. These changes transgress the limits of Linnæan species. The inheritance of cytoplasmic characters occurs in the female line only in the higher organisms, and is therefore not affected by crossing. Crosses between gametes having a differing number of chromosomes : triploids in *Œnothera* and in the author's own experiments. The hybrids of *Papaver spp.* and of microspecies of *Erophila.*

Wednesday, September 14.

21. Discussion on *The Quantitative Analysis of Plant Growth.*

(*a*) Dr. W. L. BALLS.—*General Introduction; the Average Plant Predetermination, etc.*

(*b*) Mr. G. E. BRIGGS, Dr. F. KIDD, and Dr. C. WEST.—*A Quantitative Study of the Growth of Helianthus annuus.*

(*c*) Prof. J. H. PRIESTLEY and Miss FREDA EVERSHED.—*A Quantitative Study of the Growth of Roots.*

The results of the quantitative study of the weight of roots produced upon various cuttings were reported. They appear to offer an interpretation of Sachs' grand period of growth which was based on measurement of length. Correlation is shown between rate of increase in weight and production of branch roots.

22. Prof. FRITSCH.—*The Moisture Relations of Terrestrial Algæ.*

SECTION L.—EDUCATION.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 467.)

Thursday, September 8.

1. Mr. J. DON and Mr. JAS. GRIGOR.—*The Preference of Pupils in Subjects of Study.*

The materials for this investigation were collected in 1919, and relate to pupils at the Intermediate stage in the Higher Grade Schools of the West of Scotland. In all, tests were made in 93 schools, comprising 1,858 boys and 1,762 girls. The method adopted was to ask the pupils to write down the five subjects, English, Mathematics, French, Drawing, and Science, and thereafter to place the number 1 after the subject they liked best, the number 2 after the second best, and so on. Taking the combined vote of both sexes, it is found that the order of preference is English, Science, Drawing, Mathematics, French, but boys and girls show some differences in their preferences. The former have a decided aversion from French, for 60 per cent. of them place it fourth or fifth. On the other hand, they rate Science slightly higher than English, and Drawing stands third. Girls put both Science and Mathematics at the end of their choice, Science being actually lowest. Very wide divergences are found in the votes for first place, English, for example, receiving 1,077 marks and French only 547. Similarly, French is put lowest by 1,208 pupils and English by 273. For the second place English, Science, and Drawing are close rivals, while Mathematics, Science, and Drawing compete with almost level scores for the fourth position. In fact, it would seem that after having made their first and last selections pupils are less decided in their expression of preferences, but girls are more discriminating than boys. Whatever subject is put first, English in general is never far behind, but, except where English comes first, French comes out badly. With Mathematics first, Science takes second place, and those who put Science foremost choose English for the second position. The data collected afford some insight into the question of affinity between subjects of study. Postulating that if two subjects are placed contiguously, say first and second, second and third, and so on, there is in the mind of the pupil an affinity of liking or indifference, or dislike, it is found that the couple English-French comes out highest and Mathematics-Science next in order. Drawing goes fairly well with English or Science or Mathematics. Boys with a taste for Mathematics or Science have very little liking for French.

To test the correctness of these deductions a further assumption was made—namely, that subjects placed by the pupils far apart, as first and fifth, first and fourth, or second and fifth, have no mutual affinity, and that children dislike studying them at the same time. The results so obtained confirmed almost to the letter the conclusions already arrived at.

2. Joint Meeting with Sections F and J. Discussion upon *Vocational Training and Tests.*

Dr. C. W. Kimmins stated that intelligence tests are now being used effectively in connection with defective children who are being sent to special schools. Similarly, intelligence tests were required in awarding scholarships. Children frequently passed examinations well, but had not sufficient native ability to profit further. Investigations regarding the after-school employment of children made in the London districts showed the need of vocational tests, and the desirability of adopting a system of tests by which it would be possible for a child to receive, on leaving school, a statement showing the line of employment for which he or she was best fitted.

Mr. Kennedy-Fraser.—Two recent developments have made vocational training of great importance as a part of a system of general education, viz.: (1) the extension of the school age; (2) the discovery of an appreciable proportion of the general population to be so definitely sub-normal in intelligence as to be quite unfit to profit by any general course of education devised to meet the needs of the average pupil over the age of ten. By applying a general intelligence test to all the children in a school system it would be possible to give the higher stages of a general education only to those who show they can profit by it, while to those who come out at the lower end of the scale some form of pre-vocational training suited to their mental level could be substituted.

Dr. C. S. Myers, F.R.S., urged that right choice of occupation was necessary, in order to avoid unhappiness, waste of time and money. The choice must be made by the individual: no compulsion against his convictions; no drift. The choice should be assisted during the last year of school life by the use of the cinematograph, and lectures on duties, responsibilities, prospects and drawbacks of various occupations. The advice of parents and teachers was inadequate; expert advice was essential. The expert should visit the school, and have access to school reports, which should show a continuous record of the child's activities and interests.

Mr. F. Watts believed that with a properly organised state of society vocational tests would be an absolute necessity; that industry itself must be brought into vital contact with the school, and must show a more sympathetic interest in the continued education of its young entrants.

Sir William Beveridge, K.C.B., urged a more careful selection of boys for the jobs they had to do, and condemned the methods of selection adopted by the majority of the employers.

Friday, September 9.

3. *Report of the Committee upon Training in Citizenship.* See p. 361.

4. *Report of the Committee upon Educational Pictures.* See p. 376.

5. Joint Meeting with Section E. Discussion on *The Teaching of Geography.* See p. 429.

6. *Report of the Committee upon an International Auxiliary Language.* See p. 390.

Monday, September 12.

7. Presidential Address by Sir W. H. Hadow, C.B.E., on *The Place of Music in Education* (see p. 187); followed by a discussion opened by Mr. Plunkett Greene.

8. Dr. Cranage (Cambridge), Sir Halliday Croom (Edinburgh), Sir Henry Miers, F.R.S. (Manchester).—*Extra-mural University Education.*

Dr. Cranage stated there was a great and increasing demand for higher education outside the Universities. Evidence showed that extra-mural activities had a reflex benefit on intra-mural work.

Sir Halliday Croom showed that extra-mural teaching had played a very large and far-reaching part in the development of the Edinburgh School of Medicine.

Sir Henry Miers claimed that tutorial classes for working people had helped Universities to develop methods for teaching adults. Extra-mural work would need a resident tutor or organising representative in each populous centre.

9. Mr. D. Kennedy-Fraser.—*The Need for Objective Measurements in Experimental Education*; followed by an exhibition of Recent Test Material (in the Experimental Education Laboratory, Moray House).

In order to form a scientific basis for our conclusions on such educational questions as the relative value of different teaching methods, the nature of learning process, the innate capacity of the children and their resultant performance, we must have some reliable means of obtaining quantitative estimates which shall be free from the subjective differences of examiners. Objective scales of this nature are being worked out for the various elements of the school curriculum, and also for the different capacities, general and specific, of the children. Some of these were discussed by the lecturer, and others were shown in the exhibit which had been prepared in the experimental educational laboratory of Moray House.

Tuesday, September 13.

10. Dr. E. H. Griffiths, F.R.S.—*Science and Ethics.* See p. 479.

11. Mr. James McLaren.—*A Simple Method of Spelling English Phonetically.*

12. Mr. D. M. Cowan, M.P.—*The Cost of Education.*

The necessity of determining the margin of safety in respect of (1) health and education, and general social welfare of the people; (2) the Navy, Army, and other means of defence against possible attacks from without. The margin of safety in education should be such as to ensure the internal stability of the State and the maintenance of its trade and commerce in competition with other nations. The Acts of 1918 showed that we had not reached that standard, and the exigencies of the moment had postponed their full operation. The source of the funds required for its establishment was essentially national and not local, so that the burden should be distributed more evenly.

13. Dr. Alex. Morgan.—*University Reform.*

The increased demand for higher education resulting from the 1918 Education Act makes necessary a considerable amount of reform in our Universities—administrative, educational, and financial—not to a uniform type, but possessing the same ideals, to have individuality of function, and together provide a comprehensive means of higher learning for the whole nation. The chief governing body to be representative of all interests. The need of a small joint Committee of the four Scottish Universities to deal with the system as a whole—to co-ordinate their activities, to develop specialisation of function, and to mediate in cases of dispute and to pass ordinances with power of appeal to the Privy Council.

SECTION M.—AGRICULTURE.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 467.)

Wednesday, September 7.

Special Lecture to Farmers.—*Science and Crop Production*, by Dr. E. J. Russell, F.R.S.

Thursday, September 8.

1. Dr. Winifred E. Brenchley.—*Effect of Long-continued Manuring on Grassland.*

2. Dr. W. G. Smith.—*Methods of Grassland Analysis; Results for the Period* 1914-1920.

3. Dr. W. G. Smith and Dr. A. Lauder.—*A Survey (Botanical and Chemical) of Hill Farms in the Lothians.*

4. Mr. M. M. Monie.—*Account of Soil Survey Work in West of Scotland.*

5. Prof. J. Hendrick.—*The Absorption and Retention of Manurial Substances by Granite Soil.*

6. Mr. H. J. Page and Mr. H. G. Thornton.—*On the Rapid Fluctuations in Bacterial Numbers and Nitrate Content of Field Soil and their inter-relation.*

In the afternoon demonstrations were given as follows:

(*a*) Dr. F. A. E. Crew.—Demonstration on the Wools of Primitive Breeds of Sheep (at the Station for Research in Animal Breeding, High School Yards).

(*b*) Dr. R. Stewart MacDougall.—Exhibition of Insects Injurious to Stock (at the Entomology Laboratory).

(*c*) Exhibition of Early Works dealing with Agriculture and Kindred Subjects (at the Agricultural Department).

Friday, September 9.

7. Dr. J. F. Tocher.—(*a*) *Citric Solubility of Mineral Phosphates;* (*b*) *Method of Determining the Significant Differences of Yield of Milk;* (*c*) *Statistical Analyses of Scottish Milk Records.*

8. Prof. J. Hendrick.—*A New Scheme for the Determination of Unexhausted Manurial Values.*

9. Prof. R. A. Berry.—*Production and Utilisation of Whey.*

10. Prof. R. H. Leitch.—*Research Work in Dairying.*

11. Dr. Wm. Taylor and Mr. A. D. Husband.—*Note on the Effect of Varying Rates of Secretion of Milk on its Percentage Composition.*

12. Mr. J. Alan Murray.—*Ensilage.*

Ensilage is a means of conserving forage for winter keep. The only alternative is to dry the forage, i.e. to make it into hay. The former is the more expensive method, and causes greater deterioration. The greater expense of ensilage is attributable mainly to the annual charges for capital expenditure on silos and machinery for filling them. The expenses of conservation by ensilage

are about six times the cost of conserving the same crop (oats and tares) by drying. If the expenses of producing the crop are included the total cost (per acre) of silage is about double that of hay.

The nature and extent of the deterioration in ensilage may be inferred from analyses. Results recently obtained by the author were as follows :

Oats and Tares.	Albd.	Fat.	N-free Ext.	Fibre.	Ash.	Water.
Original Fodder . .	1·98	0·21	7·97	4·98	1·45	83·5
Silage from same . .	2·08	0·25	3·54	6·07	3·36	84·7

The alteration in the percentage of ash corresponds to a loss of 65.7 per cent. of the original organic matter, comprising 54.7 per cent. of the albd., 48.8 per cent. of the fat, 80.8 per cent. of the N-free ext., and 47.2 per cent. of the fibre. The following analyses by Grandeau have been quoted to show how slight is the alteration in composition :

Maize.	Albd.	Fat.	N-free Ext.	Fibre.	Ash.	Water.	Acid.
Original . .	1·22	0·25	10.99	4·98	1·29	81·28	—
Silage . .	1·24	0·36	9·73	4·91	2·25	81·28	0·23

The alteration in the percentage of ash corresponds to a loss of 45.6 per cent. of the original organic matter. This loss is above the average; the previous case is extreme. The actual loss varies widely; the average is probably about one-third in the case of oats and tares and about one-fifth in the case of maize. In all cases the loss falls most heavily upon the constituents of the N-free extract, i.e. upon the most readily digestible constituents of the food. The relative increase in the more resistant fibre lowers the nutritive value of the remainder. Haymaking also is subject to risk. The losses are similar in character, but the average loss in haymaking is less than the average in ensilage.

The notion that farmers can save money by dispensing with root crops and substituting silage in the rations of animals is a fallacy. The larger part of the expenses of root growing is attributable to fallowing operations, and should be spread over the whole rotation. Discontinuance of root growing would appear to involve a return to the system of bare fallow. If silage can be safely substituted for roots there is no apparent reason why the same fodder conserved by drying (hay) should not be substituted instead. It is well known that this cannot be done unless the resulting deficiency be made good by additional cake or meal, for the dry matter of roots has twice the nutritive value of an equal weight of dry matter in hay and silage. The fact that silage and roots are both succulent fodders is irrelevant. Cakes and meals which, like hay, are also dry can be substituted for roots because they are of similar dynamic value.

The widespread impression that the allowance of cake may be reduced if silage be substituted for roots is mistaken. The exact contrary is the truth, except in the rare case in which the original ration contains only the minimum of protein. It is estimated that a cow of 1,000 lb. live weight producing $2\frac{1}{2}$ gallons of milk would require about 12 lb. of starch equivalent. If silage be substituted for roots in proportion to the amount of dry matter the amount of cake required to complete the ration would be 7.7 lb. instead of 5.5 lb., as shown in the tabular statement below (σ = starch equivalent per lb.) :

	lb.	σ	Total.		lb.	σ	Total.
Roots . .	80	× ·07 =	5·6	Silage . .	40	× ·10 =	4·0
Hay and Straw	10	× ·25 =	2·5	Hay and Straw	10	× ·25 =	2·5
Dec. Cake .	5·5	× ·71 =	3·9	Dec. Cake .	7·7	× ·71 =	5·5
			12·0				12·0
Digestible Protein . .			2·31	Digestible Protein . .			3·39
Total Dry Matter . .			22·9	Total Dry Matter . .			25·3

The silage ration contains more protein than the roots ration ; but, as only about $1\frac{1}{2}$ lb. is required, the latter contains more than enough.

In the afternoon an excursion took place to the Plant Breeding Station and Neighbouring Farms.

Saturday, September 10.

Excursion to East Lothian Farms.

Monday, September 12.

13. Presidential Address by Mr. C. S. Orwin on *The Study of Agricultural Economics.* See p. 194.

14. The Rt. Hon. Lord Bledisloe.—*Wheat as the Basis of Britain's Food Supply in Time of War.*

Advantages of Potatoes (supplemented by Pig Meat) over Wheat: (1) Great Britain self-contained in its potato requirements, and an exporter. Under normal conditions she imports four-fifths of her wheat requirements from abroad. (2) The normal production of wheat is preponderantly in the Eastern counties of Great Britain (ten counties out of eighty-six provide more than half the total output). Potatoes grown in every part of the kingdom. (3) Many farmers are unfamiliar with wheat production, and have neither implements nor buildings necessary for its production and storage. Every farmer, gardener, and allotment-holder knows how to grow potatoes. (4) Wheat crop may be wholly lost for human requirements by bad weather or incendiarism. Potatoes, though subject to disease (which can be minimised by spraying), are less vulnerable, as the edible tuber is beneath the ground. (5) Potatoes provide an immense quantity of starchy food, far exceeding wheat in output per acre. Crop can be obtained in shorter time and harvested at different periods of spring, summer, and autumn. (6) Potatoes are relatively deficient in fat and protein, but these can be supplied, by way of supplement, by pig-meat. Production of pigs in war-time should be encouraged, and not discouraged, as during late war: their capacity for rapid reproduction, large families, high percentage of fat-yield, and great variety of food products, render them invaluable meat-providers in a national emergency. Grazing varieties deserve special encouragement. (7) Large areas of permanent and temporary pasture provide a valuable storehouse of accumulated fertility, to be utilised in time of war, when fertilisers are bound to be scarce. No crop thrives better in newly-turned pasture than potatoes. (8) Potato flour can be converted into wholesome and palatable bread, scones, and cakes. (9) Surplus or unsuitable potatoes can be utilised both as stock food and as source of motor spirit, commercial starch, etc. (10) The home production of breadstuffs in the form of potatoes will reduce to a minimum the cost and risks of marine transport. Moreover, their production in every part of the kingdom for local needs will largely reduce the strain on internal transport.

15. Sir Henry Rew.—*Agricultural Statistics: Their Collection and Use.*

16. Mr. A. W. Ashby.—*Standards of Production in Agriculture.*

17. Mr. Pryse Howell.—*Economic Surveys of Agriculture in Wales.*

Tuesday, September 13.

18. Dr. W. E. Elliott, M.P., and Mr. Arthur Crichton.—*Rickets in Pigs.*

During the past two years a series of feeding and metabolic experiments have been conducted on pigs, with the object of determining the cause of a disease variously known as 'rheumatism,' 'cramp,' or 'rickets.' The most obvious signs of the disease are loss of appetite, lethargy, stiffness of the hind quarters, a stilted gait, and, later, loss of power of the legs. In severe cases deformities of the limbs and fractures of the ribs are found. The condition is very prevalent in pigs kept in confinement. The results of the

experiments seem to show that: (1) The condition is produced in animals deprived of access to earth or other mixtures of minerals, and fed only on grains and certain other concentrates commonly used in pig-feeding. (2) The inorganic constituents in these feeding-stuffs do not correspond with the requirements of the growing pig. There is a marked deficiency of calcium and an excess of acid radicles. (3) If the mineral matter of a ration composed of these feeding-stuffs be adjusted to the requirements of the animal by a mixture of salts compounded to correct the deficiencies the disease does not occur. (4) The addition of Fat soluble A or of Water soluble C to a ration that produces the condition does not prevent the onset of the symptoms.

It is believed that one, and probably the chief, factor in producing the condition is a deficiency in one or more of the necessary inorganic constituents of the food.

19. Dr. J. B. Orr.—*The Application of the Indirect Method of Calorimetry to Ruminants.*

In recent years calorimetry has been practised on the human subject by various indirect methods. In the simplest of these methods a sample of expired air is collected in a bag by means of a mouthpiece with a two-way valve. From the volume of air expired in a given period, and the difference in the percentage of O_2 and of CO_2 in the inspired and the expired air, the amount of O_2 absorbed and of CO_2 exhaled during the period can be determined. The ratio $\frac{\text{Vol. of } CO_2 \text{ exhaled}}{\text{Vol. of } O_2 \text{ absorbed}}$, known as the Respiratory Quotient, gives an indication of the nature of the material oxidised. From the amount of oxygen absorbed the heat production can be calculated, since the consumption of any given amount of oxygen corresponds to the liberation of a definite amount of heat, the amount of heat per unit of volume of oxygen varying with the Respiratory Quotient.

For the application of the method to ruminants a special mask has been constructed. The mask fits over the muzzle, and is made airtight by a rubber band, which can be inflated. It is provided with two valves—an inlet and an outlet. From the outlet the expired air can be conveyed to and collected in a large bag. The volume of the air expired during the experimental period is determined by passing it through a gas-meter. The percentages of O_2, CO_2, and combustible gases in the expired air are determined by a modified Haldane's gas analysis apparatus.

From these results it is possible to calculate (*a*) the rate of formation of combustible gases; (*b*) the proportion of expired CO_2 which is due to fermentation in the rumen; (*c*) the heat production due to fermentation; and (*d*) the heat production due to tissue metabolism. Experiments which have been carried out by means of an airtight chamber have shown that CH_4 and CO_2 are not lost through the skin or by the anus in amounts that seriously interfere with the accuracy of the results. Preliminary experiments have been carried out on the goat to determine the rate of metabolism of the animal (*a*) standing and lying, and (*b*) before and after feeding.

Following this paper Mr. J. Golding exhibited photographs of an abnormal litter produced by a sow fed on a diet deficient in vitamin A.

20. Major C. C. Hurst.—*The Genetics of Egg Production in Poultry.*

Five years' experimental breeding on Mendelian lines, from 1910-16, with three distinct utility strains of White Leghorns and White Wyandottes, show that the first year's egg-production of a hen depends on the combined action of at least seven main genetic factors. The Mendelian pairs identified are :—

E — e	Early	— Late	Sexual maturity of pullets.
W — w	Fast	— Slow	Rate of winter production.
S — s	Fast	— Slow	Rate of spring production.
M — m	Slow	— Fast	Rate of autumn production.
H — h	Broody	— Non-Broody	Instinct.
N — n	Small	— Large	Egg-mode.
C — c	Brown	— White	Egg-mode.

The first of each pair is dominant, and the second recessive. In the course of the experiments more than 50,000 eggs were recorded, and each egg was weighed and graded for size and colour. In order to analyse the complex and continuous data of egg-production a system of uniform gradings was formulated which ultimately led to the identification of the factorial pairs. The somatic gradings are based throughout on the genetic factors concerned, so that each hen has its somatic and genetic characteristics combined in a single formula. Production is graded in percentages.

SUMMARY OF THE RESULTS IN WHITE LEGHORNS AND WHITE WYANDOTTES.

Characters	Totals	Dominant	Observed	Expected	Recessive	Observed	Expected
Sexual maturity .	335	Early (E) .	286	289·00	Late (e) .	49	46·00
Winter rate	266	Fast (W) .	231	231·75	Slow (w) .	35	34·25
Spring rate	224	Fast (S) .	216	217·25	Slow (s) .	8	6·75
Autumn rate	194	Slow (M) .	140	145·50	Fast (m) .	54	48·50
Broodiness .	201	Broody (H)	50	58·50	Non-B. (h)	151	142·50
Egg-size .	331	Small (N) .	135	128·75	Large (n) .	196	202·25
Egg-colour .	331	Brown (C) .	137	142·00	White (c) .	194	189·00
Totals. .	1882	—	1195	1212·75	—	687	669·25

Eighteen definite exceptions appeared, of which two proved to be somatic and not genetic, eleven were slight exceptions probably of the same nature, three were pathological, one was possibly an incomplete dominant, while one was apparently a true mutation.

Pearl's discovery of two genetic factors for winter-production in Plymouth Rocks (1912), confirmed by Goodale in Rhode Island Reds (1918–19), is also confirmed in White Leghorns and White Wyandottes, in which the presence of both E and W factors is necessary for high winter-production.

No sex-linkage was found in either of the production factors of the Leghorns or Wyandottes used, and in this respect these two breeds resemble Goodale's Rhode Island Reds rather than Pearl's Plymouth Rocks.

There is a definite difference of rhythm between the discontinuous Slow (w) and the discontinuous Slow (s) birds, and it is possible that the Slow (s) birds are pathological.

Slow (M) birds are deep autumn moulters, while Fast (m) birds are partial autumn moulters.

A sensible proportion of broody hens do not show broodiness until their second season, so that it was not possible to ascertain in all cases the true nature of the 'Non-Broodies.'

The appearance of a few broody exceptions in the Leghorns gives support to Punnett's (1920) suggestion of the possible presence of an inhibitor to the broody factor in certain non-broody birds, and an HI scheme of broodiness does bring into line many of the complicated and conflicting data published. To demonstrate this satisfactorily, experiments on a considerable scale would be required.

The results indicate the gradual evolution of the increase of fecundity in the hen by a succession of definite and discontinuous steps or mutations. Early maturity, fast winter rate, fast spring rate, and possibly brown-egg are dominant mutations, while fast autumn rate, large-egg, and possibly non-broodiness are recessive mutations. A single case of the occurrence of a recessive mutation for large-egg was observed, which originated in a Wyandotte cock. The experimental matings made were not suitable for testing satisfactorily the question of linkages of the above factors.

Economic Significance of Results.—Pure and permanent strains of high producers of large eggs can be made by the elimination of birds carrying e w s M H N factors in accordance with the factorial scheme presented. E W S m h n birds are the best layers, and E E W W S S m m h h n n birds

breed true. Results show that high production is not incompatible with high fertility and vigour, as is often supposed. Old methods of grading production by winter and annual records are inadequate somatically, and misleading genetically. All things are possible to a winter-record in early hatchings.

The system of grading production presented has a double value to the practical breeder, because the descriptive somatic gradings, being based on the genetic factors concerned, give a line also to the breeding value of the bird, for the extreme grades tend to breed true. The grading of winter-production by percentages minimises the unequal influences of variable dates of hatching. The adoption of this grading system to laying competitions would lead to rapid progress in poultry-breeding, and be of educational value to poultry-keepers in general, for the winning birds would breed winners with more frequency than they do now, and the reasons would be obvious. An extension of the laying competitions to 56 weeks or 400 days would extend the biological year of the layer to fourteen lunar months, and thus eliminate the deep moulters (M birds). The result would be that the 200-egg hen would soon be superseded by the 300-egg hen.

Brown or white egg-mode can be bred true, and broodiness can no doubt be eliminated eventually by extended progeny-tests. The large egg-mode, which is so important in the Wyandotte, can be bred true by the elimination of the N birds of both sexes. The practical proof of the above scheme lies in the fact that homozygous strains of E E W W S S m m n n c c birds were bred in 1914 at Burbage from the heterozygous birds of the original pedigree strains.

Since the War (1919-21) other pedigree strains and other breeds have been tested with success on the basis of the above factorial scheme.

21. Miss Dorothy J. Jackson.—*Genus Sitones and its Importance in Agriculture.* (With Lantern Demonstration and Exhibition of Specimens.)

The genus Sitones belongs to the Curculionidæ, or weevils, and includes several species which are well-known pests of cultivated Leguminosæ in Europe and America.

Research was commenced in 1918, with the object of discovering which species were injurious to leguminous crops in Britain, and the life history of these species has been determined.

The injurious British species may conveniently be divided into two groups; the first group includes those species which breed principally upon peas, beans, and tares, and which later migrate to clover and lucerne. The most important species in this group is *Sitones lineatus*, L., which in the adult stage causes serious damage to the foliage of peas and beans in spring, especially in the south of England. In the larval stage it is very destructive to the root-nodules of these plants. The second group includes those species which occur upon clover throughout the year. Such are *S. flavescens*, Marsh, *puncticollis*, Steph., *sulcifrons*, Thun., and *hispidulus*, F., the latter occurring also on lucerne. These species in the adult stage are much less injurious than *S. lineatus*, but in the larval stage are destructive both to the roots and the root-nodules of the clover.

No satisfactory method of control is at present known. In the case of the species which breed upon clover, control would be extremely difficult, on account of their prolonged period of egg laying, but this difficulty would not apply to the species which breed upon peas and beans. Sitones are liable to attack by insect and fungus parasites. Thus various species of Braconidæ belonging to the genera *Perilitus*, *Liophron*, and *Pygostolus* have been bred from the adults of several of the injurious Sitones. The fungus *Botrytis bassiana* (Balsamo), Montagne, is common on these beetles, and laboratory experiments on infection with the fungus spores have proved very successful, death invariably occurring in nine to thirteen days.

22. Miss M. S. G. Breeze.—*Degeneration in Anthers of Potato.*

The investigation on degeneration in potato anthers described below was started four years ago as a necessary accompaniment to plant-breeding experiments which were undertaken at the same time for economic ends.

The sterility of potato flowers, particularly on the male side, is a well-known fact, and a knowledge of the condition of the anthers is required at the outset of any hybridising work in order to avoid unnecessary labour and waste of time.

As the results of Tackholm and others show, such an investigation should lead to a better understanding of some phylogenetic and systematic problems.

The five anthers of the potato are long and fleshy. The yellow colour is due to a pigment in the anther wall, the pollen itself appearing as a white powder to the naked eye.

In the economic varieties of potato two distinct types of pollen have been identified—long and short. The long type is found in the anthers of Myatt's Ashleaf and in some hybrids derived from this variety, whereas in Great Scot, Leinster Wonder, Majestic, Ker's Pick, and in many others, the pollen grain is polygonal in shape, being about as long as it is broad, with usually a pentagonal face at each end. Amongst this latter type of grain are a considerable number of giant or compound grains. These giants appear to be perfectly healthy, and in rapidly degenerating pollen, such as is found in Great Scot, are the last to survive.

Types of Pollen Degeneracy.—Three types of anther degeneracy have been demonstrated in the varieties of potato grown at the present time.

1. The anthers of many economic varieties contain a larger or smaller percentage of *shrivelled grains.* In Great Scot, for example, most of the grains are found to be shrivelled and empty. The degeneration of the grain takes place after the formation of the pollen mother cell.

It must be supposed that the breakdown occurs immediately after the release of the grain from the pollen mother cell. The nucleus is first observed to degenerate, the cytoplasm becomes thin, and takes stains with difficulty. Finally the contents of the grain entirely disappear, leaving only the empty skin. This type of pollen degeneracy is by no means uncommon among angiosperms, and it is of interest to note that the breakdown rarely occurs previous to the formation of tetrads. This has been found to be the case by Juel in *Syringa chinensis*, Geerts in *Œnothera Lamarckiana*, and Dorsey in the Grape. I have also found the same stages of degeneration in Petunia and Rosa hybrids.

2. *Hypertrophied Grains.*—A form of degeneration in potato pollen which occurs frequently in the same anthers as does the shrivelled pollen type of degeneration just described is that of hypertrophied or swollen grains, many of which contain minute bodies which give the starch reaction when treated with iodine. In all examples of potatoes *producing pollen* I have found a smaller or larger percentage of these hypertrophied and starch-filled grains. When placed on the stigma some of them have been observed to send out germ tubes, which are distinguished from the pollen tube of the normal grain by their inflated and irregular shape. Similar hypertrophies have been found by me in Petunia and Rosa hybrids. They have also been described for *Rumex crispus* by Winfield Dudgeon (*Bot. Gaz.*, Nov. 1918).

3. *Up-to-date Type of Pollen Degeneracy.*—Several varieties of potato exhibit the form of anther degeneracy to be described below. The usual case for this type is for no pollen to be formed at all. Section of very young anthers of *Up-to-date* flowers show pollen mother cells which are apparently normal. No reduction division has been observed. Slightly older buds, in which the anthers are still quite green, have the anther sacs filled with bodies, many of which have the appearance of hypertrophied mother cells. These bodies vary in size and shape and granular density. Some consist of a network of cells, each cell of which is more or less pentagonal, and some of which were observed to have five round chromosome-like particles of chromatin. Larger bodies contained dark lateral masses connected with a semi-transparent tube. Still larger ones showed the dark masses completely separate.

At present it is difficult to speak with any definiteness of the Up-to-date type of potato-anther degeneracy pending further investigation.

Heredity of Degenerate Pollen.—Dr. Salaman has found that 'the heredity of male sterility is distinctly dominant.'

It is hoped to determine, if possible, the extent to which the various types of degeneration described above are inherited. This year 300 seedlings have

been raised as the result of a successful cross between Up-to-date and Leinster Wonder. Apart from the promising economic value of the hybrids, they should prove useful material for the further investigation of the pollen problems.

Correlation of Degenerate Pollen to Disease.—An example of pollen sterility accompanying a diseased condition in the plant is to be found in the Quercina type (formerly called *Quercina mutant*) in the Jimson weed described by A. F. Blakeslee in the *Journal of Genetics*, April 1921. The typical Quercinas have anthers containing only a few sterile (shrivelled) pollen grains.

In potato, out of twenty-five varieties which showed markedly the Up-to-date type of degeneration, nineteen are recorded as being susceptible to wart disease.

It is too early yet to deal with the morphological and physiological problems that lie behind the facts stated. The question whether these states of degeneracy are leading to a unisexual condition in many varieties, or whether they denote a hybrid origin, with accompanying disturbances of the sexual cells, remains to be demonstrated.

REFERENCES TO PUBLICATION OF COMMUNICATIONS TO THE SECTIONS.

AND OTHER REFERENCES SUPPLIED BY AUTHORS.

Under each Section, the index-numbers correspond with those of the papers in the sectional programmes (pp. 408–464).

References indicated by 'cf.' are to appropriate works quoted by the authors of papers, not to the papers themselves.

General reference may be made to the issues of *Nature* (weekly) during and subsequent to the Meeting, in which résumés of the work of the sections are furnished.

Section A.

4. To be published in *Astrophysical Journ.* (Contributions of Mt. Wilson Observatory).

7. *Philosophical Mag.*, Oct. 1921.

9. Expected to be published in *Proc. Roy. Soc. Edinb.*

12. Cf. *Sc. Proc. Roy. Dublin Soc.*, xvi, 18, Mar. 1921 ; *Nature*, vol. cvii., June 23, 1921 ; *Engineering*, Sept. 9, 1921.

14. An earlier paper on this subject is to be published in *Proc. Roy. Soc. Edinb.*

18. Cf. *British Rainfall*, 1921 (when published).

29. Cf. *Astrophysical Journ.*, **18**, 287, Nov. 1903 ; *Monthly Notices R.A.S.*, **58**, 431, 539 ; **76**, 15 ; **80**, 574 ; **81**, 515.

Section B.

3. See *Journ. Chemical Industry, Chemical Trade Journ.*, &c.

7. Cf. Jaeger, 'The Action of Light of short wave-lengths on some Organic Acids and their Salts,' to be published in *Trans. Chem. Soc.*

12. Cf. *Journ. Institute of Metals*, **32**, pp. 241-276, 1919 ; further matter to be published later in same.

13. *Engineering*, Oct. 14, 1921.

Section C.

2. To be published in *Scottish Geographical Mag.*

5. Expected to be published in *Geol. Mag.*

9. *Geol. Mag.*, Nov. 1921.

SECTION D.

3. *Journ. of Genetics*, **11**, 2, pp. 141-181, Sept. 1921.

7. *Proc. Zool. Soc. London*, Sept. 1921.

10. Cf. D. Ward Cutler, 'Observations on Soil Protozoa,' in *Journ. Agric. Soc.*, **9** (1919); 'Method for estimating the number of active Protozoa in the Soil,' ibid., **10** (1920); D. Ward Cutler and L. M. Crump, 'Daily Periodicity in the number of active Soil Flagellates; with a brief note on the relation of Trophic Amœbæ and Bacterial Numbers,' in *Ann. App. Biol.*, **7** (1920).

11. To be published in *Museums Journ.*

16. (Dr. J. Drever.) To be embodied in article for *Brit. Journ. Psychol.*

20. Cf. 'On the Classification of Actiniaria,' part ii., to be published in *Quart. Journ. Microscop. Sci.*

22. Cf. Rennie, White, and Harvey, 'Isle of Wight Disease in Hive Bees,' in *Trans. Roy. Soc. Edinb.*, **52**, part iv. (No. 29), pp. 737-779.

24. Cf. 'Further observations on the reproductive system of *Cimex*, with special reference to the behaviour of the Spermatozoa,' in *Indian Journ. Med. Research*, **7**, No. 1, pp. 32–79, plates v–xii, July 1920.

25. Cf. 'Mollusca of the Inlé Lake,' in *Rec. Ind. Mus.*, **14**, p. 103, pls. xv-xviii (1918); 'The Gastropod Fauna of ancient lake beds in Burma,' in *Rec. Geol. Surv. Ind.*, **50**, p. 227 (1919).

27. Expected to be published in *Journ. Bombay Nat. Hist. Soc.*

SECTION E.

3. To be published in *Scottish Geographical Mag.*

4. Cf. *Geographical Journ.*, Sept. 1921; material to be published also in *Central Asian Journ.*, and in book form.

11. Cf. *Carte du Monde au Millionième, Rapport pour 1921* (Ordnance Survey Office, Southampton, Aug. 1921).

12. *Scottish Geographical Mag.*, Oct. 1921; cf. book, *Ancient Tales from Many Lands*, to be published shortly (Benn).

13. *Scottish Geographical Mag.*, Oct. 1921.

SECTION F.

On the general results of inquiries made by committees of this Section into questions of national finance and labour during recent years, see *British Finance during and after the War*, 1914-21, co-ordinated and revised by A. H. Gibson, and *British Labour, Replacement and Conciliation*, 1914-21, both volumes edited by Prof. A. W. Kirkaldy, and published by Sir Isaac Pitman & Sons, 1921, 10*s.* 6*d.* each volume.

3. Cf. 'Influence of Trade Unionism on Wages,' in *Edinb. Rev.*, July 1913.

5. *Financial News*, Sept. 10, 1921.

7. *Economic Journ.*, Sept. 1921.

8. *Bankers' Mag.*, Oct. 1921.

11. Cf. Dr. Mary Rankin, *Arbitration and Conciliation in Australia* (London, Allen & Unwin, 1916).

12. To be published in *Economic Journ.*

SECTION G.

2. *Engineering*, Sept. 9, 1921, p. 387.
3. ,, Sept. 9, 1921, p. 368.
5. ,, Sept. 9, 1921, p. 389.
6. ,, Sept. 16, 1921, p. 420.
7. ,, Sept. 16, 1921, p. 410.
8. ,, Sept. 16, 1921, p. 424.
10. ,, Sept. 16, 23, 1921, pp. 407, 462.
11. ,, Sept. 16, 1921, p. 422.

12. For discussion on Report see *Engineering*, Sept. 16, 1921, p. 408.

13. *Engineering*, Sept. 16, 1921, p. 399.

14. *The Electrician*, Sept. 23, p. 386.

15. ,, Sept. 30, p. 408.

16. *Engineering*, Nov. 4, 1921, p. 642.

17. ,, Sept. 23, 1921, p. 458.

18. ,, Sept. 23, 1921, p. 457.

19. ,, Oct. 7, 1921, p. 523.

20. ,, Sept. 23, 1921, p. 456.

23. ,, Oct. 21, 1921.

Section H.

3. To be published in *Burlington Mag.*

4. To be published in *Man.*

5. Cf. *Antiquaries Journ.*, Apr. 1921 ; *Proc. Prehist. Soc. E. Anglia*, 1921.

7. Cf. Ridgeway, *The Dramas and Dramatic Dances of Non-European Races*, Cambridge, 1915.

8. To be published in *Journ. Roy. Anthrop. Inst.*

9. To be published in *Annual* (**24**) of British School at Athens.

11. Cf. *Antiquaries' Journ.*, i. (1921), p. 61 ; *ibid.* ii. (Jan. 1922) ; *Times Lit. Supp.*, Dec. 2, 16, 1920, pp. 794, 856.

14. To be published in *Journ. Roy. Anthrop. Inst.*

15. To be published in *Folk Lore.*

16. Cf. Hilton-Simpson, *Among the Hill Folk of Algeria* (Fisher Unwin, 1921) ; 'The Influence of its Geography upon the People of the Aures massif, Algeria,' to be published in *Geog. Journ.*

17. To be published in *Journ. Roy. Anthrop. Inst.*

18. Cf. 'Bodily Measurements and Human Races,' in *Journ. Asiat. Soc. Bengal* (n.s.), **16**, pp. 41-56, pls. iii-vi, 1920.

20. *John o' Groat Journ.*, Sept. 30, 1921, p. 2.

21. To be published in *Folk Lore.* Cf. Hastings' *Encyclopædia of Religion and Ethics*, **3**, pp. 358 *seqq.*, **5**, pp. 678 *seqq.* ; MacCulloch, *Fairy and Kindred Beliefs* (Marshall Jones Co., Boston, U.S.A., forthcoming, 1922).

Section I.

7. Cf. McDowall, D.Sc. Thesis, Univ. of Edinburgh, 1921.

14. Prof. Briggs' paper is expected to be published in *Proc. Roy. Soc. Edinb.* The results of Drs. Haldane's and Douglas' investigations are to be published in *Journ. of Physiology.*

15. Cf. Edridge-Green, 'New Facts of Colour-Vision,' in *Nature*, Aug. 25, 1921 ; *The Physiology of Vision* (Bell, London, 1921).

Section J.

5. Expected to be published in *Brit. Journ. of Psychology.*

8. Cf. Pear, 'The Rôle of Repression in Forgetting,' in *Brit. Journ. Psych.* **7**, pp. 139-146, 1914-15 ; *Studies in Remembering and Forgetting* (to be published shortly).

13. *Psyche*, **2**, 2 (n.s.).

15. The subject is to be dealt with by Dr. Kimmins in a book, *The Springs of Laughter*, to be published shortly.

17. *Psyche*, **2**, 2 (n.s.).

SECTION K.

3 (*c*). Expected to be published in *Trans. Roy. Arboric. Soc. Scot.*

9. *Trans. Roy. Soc. Edinb.*, **53**, i, No. 1 ('Studies in Floral Morphology,' No. 2).

11. Expected to be published by Linnean Soc.

12. Expected to be published in *Annals of Botany.*

13. *Gardeners' Chronicle*, **70**, pp. 160, 174 (1921); to be published in greater detail in *Journ. Roy. Hort. Soc.*, 1922.

17. Summary to be published in *Discovery.*

21 (*b*). To be published in *Proc. Roy. Soc.*

SECTION L.

1. *Journ. Experimental Pedagogics*, Dec. 5, 1921.

SECTION M.

For Dr. Russell's special lecture, see *Nature*, Sept. 22, 1921.

1. A partial account of Rothamsted Park grass experiments since 1877, expected to be published in *Journ. Agric. Sci.*

5. Probably to be published in *Journ. Agric. Sci.*

6. Expected to be published in *Journ. Agric. Sci.*

7 (*a*, *b*). *Journ. Agric. Sci.*, 1921.

7 (*c*). *Biometrika*, 1922.

8. Cf. Hendrick: 'Unexhausted Manurial Values: a criticism, with some suggestions,' in *Trans. Highland and Agric. Soc.*, **27**, pp. 256-280 (1915); 'An improved scheme for determining Unexhausted Manurial Values,' *ibid.* **32**, pp. 1-34 (1920).

11. Expected to be published in *Journ. Agric. Sci.*

12. To be published in *Estate Book* (Country Gentlemen's Assoc.).

14. Cf. Lord Bledisloe, *Pigs and Potatoes with Milk as the Basis of Britain's Food Supply* (London, Rees, 1921).

15. *Journ. Ministry of Agriculture and Fisheries*, Oct. 1921. Cf. Rew, *Food Supplies in Peace and War* (London, Longmans).

18. To be published in *Brit. Journ. Experimental Pathology.*

19. To be published in *Journ. Agric. Sci.*

20. Cf. *Report*, World's Poultry Congress, The Hague, 1921; *National Poultry Journ.* **2**, p. 148 (1921); *Report*, Internat. Eugenics Congress, New York (1921, to be published).

22. Cf. *Annals of Applied Biology*, **7**, Nos. 2 and 3, pp. 269-298, Dec. 1920; and further papers expected to be published in same journal.

SECTIONAL COMMUNICATIONS

ORDERED BY THE GENERAL COMMITTEE TO BE PRINTED *in extenso.*

DISCUSSION ON THE STRUCTURE OF MOLECULES.

Dr. IRVING LANGMUIR opened the discussion with an account of the theory proposed by G. N. Lewis in 1916, and subsequently extended by the speaker, according to which the fundamental conceptions of valence and the numerical values of positive and negative valence and covalence may be derived for most elements from a few postulates regarding the structure of atoms. Assuming the Rutherford type of atom, consisting of a positive nucleus surrounded by a number of electrons equal to the atomic number of the atom, and also assuming that Coulomb's law applies to the forces between the charged particles in the atom, the existence of repulsive forces must also be recognised. These forces prevent the electrons from falling into the nucleus, but it is immaterial for the purpose whether the repulsive force be dynamic, as assumed by Bohr, or static, as assumed by Lewis and by the speaker. Only three postulates have to be made.

Postulate 1.—*The electrons in atoms* (*or ions*) *tend to surround the nucleus in successive layers containing* 2, 8, 8, 18, 18, *and* 32 *electrons respectively.*—When the number of electrons is such that they cannot all form into complete layers in accord with Postulate 1, the extra electrons remain in the outside incomplete layer, called the *sheath* of the atom. Every electrically neutral atom must contain a number of electrons equal to the atomic number of the nucleus. Should the outside layer be nearly complete, a few additional electrons may be taken up to complete the layer, forming a negatively charged ion. The sheath of any atom or atomic ion consists of all the electrons in the outer layer, provided that this layer is incomplete when the atom is electrically neutral. The inert gases are the only elements whose neutral atoms have no sheaths, since their outer layers consist of electrons which already form a complete layer in the neutral atom. Sodium and calcium ions also have no sheaths, but the atoms of those metals have an incomplete sheath containing one electron. The fluorine atom has an incomplete sheath of seven electrons, whilst the fluorine ion has a complete sheath of eight electrons. The tendency expressed by Postulate 1 can only be satisfied by an interaction between atoms involving a rearrangement of electrons. This is to be regarded as the fundamental cause of chemical action. A complete compound is formed if the interaction between atoms leads to the complete satisfaction of the postulate.

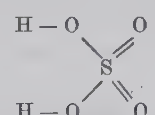
Any pair of electrons which is rendered stable by the proximity of one or more positive charges is called a duplet. Postulate 2 states that *two atoms may be coupled together by one or more duplets held in common by the completed sheaths of the atoms.* A given group of neutral atoms may interact to complete their sheaths in two ways : (1) Atoms having sheaths containing only a few electrons may give up these extra electrons to other atoms, and atoms having nearly complete sheaths may take up electrons from other atoms. (2) Atoms may share duplets with other atoms, and so complete their sheaths with fewer electrons than would otherwise be necessary. The transfer of electrons corresponds with positive and negative electrovalence, whilst the sharing of duplets corresponds with covalence. The term electrovalence covers both positive and negative valence, which differ only in algebraic sign, being positive when an atom gives up electrons and negative when it takes up electrons. The electrovalence of an atom in a compound may thus be defined as the number of electrons which the neutral atom must give up in forming that compound. If the neutral atom must take up electrons, the electrovalence must be expressed as a negative number. Defining the covalence as the number of duplets which an atom shares with neighbouring atoms, every duplet shared by two atoms corresponds with a (covalence) bond between atoms. A simple algebraic relation shows that the sum of the electrovalences and the covalences for all the atoms in any complete compound is zero.

The *kernel* of an atom is defined as that part of the atom which remains after the sheath is removed. Since the neon atom has no sheath, the whole atom constitutes a kernel with zero charge. The kernel of the sodium atom is the sodium ion with a single positive charge, whilst the kernel of the fluorine atom is the fluorine ion, consisting of the nucleus and two electrons, the whole having seven positive charges. The positive charge on the kernel is equal to the number of electrons in the sheath of a neutral atom. Assuming that duplets are shared equally by two atoms, which would be the case if the two atoms were substantially alike in size and structure, the sum of the electrovalence and the covalence, for any atom in a compound, is equal to the *residual atomic charge.*

Postulate 3.—*The residual charge on each atom and on each group of atoms tends to a minimum.*—By ' residual charge ' is meant the total charge regardless of sign. By ' group of atoms ' is meant any aggregate of atoms characterised by nearness to one another. It will be possible to express this as a quantitative law when the repulsive forces between charged particles are better understood. As a familiar example, in any small finite volume of a salt solution the charges on the positive and negative ions tend to be equal, or the residual charge tends to a minimum. Postulates 1 and 3 are often in conflict, and the result is then a compromise, complete compounds being formed provided that this can take place without the charges on the ions becoming too large. Although Postulate 3 does not definitely fix the charges of the individual atoms in the compounds under consideration, it does determine the distribution of these ions in space. This is a factor of prime importance in the crystal structure, in the electrolytic conductivity of substances in the liquid state, and in other properties. When the number of ions of one sign is much larger than that of the other sign, as in such compounds as $AlCl_3$, PCl_3, or SF_6, Postulate 3 requires that the negative halogen atoms shall surround the most strongly positive atoms. The ions thus form groups having strong internal and weak external fields of force, so constituting molecules of considerable stability and inertness. This is in accord with the volatility and absence of electrolytic conductivity of these compounds.

Salts are typically complete compounds, and when the atomic charges are small, as in NaCl, BaBr, K_2S, &c., the compounds are fairly readily fusible, soluble in liquids of high dielectric constant, good electrolytic conductors when molten or in solution, and volatile with great difficulty. With larger charges, as in MgO, BN, Al_2O_3, &c., the strong forces give great infusibility, insolubility, and hardness. These compounds are good electrical insulators at moderate temperatures, but conduct electrolytically when molten.

In any group of atoms Postulates 1 and 3 are both completely satisfied if the covalence of each atom is equal to the negative valence of that atom. The negative valences of carbon; nitrogen, oxygen, and sulphur are 4, 3, 2, and 2 respectively, whilst that of hydrogen and the halogens is one. On writing structural formulæ as employed in organic chemistry, using these values for the valence, results in complete accord with Postulates 1, 2, and 3 are obtained. It is seen that only negative valences can be used in such structural formulæ (that is, as covalences), and that even these can only be legitimately used in compounds from which electropositive atoms are entirely absent; for if some of the atoms have a positive residual charge, some others must have a negative charge, and for these as well as for electropositive atoms the covalence is not equal to the negative valence. From this point of view it is incorrect to write such a structural formula as

Na — Cl, 

in which the covalence of one atom is taken as equal to the positive valence of that atom. In crystalline salts only electrovalences are concerned. The sodium and chlorine atoms in sodium chloride are converted into ions by the passage of an electron from one atom to the other. There is no definite molecule, and each sodium ion is similarly related to six chlorine ions. The ready ionisation of the salt when dissolved in water is in accord with this view.

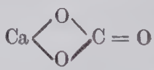
Since the sheaths of atoms of atomic number less than about twenty-five never contain more than eight electrons, the covalence of these atoms cannot exceed four, but with heavier atoms larger values might be expected. Large covalences are improbable in most cases, since they imply large negative valences, which means that the number of electrons in the sheath must be much larger than the charge of the kernel. There is evidence, however, that large covalences sometimes exist, the compounds $Fe(CO)_5$ and $Ni(CO)_4$, for instance, being complete compounds in which the central atoms have the covalences 10 and 8 respectively. Since the number of electrons in the sheath of iron is 8 and of nickel is 10, the complete sheath in each case containing 18 electrons, the negative valences are 10 and 8 respectively, or the same as the covalences needed to account for the above compounds.

The structure of a few substances is not adequately accounted for by the principles described, among them being N_2,CO,CN−, and NO. These may have the simple octet structure described in the speaker's earlier publications. The prediction that lithium hydride would exhibit the properties of a salt has been fulfilled, this compound having proved to ionise, hydrogen being an anion. In double molecules such as H_4O_2 (in ice), H_2F_2, and such compounds as KHF_2, it seems that the hydrogen nuclei, instead of forming duplets with electrons in the same atom, form duplets in which the two electrons are in different atoms. The hydrogen nucleus itself thus acts as a bond in such a case.

The theory thus outlined has been successful in accounting for the chemical nature of most compounds. The quantitative aspects are under consideration, and it is intended to put Postulates 1 and 3 into a form which will permit of at least rough calculations of the relative stabilities of various substances as measured, for instance, by the heat of formation.

Prof. A. Smithells exhibited models illustrating the constitution of atoms according to Langmuir's theory. The arrangement of electrons in layers was shown, and also the grouping of atoms in molecules to which the arrangement leads. For instance, the union of carbon atoms by means of electrons leads to a tetrahedral structure, already known from chemical evidence to exist. One series of models illustrated the structure of sodium carbonate historically, from the original arrangement of Dalton to that of Langmuir.

Prof. W. L. Bragg.—X-ray analysis makes it possible to study the state of molecular aggregation in a solid and to find the positions of the atoms relatively to one another. The structural formula of calcium carbonate, on the ordinary hypothesis of valency, is of some such form as



but an examination of the solid compound shows that three oxygen atoms are similarly related to every carbon atom. The crystal consists of two kinds of units, calcium atoms and CO_3 groups, the latter having perfect threefold symmetry, and there is no distinction between single and double bonds.

The interpretation of the structure of graphite is not quite so certain as that of calcite, but the evidence is very strongly in favour of a hexagonal arrangement in sheets.

Each carbon atom is equally related to three neighbours in the same plane. They are separated by a much greater distance from the atoms in other planes, and the cleavage indicates that the forces between successive planes are slight.

These hexagons might be written as benzene rings with valency bonds, but such a formula would indicate a lowered symmetry of the structure, which is not the case.

The work of Kossel, Lewis, and Langmuir has shown how valency may be partly explained. There is a sharp distinction between two cases. In the first, two separate electron systems, each surrounding a nucleus, are held together by electrostatic forces. In the second, the electron envelopes of each nucleus must be supposed to interpenetrate in some way certain electrons forming part of both systems.

This view is supported by the evidence of crystal structure. In such a compound as solid potassium chloride the crystal consists of an alternate arrange-

ment of positive and negative ions, there being no grouping of the atoms into molecules. The valency of the K atom cannot be represented by a single bond, but must be uniformly distributed around the atom among six neighbouring chlorine atoms. In many compounds a complex group of atoms forms the negative ion instead of a single atom. When electronegative ions are in combination there is evidence of a definite linking into molecules or atomic groups, of which CO_3 in calcite is an example.

The distances between atoms in the crystal structure are explicable in terms of Langmuir's conception, the electrons being arranged on a series of spherical shells whose diameters increase in arithmetical progression. The evidence given by the diffraction of X-rays is not in agreement with such a supposition. Some idea of the distribution of electrons about the nucleus may be got from measurement of the intensity of reflection of X-rays by a crystal. This intensity depends on the amount of X-rays scattered by a single atom, and that in its turn on the number and arrangement of electrons in the atoms. Recent measurements of the amplitude of waves diffracted by sodium and chlorine (Bragg, James and Bosanquet, *Phil. Mag.*, March and July, 1921) show that the distribution of electrons in those atoms is very different from that pictured. There is a far greater density of electrons near the centre of the atom than in the outer region. This is inconsistent with the Langmuir arrangement of stationary electrons on spherical shells. The success of Langmuir's model in explaining complex compounds does not depend on any assumption as to the exact structure of the atom, but on the fundamental conceptions of combination through electrostatic attraction and by electron sharing.

Prof. J. R. PARTINGTON.—1. Translational and rotational energies are approximately represented by the theory of equipartition; any excess of C_v above 6 is approximately parallel with the activities in non-polar gases.

2. Translational energy on the basis of the quantum theory with the *collision* frequency ν, is 2·98 for all gases, but the $\beta\nu$ values are of the same order as the observed values of C_v. The rotational frequencies may be whole multiples of the translational.

3. The value of n in $\eta = \eta_o \left(\frac{T}{273}\right)^n$, where $\eta =$ viscosity, T = temperature, is related to the critical pressure: $n = 0{\cdot}642 + 0{\cdot}00116\, p_c + 0{\cdot}0000399\, p_c^2$.

4. The molecular heat of hydrogen is not satisfactorily represented by Einstein's formula with $\beta\nu =$ const. (Eucken), or $\beta\nu \propto \sqrt{T}$ (Nernst). It is represented by Debye's formula with $\beta\nu = 65/\sqrt{T}$.

5. The model of the nitrogen molecule on Bohr's theory has the correct energy if three coplanar rings of 8, 4 and 2 electrons, between the two nuclei, are assumed, and $\nu = A\varpi/B \cos\phi$, as in Krüger's theory.

Prof. A. O. RANKINE.—It is possible in several cases to obtain support from the kinetic theory of gases for the views propounded to-day by Dr. Langmuir. The dimensions of the molecule Cl_2, for example, as estimated from the viscosity, are found to be very nearly those which we should expect if the molecule is like two argon atoms with their outer electron shells contiguous, as electron sharing would involve. Similar relations exist between bromine and krypton, and between iodine and xenon. In the same way it may be shown that the molecules CO_2 and N_2O, to the nearly identical physical properties of which Dr. Langmuir has called attention, are each of them kinetically equivalent to three neon atoms in a straight line with contiguous outer electron shells.

Further confirmation comes from the consideration of the two gases krypton and methane, in relation to rubidium and ammonium, according to the theory under discussion. If we could shear from an atom of Rb its single outer electron, and also rob its nucleus of one positive charge, the remainder would be an atom of Kr. The same process applied to the group NH_4 would convert the nitrogen atom into carbon, and leave us with the molecule CH_4. Thus methane bears to ammonium precisely the same relation as krypton bears to rubidium. Now, it is well known that the same salts of Rb and NH_4 are not only completely isomorphous, but are of nearly equal molecular volume. Or the domains occupied by Rb and NH_4 in crystals are almost identical. We should anticipate, therefore, that an atom of krypton and a molecule of methane would have the same

dimensions. This proves to be the case, the molecular diameters, as deduced from viscosity, being $3 \cdot 10 \times 10^{-8}$ cm. and $3 \cdot 13 \times 10^{-8}$ cm. respectively. The small difference is well within the limits of experimental error.

Dr. S. H. C. Briggs referred to the importance of Dr. Langmuir's theory of atomic structure for the inorganic chemist. As a result of the remarkable insight with which the theory has been developed much light has been thrown upon many obscure phenomena in inorganic chemistry, and the theory will probably exert a profound influence upon the valency problem.

At present the valency theory is unsatisfactory. On the one hand we have the idea of valency with all its modifications, such as partial valency, residual valency, &c., and on the other hand we have Werner's theory of co-ordination, which is an affinity theory rather than a theory of valency. Dr. Langmuir's work appears to bring us very near to the point where it should be possible to combine these various conflicting ideas into some wider and more harmonious conception. Thus if we regard the elements as compounds of kernels and electrons (denoting the kernel by the symbol of the element with a small *k* above, and the electron by E), we may look upon the eight electrons in the neon atom Ne^kE_8 as being co-ordinated to the kernel, just as the chlorine atoms are co-ordinated to the carbon atom in carbon tetrachloride according to Werner's theory. Similarly, potassium K^kE and chlorine Cl^kE_7 unite to give a co-ordination compound, potassium chloride $K^k[Cl^kE_8]$, exactly as potassium chloride KCl and auric chloride $AuCl_3$ combine to give a co-ordination compound potassium aurichloride $K[AuCl_4]$. The detailed development of this point of view (*Phil. Mag.*, 42 (1921), 448) leads to the conclusion that the co-ordination of electrons is involved in all valency phenomena. In other words, Dr. Langmuir has brought us into a region where the co-ordination theory and the older theory of valency converge and meet.

Dr. E. K. Rideal.—Among the difficulties of the static atom from a chemical point of view are the structure of the nitrogen molecule, the formation of cis- and trans-compounds in the ethylenic series, and the apparent gradual transition from a polar or ionised to an unionised compound. These can be overcome by making simple assumptions which still await experimental verification.

The phenomena of the photo-electric effect, the specific heat of the alkali metals, and the concept of the critical energy increment in chemical reactions all indicate that in an assemblage of apparently like molecules at any temperature above 0° K molecules differ from one another in reactivity. This difference may be attributed to an alteration in the position of one of the octet or valency electrons relatively to the nucleus. This should be accompanied by a change in size and possibly in specific heat.

The energy of activation is absorbed kinetically and stored in quanta potentially. That the inverse square law is found to hold good even to sub-atomic distances may be explained on the assumption that the atoms are static except during actual absorption or emission of energy, and act as small dipoles or magnets which orientate themselves during the passage of a charged body, electron or alpha particle, through their midst. All collisions therefore occur between the charged body and identical portions of the atoms where the inverse square law holds. That the variable force suggested by Sir J. J. Thomson and G. N. Lewis is limited in direction to a tube from nucleus to electron, *i.e.* a radial force, is further supported by the fact that in the absorption or emission of energy the electron must rotate or oscillate. That it oscillates rather than rotates is indicated by the experiments of Rutherford, showing that the inverse square law holds over large regions of the atomic volume, and by the empirical relationship of Haber between the natural infra-red and ultra-violet atomic frequencies $M\nu^2 = m\nu_1^2$, where M and m are the nuclear and electronic masses respectively, a relationship comparable with the assumption of a spring vibrating with loads of unequal mass at each end.

DISCUSSION ON THE QUANTUM THEORY.

Mr. C. G. Darwin.—In several branches of physics, the deductions from the principles of classical dynamics lead to results which are not borne out by experiment, and sometimes to results which would be quite impossible. In these cases the quantum theory supplies a working rule which has an extraordinary power of giving the right results. As a theory it has no complete logical foundation at present. The facts explained by it fall into two groups. On the one hand there are the photoelectric effect and the theory of spectra, and on the other radiation and specific heats, which involve considerations of the meaning of temperature.

The essential feature of the theory is the existence of a universal constant, the quantum $h=6{\cdot}55\times10^{-27}$ erg sec., which in some way, not yet explained, controls exchanges of energy. The simplest form of the rule is that if energy is exchanged with a system of frequency ν vibrations per second, then it will be exchanged in amount $h\nu$. Its application is at present only known for periodic systems. The photoelectric effect is the simplest case. Here light falls on a metal surface and in the act electrons are emitted with a high velocity. Their energy is connected with the frequency of the light by the quantum relation. The same effect enormously enhanced is found with the X-rays, and here the converse effect is also found—that electrons of given energy can only excite X-rays of frequency below a certain amount.

It was in the radiation theory that Planck discovered the quantum. It works in exactly the same way, though here complicated by the conception of temperature. It was in this connection that Poincaré proved that anything even remotely resembling the facts of radiation could only be explained by precisely Planck's ideas. The theory of the specific heats of solids is more complicated than that of radiation, but follows the same lines. For the specific heats of gases it is rather different, as here it is necessary to 'quantise' rotations instead of vibrations. In connection with radiation, Planck attempted partially to reconcile the new mechanics with the old by his Second Hypothesis, in which the absorption is continuous but the emission still discontinuous. This hypothesis raises a good deal of theoretical difficulty, and will not work in spectrum theory, but gives good results in other directions. Closely connected with it is the question of residual energy at the absolute zero of temperature, a matter that it should be possible soon to decide by experiment.

The spectrum theory is far the most interesting branch of the quantum theory, as it has led and is still leading to extensions of quantum mechanics. The first idea in it is a natural extension of the photoelectric effect. Bohr argued that if an atom emits radiation of frequency ν, it must be because it has lost energy $h\nu$, and this explains the Combination Law, which is that the frequencies of the lines of a spectrum are given by the differences between pairs of terms of a sequence. Thus, it is possible to replace the study of the line by a study of the energy of the atom before and after the emission. But a further use of the quantum is required, since on classical principles the nuclear atom lacks definiteness. This is done by 'quantising the orbits'—that is, limiting the number of orbits possible dynamically, by imposing non-dynamical conditions. The dynamical solution of the motion of a system of n degrees of freedom involves $2n$ arbitrary constants, and the quantisation consists in fixing half of these by a certain rule in terms of the quantum, and these are the only *permissible* orbits. For example, the hydrogen spectrum is due to a single electron describing a planetary orbit about a nucleus. The permissible orbits are such as have their major axis 1, 4, 9 . . . times $0{\cdot}53\times10^{-8}$ cm. in length, and eccentricities of certain definite values. In connection with permissible orbits an interesting point arises. In some cases (of which the above is one) the quantisation can be done in several ways and each leads to a different set of permissible orbits, but the energy of each has the same set of values and so they give the same spectrum. These are known as degenerate cases and are one of the most important points of difficulty in the theory. So far the Bohr theory has been successfully applied to the ordinary hydrogen spectrum, including its fine structure, the influence of electric and magnetic fields and partly the banded spectrum, too; also to the enhanced helium spectrum and to the X-ray spectra, and a beginning has been made on the alkali spectra.

Bohr has recently succeeded in extending the theory by his Principle of Analogy to the question of the intensity and polarisation of the lines. This reduces the character of a spectrum by means of a formal analogy with the (fallacious) predictions of the electro magnetic theory. Its physical meaning is hard to see, but it seems the sort of

hypothesis which should ultimately fit in with the final reconciliation of the two mechanics.

The complete justification of the necessity for a quantum theory is a fairly difficult matter. Attempts are sometimes made to devise mechanical models which would account for phenomena without it. These *ad hoc* models are invariably complicated, and if they existed would entail other consequences contrary to the facts.

Possible chances of development might lie in the direction of discovering a method of quantising non-periodic processes, or of deciding the real meaning of frequency in the quantum relations. Another point of great interest is in the Zeeman effect. Here, the result of quantum theory is identical with that from classical mechanics, and an inspection of the quantum equation shows that the quantum occurs on both sides and so divides out. This suggests that many phenomena which at present are thought to be satisfactorily explained by dynamics are really quantum phenomena, but that the quantum has divided out from the equations.

Another interesting point connects the quantum with Weyl's extension of relativity theory. In relativity the property of rigidity plays a certain part, and in the most recent developments of the theory this rigidity is typified in the expression 'radius of the electron.' The quantum theory of spectra suggests that there is a much truer measure of this characteristic, and that is the radius of the permissible orbit which an electron describes about the nucleus.

Sir Oliver Lodge, F.R.S.—If a projected electron enters a Bohr atomic system and is retained, the energy gained by the system, which must be re-radiated before a return to equilibrium, is $\frac{1}{2}mu^2$, where u is the velocity from infinity. Converted into an orbital velocity, $v=u/\sqrt{2}$, this becomes mv^2 and is associated with an angular momentum mvr. Writing this as $h/2\pi$, and v as $2\pi r\nu$, where ν is the frequency belonging to the orbit in question, the energy gained and requiring emission is $h\nu$. You can therefore have multiples of it but no fractions.

Now by Kepler's third law applied to the case $rv^2 = Ne^2/m$, where N is Moseley's atomic number for the bombarded atom and Ne the charge of its kernel; so that this quantity rv^2 is constant for all the rings of any given atom, and changes by unit steps for atoms higher or lower in the series.

But the bombarded atom has stepped down one in the series, by accretion of an extra electron in its kernel, so that whereas the first equilibrium condition of its sheath was

$$r_1v_1^2 = Ne^2/m,$$

its second equilibrium condition is

$$r_2v_2^2 = (N-1)e^2/m;$$

and the energy that must have been radiated to attain this second condition is

$$\tfrac{1}{2}m(v_1^2 - v_2^2),$$

or, what is the same thing,

$$\tfrac{1}{2}e^2\left\{N\left(\frac{1}{r_1}-\frac{1}{r_2}\right)+\frac{1}{r_2}\right\}.$$

This therefore is what can be equated to $h\nu$.

Moreover, since rv^2 is constant, and mrv is some multiple of h, say $nh/2\pi$ where n is a whole number, it follows that the different orbital radii in a single atom proceed as the squares of the number n; so that, properly replacing r_1 by n^2 and r_2 by $(n+1)^2$ in the above expression, we get from it the Balmer and other series,—Rydberg's constant and all—except for a small correction which becomes less significant as N is large.

The process of conversion from one stable circular orbit to another one, by reason of the smaller attraction of the nucleus suddenly weakened by the incoming electron, can be traced dynamically through an ellipse—to the varying speed in which the radiation must be supposed due. The speed of every sheath electron is suddenly too big for the weakened nucleus to hold in a circular orbit; hence the ellipse, and the readjustment. (See *Journ. Roy. Inst.*, Appendix I. to report of lecture on 'Ether and Matter,' Feb. 28, 1919.)

If it be objected that the gain of an electron has on this view precipitated an atom a step down the series, so that it would subsequently emit a different spectrum, the reply is that the percentage of converted atoms is excessively small and that the proportion of converted material may be inaccessible to observation.

Sir JOSEPH LARMOR, F.R.S.—Sir Oliver Lodge communicated two notes by Sir J. Larmor, entitled 'Escapements and Quanta' and 'Non-Radiating Atoms,' which were subsequently published in *Phil. Mag.*, Oct. 1921.

Dr. H. S. ALLEN.—In his work on the magneton, the late Prof. S. B. McLaren found that the angular momentum of the system is proportional to the number of tubes of electric induction terminating on the surface and to the number of tubes of magnetic induction linked through its aperture. According to the quantum theory the angular momentum is $nh/2\pi$, where n is an integer and h is Planck's constant. If we identify the two expressions for the angular momentum, and regard the charge of the magneton as equal to the electron charge, e, we find that the number of tubes of magnetic induction is equal to n (h/e). This suggests that the ratio of h to e defines the fundamental unit magnetic tube, and on substituting numerical values it appears that one C.G.S. magnetic tube (one 'Maxwell') contains 2·43 million 'quantum tubes.' Such an electrodynamic interpretation of Planck's constant has been given by A. L. Bernoulli on the assumption that an electron is moving in an orbit in a uniform molecular magnetic field. In a paper read before the Royal Society of Edinburgh in November 1920, I showed that, without this last restriction, when any number of point charges are revolving round an axis with a common angular velocity, the number of magnetic tubes passing through the stationary circular orbits is equal to an integer, n, multiplied by the constant factor h/e. Recently I have shown that when an electron revolves round the positive nucleus in an elliptic orbit, in which case the size and shape of the ellipse depend upon *two* integers n and n^1, the sum of these integers represents the number of quantum magnetic tubes passing through the elliptic orbit. The result may be generalised as follows:—The restrictions imposed by the Quantum Theory on the stationary states of a dynamical system require that when 'separation of the variables' can be effected, the mean value of the kinetic energy corresponding to a particular degree of freedom is equal to $\frac{1}{2}nh\nu$, where the mean value is taken over the period, $1/\nu$, corresponding to the co-ordinate under consideration. Let this mean energy be identified with the mean electrokinetic energy $\frac{1}{2}Ne\nu$, arising from the periodic motion of an electric charge e with frequency ν. Then N, the number of magnetic tubes associated with the moving charge, is given by $n(h/e)$. These results indicate the existence of discrete tubes of magnetic induction as suggested long ago by Faraday. We may, in fact, regard a unit tube of magnetic induction as one quantum.

Prof. WILLIAM WILSON.—The type of quantum theory which has been most successful—especially in its application to spectra—is based on the following hypotheses:—

1. Interchanges of energy between physical systems are discontinuous in character. That is to say, each system behaves normally in a conservative way. It is then said to be in one of its stationary states. It can only pass from one stationary state to another abruptly with the emission or absorption of a corresponding amount of energy.
2. The motion of a system in one of its stationary states is subject to Hamiltonian dynamics (with relativistic extensions).
3. If the positional and impulse co-ordinates be denoted by q_i and p_i, then in the important cases the co-ordinates can be so chosen that each q_i librates between fixed limits q_{i1} and q_{i2} and each p_i is a function of q_i only.

The fundamental hypothesis of the quantum theory lays down that each integral

$$\text{(1)} \qquad I_i = \int p_i dq_i = \tau_i h$$

where the integration is from $q_i = q_{i1}$ to $q_i = q_{i2}$ and back again. The number τ_i is a positive integer or zero and h is Planck's constant.

The energy of the system can be expressed in terms of the I_i. For instance, in the case where the system is simple harmonic with only one q, we find the energy expressed by

$$\text{(2)} \qquad E = I\nu = \tau h\nu,$$

where ν is the frequency of its motion. In the case of an electron revolving round a positively charged nucleus we have

$$\text{(3)} \qquad E = -\frac{2\pi^2 me^4}{(I_1 + I_2)^2} = -\frac{2\pi^2 me^4}{(\tau_1 + \tau_2)^2 h^2},$$

where we have supposed the mass of the nucleus large compared with that of the electron and where further the charge on the nucleus is numerically that of the electron. A hydrogen atom, according to Rutherford and Bohr, is a system of this latter kind.

An important additional hypothesis is employed in connection with radiation. If, in a transition from one stationary state to another, a system emits radiation, the frequency ν of this radiation is given by equating the emitted energy to $h\nu$. This hypothesis is due to Bohr, who succeeded in deducing Balmer's and similar series emitted by hydrogen by applying this hypothesis in connection with (3) given above.

This gives for the Rydberg constant

$$\text{(4)} \qquad N=\frac{2\pi^2 me^4}{h^3}.$$

Equation (3) requires a simple modification in consequence of the fact that mass of the nucleus is finite, and a further modification due to Sommerfeld takes account of the relativistic dependence of the mass of the electron on its velocity, the kinetic energy being equated to

$$\text{(5)} \qquad \mu c^2(\gamma-1),$$

where μ is the mass of the electron for small velocities, and $\gamma=(1-v^2/c^2)^{-\frac{1}{2}}$, v being the velocity of the electron. Sommerfeld has shown that the modification of (3) which is thus introduced is just what is required to account for the fine structure of the lines in Balmer's series.

[The hydrogen atom, without the relativistic modification of its motion, furnishes an example of what is called a degenerate system in which the number of fundamental frequencies is smaller than the number of co-ordinates q_i.]

Epstein, by employing the classical dynamics of Hamilton and Jacobi for the stationary states of a hydrogen atom in the presence of a uniform external electrostatic field, and the principles of the quantum theory as given above, has succeeded in explaining the resolution of the hydrogen lines observed by Stark.

For transitions between stationary states for which the changes in the integers τ are small by comparison with their initial and final values, the quantum theory approaches asymptotically to the ordinary theory and in particular the frequencies of the emitted radiation are, in such a case, what would be given by ordinary electrodynamics. Bohr and his pupil Kramers have recently made use of this feature of the quantum theory, by a sort of process of extrapolation, to get information about the intensity and polarisation of spectral lines. Notwithstanding their success in this direction, this process of extrapolation (Bohr's correspondence principle) can only be regarded as provisional in character.

The evidence for the existence of stationary states is not confined to spectroscopic observations. Such experimental investigations as those of Franck and Hertz, Richardson and Bazzoni and Davis and Goucher furnish additional evidence of the most striking kind.

These states then have to be reckoned with as physical facts, and when, as in the case of atoms, they involve the revolution of electrons round charged centres, we are confronted with what is doubtless the chief of the difficulties besetting the quantum theory—namely, that the electromagnetic theory requires continuous radiation of energy from such a system. Probably this and some other difficulties will be surmounted by a suitable alternative for the four Maxwell-Lorentz equations

$$\text{(6)} \qquad F^{mn}_{n}=J^m.$$

These, I think, can only be true in a macroscopic sense. The remaining field equations are not in any obvious way inconsistent with the features of the stationary states of the quantum theory.

Prof. J. C. McLennan, F.R.S.—In applying to hydrogen his theory of the fine structure of spectral lines, developed by extending Bohr's ideas of the origin of radiations founded on the quantum theory and incorporating with them the principle of relativity, Sommerfeld has shown that each member of the doublet H_α should consist of a close triplet, each member of H_β of a

close quartette, each member of H_γ of a close quintette, etc. He has shown, moreover, that his theory lends itself to the precise calculation of the intensities of these constituents of the members of the doublets. Sommerfeld has found, too, that, theoretically, the magnitude of the doublet separation should be constant for all members of the Balmer series, and equal to 0.365 cm.$^{-1}$. In the case of H_α and H_β and of H_γ, the calculated distribution and intensities of the fine structural components is such that in actual determinations of the doublet separations values less than 0.365 cm.$^{-1}$ should be obtained. For H_δ, and the higher members of the series, effects connected with the fine structure of the components of the doublets should be less in evidence. It follows, therefore, that in proceeding from H_α to the higher members of the Balmer series we should expect on the basis of Sommerfeld's theory to obtain for the doublet separations values that rapidly increased up to 0.365 cm.$^{-1}$ for H_δ and then remain constant for the remainder of the series. A direct test of Sommerfeld's theory through an examination of the structure of the doublets of the Balmer series of hydrogen is necessarily attended with considerable difficulty. With atoms so light as those of hydrogen the Doppler effect arising from molecular thermal agitation is considerable at ordinary temperatures. As a result the members of the doublets cannot ordinarily be obtained as sharp lines, but as broad and more or less diffuse bands. This diffuseness is, however, enhanced by the Stark effect which always exists to a greater or less extent when the emission of radiation is brought about by electrical stimulation. What has been taken to be a remarkable confirmation of the validity of Sommerfeld's theory has been obtained by Paschen through a study of the structure of spectral lines belonging to series originating in the Helium univalent ion. With this element the Doppler effect is less marked than with hydrogen, and although the nuclear electric charge for Helium atoms is twice as great as that for atoms of hydrogen the lines of the spectrum of Helium are less influenced than those of hydrogen by the Stark effect, and on that account are sharper.

In summing up the results of Paschen's observations, Sommerfeld has reached the conclusion that, qualitatively and quantitatively, they constitute a definite and strong confirmation of his theory. A further confirmation is found in the fact that the series of the Röntgen spectra of the elements consists of doublets with a constant separation between the components of approximately 0.365 cm.$^{-1}$.

In discussing Sommerfeld's theory and its supposed confirmation by Paschen, Stark has pointed out that a vital characteristic of the theory lies in its quantitative features. He has drawn attention in particular to Paschen's observations on the Helium line $\lambda = 4686$ Å. U., and has emphasised the latter's failure to find three components whose presence was demanded by the theory, and to his observation of a component whose presence was not predicted by it. Stark also makes a point of the fact that the observed relative intensities of the components of $\lambda = 4686$ Å. U. do not agree with the values calculated by Sommerfeld. Moreover, he lays particular stress on the fact that while Sommerfeld's theory indicates that the doublet separations in the Balmer series of the hydrogen spectrum should gradually increase in passing from H_α to H_δ, the results of Gehrcke and Lau taken as they stand show doublet separations gradually decreasing in magnitude as we pass from the first to the fourth member of the series.

From the above it will be seen that while strong confirmation of Sommerfeld's theory has been obtained from Paschen's investigation of the structure of a number of wave-lengths in the spectrum of Helium, and from an important characteristic of the L. series in the Röntgen spectra of the elements it is highly desirable to have the validity of the theory tested directly by making accurate determinations of the doublet separations of as many as possible of the members of the Balmer series of hydrogen.

In some experiments made at Toronto, measurements were made of the separations of the doublets H_α, H_β, H_γ, and H_δ.

In determining the separation of the components of the different wave-lengths the plates were measured up with a Hilger photo measuring micrometer, and readings were taken with it at the edges as well as at the centres of the interference bands. The mean values of the separation of the components of the four doublets H_α, H_β, H_γ, and H_δ, taken from centre to centre are given in the Table below. The results of Merton and of Gehrcke and Lau are also given in the table. It will be seen that the separation gradually decreased from 0.154 Å.U. for H_α, to 0.049 Å.U. for H_δ, while the corresponding frequency differences dropped from 0.36 cm.$^{-1}$ to 0.29 cm.$^{-1}$.

TABLE.

Line Wave Length	Separation of Components					
	Merton		Gehrcke and Lau		McLennan	
Hα 6563 Å.U.	·145 Å.U.	·34 cm.$^{-1}$	·126 Å.U.	·29 cm.$^{-1}$	·154 Å.U.	·36 cm.$^{-1}$
Hβ 4861 „	·093 „	·39 „	·070 „	·29 „	·085 „	·36 „
Hγ 4341 „			·058 „	·31 „	·062 „	·33 „
Hδ 4101 „			·043 „	·26 „	·049 „	·29 „

The results may be plotted with the wave-length differences as ordinates, and the squares of the wave-lengths as abscissæ. Had the frequency difference for the components of the four wave-lengths been constant and equal to 0·365 cm.$^{-1}$, the values of $\Delta\lambda$ would have registered more or less closely with a straight line through the origin. With the results obtained, however, for the separations of the doublet components, the values of $\Delta\lambda$ lay close to a curve which when extended cut the zero ordinate line at approximately 0.133×10^{8} cm.^{2}, which meant that the results pointed to a separation of the doublets that vanished at $\lambda = 3648$ Å.U, the limiting wave-length of the Balmer series. Should this result turn out to be correct it would show that the Balmer series of hydrogen should be classified as a principal rather than as a subordinate series and that the theory put forward by Sommerfeld is inadequate.

Dr. Irving Langmuir.—Dr. Darwin has objected to the theories that have attempted to explain the quantum phenomena because such theories seem to necessitate electrons that have structures more complicated than clocks. Surely, however, we must look for a mechanism underlying the quantum theory, and it seems impossible, at least to the Anglo-Saxon mind, to believe that it should depend ultimately upon a structure of energy or upon such integral equations as are used in determining the stationary orbits in Bohr's theory. If we are to have a mechanism at all it appears logical to look for it within the electron itself, even if this does lead us at first to unpleasant complications in our conception of the electron.

Bohr speaks of a uniquantic or diquantic orbit of an electron, but seems to consider that the electron itself does not change in passing from one orbit to another. Is it not more logical to think of the orbit as resulting from the properties of the electron and to consider that quantum changes occurring in radiation cause discontinuous changes in the structure of the electron? The properties of isotopes indicate that we may disregard the structure of the nucleus and need only consider its total electric charge.

The striking experimental verification of Bohr's theory, especially in the fine structure of the lines of the Balmer series, seems to prove almost without possibility of doubt that the mechanical model proposed must be substantially correct. Nevertheless, we should learn from the apparently irreconcilable conflict between the wave theory and the quantum theory of radiation that in the present state of science it is not safe to draw such definite conclusions regarding the ultimate mechanism. The facts underlying the periodic table of the elements prove that the laws governing the arrangement of electrons in atoms are essentially simple, although secondary complications exist much as in the case of the gas laws. This inherent simplicity argues strongly for a static arrangement of the electrons. It seems almost impossible that this simplicity could result from the orbital motion of such numbers of electrons as exist in atoms. It is therefore worth considering whether the simple results of Bohr's theory can be obtained from any reasonable assumptions regarding the properties of electrons in a static atom.

As a simple mathematical analysis shows (Langmuir, *Science*, 53, 290 (1921), we obtain Bohr's equation for the frequency corresponding to the lines of the Balmer series if we assume that there are two forces acting between an electron and a nucleus of charge N. The first force is the ordinary Coulomb inverse square law of force. The second force which we may call the "quantum force" is a repulsive force equal to

$$F_q = \frac{1}{mr^3}\left(\frac{nh}{2\pi}\right)^2$$

where m is the mass of the electron, r is the distance between the electron and the nucleus, h is the quantum constant and n is an integer which expresses the quantum state of the electron. It should be noted that this expression does not contain N the charge on the nucleus, nor e the charge on the electron. A free electron (not bound in an atom), for which n is zero, has no quantum force and thus acts strictly in accord with Coulomb's law. But when the electron becomes modified by undergoing quantum changes, resulting from the emission of radiation, it is not able to come indefinitely near to the nucleus, but tends to reach a definite position of equilibrium. The total potential energy of the atom in each of these positions (corresponding to the different values of n) is the same as the total energy of the atom in Bohr's theory. The distance between the nucleus and the electron in its equilibrium position is the same as the radius of the orbit of the electron in Bohr's theory. Finally, the period of oscillation of the electron about its position of equilibrium is the same as the period of revolution of the electron in its orbit about the nucleus in Bohr's theory.

This represents merely an attempt to interpret the quantitative results of Bohr's theory on the basis of a static arrangement of electrons. Without further assumptions regarding the properties of the electrons, it is not possible to account for the fine structure of the lines. Nevertheless, it seems useful to pursue this line of reasoning to other atoms and molecules, particularly to construct models which will throw light on chemical relationships. By assuming an analogous (although not identical) quantum force between two electrons in the same atom or molecule, I have been able to devise models of the hydrogen molecule and helium atom which give the total energy more accurately than any of the other models that have been proposed. I am planning to study models of lithium and other simple elements in a similar manner. This theory seems to afford a very satisfactory explanation for the fact that only two electrons occur in the first shell of any of the atoms.

SCIENCE AND ETHICS.

By E. H. GRIFFITHS, *Sc.D., D.Sc., LL.D., F.R.S.*

Let us consider the dictionary definition of the two words I have chosen for a title.

'SCIENCE : Knowledge—the comprehension or understanding of truth or facts by the mind.' The dictionary adds : 'The science of God must be perfect.'

'ETHICS : A system of moral principles; a system of rules for regulating the manners and actions of men.'

If we agree with these definitions it must appear impossible to accept any code of ethics which ignores that 'comprehension and understanding of truth' which is the hall-mark of the scientific mind. Yet for centuries the disciples of what they termed ethics imprisoned, nay, tortured, the searchers after truth wherever they encountered them. Even to-day many regard ethics and natural science as antagonistic rather than as closely allied, and I believe that certain of our educational difficulties arise from this misapprehension.

I trust, therefore, that a necessarily brief and imperfect inquiry into the causes and effects of this imaginary antagonism may not be considered in this section as an unworthy subject for consideration.

The neglect of natural science by the British public has compelled those who believed in both its technical and ethical value to use, in their efforts to overcome the prevailing indifference, those arguments which would most strongly attract attention and arouse interest. Special emphasis, therefore, had to be laid on the *material* value of the applications of science and the benefits to industry which have had their origin in scientific research. This Association has, during the past ninety years, played a great part in the awakening of the public conscience, and the 'man in the street,' and, what is more surprising, the politician in his Cabinet, is beginning to realise that his prosperity, his security, his comfort, his health are in no small measure due to the achievements of the pioneers of science.

It is possible, however, that the advocates of natural science have, in their anxiety to secure support, unduly ignored the educational and ethical value of training in scientific method, and, as a consequence, its importance as one of the highest exercises of the human intelligence has been insufficiently appreciated by the public. In brief, natural science has been, and still is, regarded as *an* utilitarian and vocational, rather than as an ethical and educational subject.

When we read stories such as those of the persecutions of Galileo or Bruno, we thank God we live in more enlightened times. Have we, however, any right to be completely satisfied? Have we entirely freed ourselves from the superstitions and prejudices of the past?

I doubt if any man who dispassionately considers our present educational system can honestly answer in the affirmative. The burden of tradition is heavy upon us, and the more or less hidden hand of the expert in the obsolete still retards our educational progress.

In the Middle Ages, if we group men by their occupations—that is, if we separate them by vertical planes rather than by the horizontal ones indicating social conditions—we, broadly speaking, find three classes only :—

1. The men who fought;
2. The men who made things (including labourers as well as craftsmen);
3. The men who studied;

the last class being chiefly found within the monasteries.

Many men belonged to both the first and second classes, but very few to both the second and third. Hence arose the prevalent belief that he who *did* things could not possibly be a student, the natural corollary being that no student should *do* things, he should only read and talk about them. This view has been somewhat modified in recent times, but still, deep down in the minds of our teachers, with, of course, some brilliant exceptions, there remains the conviction that anything which is useful is probably non-educational.

I recently came across an interesting example. The following quotation is from the introduction to a scholarly book by Prof. Weekby, entitled 'Surnames,' which was published in 1916. Referring to his own work, he says : 'This may seem of little practical importance at a time when our leaders of science—a word which *used* to mean knowledge—are exhorting us in unattractive English to do away with this "old lumber of Greek and Latin," and bend all our efforts on transforming the rising generation into a nation of super-plumbers.'

Here the exasperating mental attitude to which I have made reference is very evident. It assumes that our only object in teaching science is to raise a 'super-plumber,' whatever that may be; and, according to the writer, it follows that what we teach him cannot, if he utilises it, be science in the true acceptation of that term.

It is unnecessary to multiply examples. Everyone conversant with public school masters, university dons, or school inspectors could multiply them without stint.

I now propose to set before you a hard task. I ask you to forget that discoveries in pure science have ever served an utilitarian or industrial purpose. Such a task must be a specially difficult one to members of an Association in which discoveries have been announced which have not only undoubtedly increased the industrial prosperity of this kingdom, but which also, but for the ignorance and indifference of our legislators concerning all scientific matters, would have removed many of the social evils which now disturb the welfare of the commonwealth.

In fact, I ask you, for the purposes of discussion with the superior persons to whom I have referred, to unite in the toast of a certain Society, 'Here's to science, pure and undefiled, and may it never be a ha'porth of use to anyone.'

Curiously enough, one may remark in passing, the spirit of that toast is precisely that in which every great discovery which has ultimately proved of service to mankind has been accomplished. Newton, when enunciating the law of inverse squares; Faraday, when investigating the phenomena of electro-magnetic induction; Maxwell, when establishing the theory that light was an electro-magnetic disturbance, were not thinking of the 'super-plumber' or his works.

Let us now, 'without prejudice,' as the lawyers say, stand for a time on this imaginary platform of the uselessness, for all practical purposes, of natural science, and consider from that standpoint the necessity of its study as a portion of a liberal and ethical education.

I am anxious at the outset emphatically to disclaim any desire to belittle or disparage the studies which are ordinarily classed under the title of the 'humanities.' It will be an evil day for us when we cease to appreciate their value, but we should, it appears to me, also claim the *equality* of science with the sister faculties for the purpose of the training of the mind and the enrichment of character. I, for one, do not want Cinderella to be made a princess and rule over her sisters; I do not even ask that she should entirely forsake the kitchen, for the kitchen should be a home of science; but I do desire that she should be regarded, mentally and socially, as equal to her sisters who discourse on art and literature in the drawing-room.

To quote Sir Ray Lankester :—

'We believe in the great importance of science and the scientific method not merely for the advancement of the material well-being of the community, but as essential to the true development of the human mind and spirit. It is only by early training in the natural sciences that a true outlook on the facts of existence can be secured. It is only by them that the supreme value of accuracy of thought and word and the supreme duty of intellectual veracity can be learned. In no other way can that complete independence of judgment in moral, as well as in intellectual, subjects be established and justified in those who faithfully adhere to them.'

Faraday wrote : 'I do think that the study of natural science is so glorious a school for the mind . . . that there cannot be a better school for education.'

These passages admirably express the views of those who urge the ethical and educational value of natural science.

To me it appears an extraordinary thing that our present educational system is based on a study of the works of man rather than on those of the Creator.

It is strange (to quote Sir Napier Shaw) that so much attention should be concentrated 'on the failings and foibles of the human side of nature, so little about the majestic and inexorable laws of the physical side.'

In a recent letter to *The Times* I saw it stated that 'in the vital element of education—the formation of character—natural science is of little or no value,' and I am afraid some would go even further and regard it as detrimental. The point of view of certain such objectors is well illustrated by the letter of a parent to a University Lecturer :—

'Sir,—I hereby give you notice that I do not want my girl Sally taught anything about her inside. It does her no good and to my mind it is very rude.'

There you have it in a nutshell. Biology is too 'rude' a science to be taught to the young.

Others honestly believe that such studies tend to the formation of an intellectual independence, to the habit of forming a judgment on the evidence alone, uncontrolled by tradition or authority, which, at all events from the theological point of view, is undesirable. If our masters and pastors state that the sun revolves round the earth, it is flat heresy to maintain or produce evidence to the contrary. This perverted idea of what is really meant by education is, I believe, the origin of many of our difficulties. It dies hard, it persists to the present day, and it is to be found in the most unexpected quarters.

The best reply to this kind of nonsense is to be found in the following passage from Tyndall :—

'The business of the students of science is not with the possible but with the actual, not with the world which *might be* but with the world which *is*. This they explore with a courage not unmixed with reverence, and according to methods which, like the qualities of a tree, are tested by their fruits. They have but one desire—to know the truth; they have but one fear—to believe a lie; and if they know the strength of science and rely upon it with unswerving trust, they also know the limits beyond which science ceases to be strong. They best know that questions offer themselves to thought which science as now presented has not even the tendency to solve. They keep such

questions open and will not tolerate any unnecessary limitation of the horizon of their souls. They have as little fellowship with the atheist who says there is no God as with the theist who professes to know the mind of God. "Two things," said Kant, "fill me with awe, the starry heavens and the sense of moral responsibility in man," and in his hours of health and strength and sanity, when the stroke of action has ceased and the pause of reflection has set in, the scientific investigator finds himself overshadowed with the same awe.'

The study of natural science *is* the study of truth. The laboratory, though at times reluctant and elusive, never tells a lie, and every research in pure science is a prayer for a revelation.

I admit that character is the product of home life, of the playground, and —under wise guidance—of contact with one's fellows, rather than of the classroom. All I claim in this respect is that in one all-important matter, the cultivation of a desire for truth and intellectual honesty, the study of natural science is—amongst all studies—pre-eminent.

No thought can be more encouraging to the student of science than that some time, it may be long after his time, his labours will be productive of benefit to succeeding generations. This idea is well expressed by the following fine passage in Buckle's 'History of Civilisation':—

'The actions of bad men produce only temporary evil; the actions of good men only temporary good; and eventually the good and the evil altogether subside, are neutralised by subsequent generations, absorbed by the incessant movement of future ages. But the discoveries of great men never leave us; they are immortal, they contain those eternal truths which survive the shock of empires, outlive the struggles of rival creeds, and witness the decay of successive religions. All these have their different measures and their different standards; one set of opinions for one age, another set for another. The discoveries of genius alone remain; it is to them that we owe all that we now have; they are for all ages and all times; they are essentially cumulative, and giving birth to the additions which they subsequently receive, they thus influence the most distant posterity, and after the lapse of centuries produce more effect than at the moment of their promulgation.'

Another charge that has been made, and wrongly made, is that the study of science has a cramping effect; that it tends to restrict the sympathies and limit the use of the imagination, one of the greatest gifts to man. This accusation has been well dealt with by Tyndall in that delightful work 'The Scientific Use of the Imagination,' from which I give the following extract:—

'There are Tories in science who regard the imagination as a faculty to be feared and avoided rather than employed; they have observed its actions in weak vessels, and were unduly impressed by its disasters; but they might with equal justice point to exploded boilers as an argument against the use of steam. Nourished by knowledge patiently won, bounded and conditioned by co-operant reason, imagination becomes the mightiest instrument of the physical discoverer. Newton's passage from a falling apple to a falling moon was at the outset a leap of the prepared imagination.'

Also the following passage from an address to the Royal Society by Sir Benjamin Brodie in 1859:—

'Lastly, physical investigation, more than anything besides, helps to teach us the actual value and right use of imagination, that wonderful faculty which, left to ramble uncontrolled, leads us to stray into the wilderness of perplexity and error, a land of mist and shadows; but which, properly controlled by experience and reflection, becomes the noblest attribute of man, the source of poetic genius, the instrument of discovery in science, without the aid of which Newton would never have invented Fluxions, nor Davy have decomposed the earth's alkalies, nor Columbus have found a new continent.'

If these things be true, again I ask, are we not justified in claiming the study of natural science as one of the highest forms of intellectual effort?

The effects of discoveries in natural science extend far beyond its own bounds. Reflect on the whole change in the mental outlook consequent on the establishment of the law of gravitation and the discoveries of the geologist. There is no branch of intellectual effort which has not been quickened and invigorated by the great generalisations of Darwin.

One strong educational argument can be advanced—real knowledge of science cannot be obtained merely by absorption of print. The learner must exercise his powers of observation, and it is extraordinary how lacking in this respect, except in the playground, are the products of our public schools. It is fortunate that the enthusiasm of our lads for cricket and football *has* quickened their powers of perception, if only in this limited area. The lad who can rightly time and catch a ball in the long field has solved by his own direct method problems connected with the paths of projectiles, resistance of the air, etc., which long baffled us, and if we could induce him to carry the same qualities of close observation, rapid reasoning, and decisive action into other spheres of life many of our educational difficulties would vanish. Unfortunately, he has learned to place in separate compartments the knowledge he gains himself by his own activities and that which he derives from the medium of the printed page.

In this connection I confess there is urgent need of reform in our methods of teaching natural science.

We have some excuse for any lack of efficiency, for we have not, like our colleagues on the humanistic side, the experience of generations of teachers to guide us. Until recent years natural science has been taught as if it were a dead language. Nothing (as Sir Napier Shaw has pointed out) has been regarded of interest unless it could be utilised for the purposes of arithmetical computation. Our examination system has endeavoured (but, thank heaven! unsuccessfully) to kill the soul of science in the rising generation. There is, however, a stirring among the dry bones, and we are awakening to the fact that science must be taught as if we believed in it for its own sake, that we must preach it as a disciple preaches his religion, and that we must refuse to be bound by the fetters in which tradition has entangled us. If we are to succeed, we must make science a living reality to our pupils, and cease to regard it merely as convenient machinery for the manufacture of conundrums.

It is true that much of what I have said only applies to those who have shown real aptitude for the study of pure science and who have devoted their energies to its pursuit. How about the average citizen? the professional man? the merchant? the shopkeeper and the workman? I reply that every one of these should at all events be given an opportunity of learning something of the achievements of science, and also of acquiring knowledge which may give him something to think about besides earning his bread and butter. There are few who cannot profit by advancing a little distance along some one of the almost infinite variety of paths within the scientific boundary. The mind which finds congenial exercise in the straight path of mathematics, the one path which never bends backward or goes downhill, may be very differently constituted from that which delights to follow the winding ways of the botanist or zoologist. If there are some to whom all such paths are distasteful, I, for one, would deprecate any effort to force them thereon, but at all events let us bring them to the gates and find if they wish to enter, and so offer them not only freedom to choose, but also opportunities for appreciation.

The distinction between 'humanistic' and 'natural science' studies is, after all, an artificial one, for science is pre-eminently humanistic. It may take faith and intelligence to live *by* science, but to live *without* it is folly. If we, as a community or as individuals, disregard its laws we inevitably pay the penalty. 'Man cannot live by bread alone,' but he most certainly cannot live without it.

Strangely enough, when we advocate the training of a lad in the methods of science, an outcry is at once raised against 'too early specialisation,' whereas, for some mysterious reason, the early study of defunct languages is never regarded as such, although if that is not 'specialisation' I do not know the meaning of the word.

Think how every boy begins, almost as soon as he can toddle, to form a laboratory for himself. He collects bricks, cards, anything to build with. He delights in a toy that goes round, and generally wants to know why it does so. He is incessantly curious to ascertain the why and wherefore of everything, often to his elders' disquiet. The girl finds her laboratory in the doll and its house.

When the lad has learned to read and write, however, his energies are diverted, by those who guide him, to (say) the kings of England, in whom he has not the slightest interest, except when they fought. From the time he is settled in the preparatory, and later in a public school, his natural desire to learn something about his surroundings is, except in the playground, repressed. When he leaves his school, and afterwards his university, and arrives at what I may term the years of *in*discretion, most of his 'wanting to know' kind of feeling has been stamped out of him. He considers its exhibition as almost 'bad form.'

Again, I plead for early education in the methods of science as a means of increasing the usefulness of what I may term the average citizen. The man who devotes his leisure hours to work which is included under the term 'social service'—it is good to reflect that the number of such men is increasing—and who desires to raise the status of our working men, or to alleviate the poverty and unhealthy conditions to which such a large fraction of our people is condemned, must, if his efforts are to have any success, be at all events acquainted with the rudiments of sanitary science. He should also be able to understand the nature of the industries in which his neighbours are engaged and have some idea of the scientific principles on which those industries are founded. Philanthropic efforts by zealous men ignorant of such principles may not only be unsuccessful but may do positive injury to those whom they desire to benefit. The guardians of the poor, the town or county councillors, and more especially the members of Parliament, would do far more good and much less mischief if their early education had given them some idea of the root causes which have brought it to pass that—to quote Professor Perry—'all the conditions of civilisation are being transformed.'

Our urgent need is to create an atmosphere in which the growth of natural knowledge may be quickened, rather than to increase the number of what I may term professional scientific men. We have, I believe, under present conditions, a sufficiency of such men. Our universities, faulty as they may be in many respects, are turning out larger numbers of eager students more or less suitably equipped. The difficulty is that they receive little encouragement, financial or otherwise, and therefore drift into employments in which their qualifications are of little use. I have known of many such men, eating their hearts out, in the—to them—uncongenial employment of teaching in secondary schools, with little in the way of promotion to look forward to.

The scientific ability which this country possesses is being largely wasted for want of encouragement.

If I were asked 'How would you create the genial atmosphere you desire?' my reply would be—I would take the first step in our primary schools. Let there be one half-hour per week in which the teacher talks to the children and tells them something of what science has done for them. If, for example, he knows that an operation has been successfully performed on some child's relative, let him make that a text for a chat about Lister's work. If the school is in a colliery district, let him give a tale or two about Humphry Davy. If many of the parents are employed on tramways or electrical works, let him say something about the history of the generation of electricity by machinery, with a story of the childhood of Faraday. Such matters in capable hands could be made as interesting as a fairy tale. If, as some wrongly say, we cannot successfully teach these children natural science, anyhow we can bring it to pass that when they leave the schoolroom they shall have some idea of what science has done for them. The consequent change in the public attitude might be slow, but I am confident that in time it would be considerable.

Such courses would have, I believe, other important consequences. They would make the working man more ready in later life to learn something concerning the origin and nature of the industries by which he earns his daily bread.

Over certain great engineering works in the United States the following sentence is inscribed: 'No work should be done by a man which can be done by a machine.' There is truth, but there is also danger in this statement. The man who has unintelligently to serve a machine, who only knows that he has to pull this handle to do one thing and push that handle to do another, tends himself to become a machine, possibly an explosive one.

Working men of former generations had to use their brains as well as their hands. Their interest and pride in their work kept them from mental stagna-

tion, and their wages depended on their skill. Our utilisation of machinery is causing that class to be a diminishing one, although samples still remain. Take the village smith, for example. He has to meet all kinds of unexpected situations—to repair an agricultural machine, to set a kitchen boiler in order, to patch up a bicycle, to shoe a refractory horse; and I believe that, in consequence, he is not only the most intelligent but also the happiest man in his district. The men in the motor-repairing shops scattered about the country are of the same type, and in increasing the numbers of such workers, who have to give intelligence to their tasks, the motor-car has rendered real national service.

Nevertheless, the whole tendency of modern industry is to deprive the labourer of all initiative or interest in his work. Can we wonder if he yields to any temptation which may introduce some human variety into his life? Give these men some knowledge of the machinery that they use; an idea of the manner in which it came into being, and the laws which govern its actions; give them something to think about when at work; in fact, make them feel that although they cannot, like the skilled engineer, be the masters, they are not the slaves of their machines, and you will raise a generation of more intelligent, more contented, and, therefore, more valuable citizens.

Lastly, I plead for a greater diffusion of natural knowledge, for the reason—an ethical reason and in itself a sufficient reason—that it would bestow increased happiness and loftier aspirations on mankind. The world he lives in would become to every man a place of increasing and widening interest as his knowledge expanded. Familiarity with nature never 'breeds contempt,' or if it does so I am afraid it is only in our relations with our fellow men.

'There need not schools, nor the Professor's chair,
Though these be good, true wisdom to impart;
He, who has not enough for these to spare
Of time or gold, may yet amend his heart,
And teach his soul, by brooks and rivers fair:
Nature is always wise in every part.'[1]

Those who, like myself, have little acquaintance with the biological sciences must feel that the walks abroad of the botanist and zoologist are to them a source of pleasure to which we are strangers. To Peter—

'A primrose by a river's brim,
A yellow primrose was to him,
And it was nothing more,'

but to the artist it is a thing of beauty, while to the artist who is also a botanist it must be more—a cause of wonder.

Objects from which some of us naturally shrink—objects of horror—creeping things 'that crawl with legs upon a slimy sea'—are to the zoologist, acquainted with their history and variety, a source of both interest and delight.

The admiration of the anatomist for the structure and beauty of the human body must increase with every organ he dissects.

A lump of coal, to the stoker merely 'something to burn,' is to the chemist a casket from which can be extracted many products useful to man, and to the geologist it is an historical document.

To the mineralogist a crystal is one of the most wonderful and, he has reason to believe, one of the most significant, of all structures.

To the physicist every block of apparently inert matter tells wondrous stories. As he reflects on its ultimate atoms, their infinite number and the forces binding them all together; as with increasing knowledge he considers these almost inconceivably minute entities as in themselves miniature solar systems containing hitherto unsuspected stores of energy, energy beside which that of our coalfields sinks into insignificance, is it strange that, having passed through the first stage of wonder which, as Coleridge says, is 'the child of ignorance,' he should have attained the second stage at which wonder becomes the 'parent of adoration'?

[1] Lord Thurlow.

If the spectacle of the starry heavens impresses even the most casual observer with a sense of awe, that awe will be increased, not diminished, if he knows something of the tremendous distances of the stars, their ordered procession, and learns from the message brought by their light something of their composition and stage of development when that message left them, possibly centuries ago.

The deeper we probe, the greater the wonder and mystery of it all, and the more knowledge we acquire the more earnest must be our desire that man may, in the ages to come, prove himself worthy of the universe in which he has been placed, a desire which is truly ethical. Science and ethics are indissoluble. Their union is admirably set forth by Bacon in the noble and well-known passage :

'Knowledge is not a couch for the curious spirit, nor a terrace for the wondering, nor a tower of state for the proud mind, nor a shop for profit and sale, but a storehouse for the Glory of God and endowment of mankind.'

CORRESPONDING SOCIETIES COMMITTEE.

Report of the Committee (Mr. WILLIAM WHITAKER, *F.R.S.*, *Chairman*; Mr. WILFRED MARK WEBB, *Secretary*; Mr. P. J. ASHTON, Dr. F. A. BATHER, *F.R.S.*, Rev. J. O. BEVAN, Sir EDWARD BRABROOK, C.B., Sir H. G. FORDHAM, Mr. T. SHEPPARD, Rev. T. R. R. STEBBING, *F.R.S.*, Mr. MARK SYKES, *and the* PRESIDENT *and* GENERAL OFFICERS *of the Association. Drawn up by the Secretary, August* 1921.

THE Committee reports that the following are the officers of the Conference of Delegates to be held at Edinburgh: *President*, Sir Richard Gregory; *Vice-President and Secretary*, Mr. Wilfred Mark Webb, F.L.S.; and that the programme is as follows:—

Thursday, September 8, 1921, *at* 2 P.M.—(*a*) Presidential Address by Sir Richard Gregory on 'The Message of Science.' (*b*) Discussion on 'Science and Citizenship.' Speakers: Sir Leslie Mackenzie and Mr. Andrew Eunson.

Tuesday, September 13, 1921, *at* 2 P.M.—Discussion on 'Regional Surveys.' Speakers: Prof. Patrick Geddes and Mrs. Fraser Davies.

The meetings will be open to all members of the Association from 2 P.M. until the discussions are ended, when the Conference will be confined to delegates only.

The South African Association for the Advancement of Science and the Edinburgh Chamber of Commerce have been added to the list of Affiliated Societies, and the Selby Scientific Society and the Dumbartonshire Natural History Society to the list of Associated Societies.

The Committee asks to be reappointed, with a grant of 40*l*.

CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES.

THE MESSAGE OF SCIENCE.

ADDRESS BY

Sir RICHARD GREGORY,

PRESIDENT OF THE CONFERENCE.

It is just forty years ago, at the York Meeting in 1881, that a Committee was appointed 'to arrange for a conference of delegates from scientific societies to be held at the annual meetings of the British Association, with a view to promote the interests of the societies represented by inducing them to undertake definite systematic work on a uniform plan.' The Association had been in existence for fifty years before it thus became a bond of union between local scientific societies in order to secure united action with regard to common interests. Throughout the whole period of ninety years it has been concerned with the advancement and diffusion of natural knowledge and its applications. The addresses and papers read before the various sections have dealt with new observations and developments of scientific interest or practical value; and, as in scientific and technical societies generally, questions of professional status and emolument have rarely been discussed. The port of science—whether pure or applied—is free, and a modest yawl can find a berth in it as readily as a splendid merchantman, provided that it has a cargo to discharge. Neither the turmoil of war nor the welter of social unrest has prevented explorers of uncharted seas from crossing the bar and bringing their argosies to the quayside, where fruits and seeds, rich ores and precious stones have been piled in profusion for the creation of wealth, the comforts of life, or the purpose of death, according as they are selected and used.

All that these pioneers of science have asked for is for vessels to be chartered to enable them to make voyages of discovery to unknown lands. Many have been private adventurers, and few have shared in the riches they have brought into port. Corporations and Governments are now eager to provide ships which will bring them profitable freights, and to pay bounties to the crews, but this service is dominated by the commercial spirit which expects immediate returns for investments, and mariners who enter it are no longer free to sail in any direction they please or to enter whatever creek attracts them. The purpose is to secure something of direct profit or use, and not that of discovery alone, by which the greatest advances of science have hitherto been achieved.

When science permits itself to be controlled by the spirit of profitable application it becomes merely the galley-slave of short-sighted commerce. Almost all the investigations upon which modern industry has been built would have been put aside at the outset if the standard of immediate practical value had been applied to them. To the man of science discoveries signify extensions of the field of work, and he usually leaves their exploitation to prospectors who follow him. His motives are intellectual advancement, and not the production of something from which financial gain may be secured. For generations he

has worked in faith purely for the love of knowledge, and has enriched mankind with the fruits of his labours; but this altruistic attribute is undergoing a change. Scientific workers are beginning to ask what the community owes to them, and what use has been made of the talents entrusted to it. They have created stores of wealth beyond the dreams of avarice, and of power unlimited, and these resources have been used to convert beautiful countrysides into grimy centres of industrialism, and to construct weapons of death of such diabolical character that civilised man ought to hang his head in shame at their use.

Mankind has, indeed, proved itself unworthy of the gifts which science has placed at its disposal, with the result that squalid surroundings and squandered life are the characteristics of modern Western civilisation, instead of social conditions and ethical ideals superior to those of any other epoch. Responsibility for this does not lie with scientific discoverers, but with statesmen and democracy. Like the gifts of God, those of science can be made either a blessing or a curse, to glorify the human race or to destroy it; and upon civilised man himself rests the decision as to the course to follow. With science as an ally, and the citadels of ignorance and self as the objective, he can transform the world, but if he neglects the guidance which knowledge can give, and prefers to be led by the phrases of rhetoricians, this planet will become a place of dust and ashes.

Unsatisfactory social conditions are not a necessary consequence of the advance of science, but of incapacity to use it rightly. Whatever may be said of captains of industry or princes of commerce, scientific men themselves cannot be accused of amassing riches at the expense of labour, or of having neglected to put into force the laws of healthy social life. Power—financial and political—has been in the hands of people who know nothing of science, not even that of man himself, and it is they who should be arraigned at the bar of public justice for their failure to use for the welfare of all the scientific knowledge offered to all. Science should dissociate itself entirely from those who have thus abused its favours, and not permit the public to believe it is the emblem of all that is gross and material and destructive in modern civilisation. There was a time when intelligent working men idealised science; now they mostly regard it with distrust or are unmoved by its aims, believing it to be part of a soul-destroying economic system. The obligation is upon men of science to restore the former feeling by removing their academic robes and entering into social movements as citizens whose motives are above suspicion and whose knowledge is at the service of the community for the promotion of the greatest good. The public mind has yet to understand that science is the pituitary body of the social organism, and without it there can be no healthy growth in modern life, mentally or physically.

This Conference of Delegates provides the most appropriate platform of all those offered by the British Association from which a message of exhortation may be given. There are now 130 Corresponding Societies of the Association, with a total membership of about 52,000, and their representatives should every year go back not only strong with zeal for new knowledge, but also as ministers filled with the sense of duty to inspire others to trust in it. In mechanics work is not considered to be done until the point of application of the force is moved; and knowledge, like energy, is of no practical value unless it is dynamic. The scientific society which shuts itself up in a house where a favoured few can contemplate its intellectual riches is no better than a group of misers in its relations to the community around it. The time has come for a crusade which will plant the flag of scientific truth in a bold position

in every province of the modern world. If you believe in the cause of disciplined reason you will respond to the call and help to lift civilised man out of the morass in which he is now struggling, and set him on sound ground with his face toward the light.

It is not by discoveries alone, and the records of them in volumes rarely consulted, that science is advanced, but by the diffusion of knowledge and the direction of men's minds and actions through it. In these democratic days no one accepts as a working social ideal Aristotle's view of a small and highly cultivated aristocracy pursuing the arts and sciences in secluded groves and maintained by manual workers excluded from citizenship. Artisans to-day have quite as much leisure as members of professional classes, and science can assist in encouraging the worthy employment of it. This end can be attained by co-operative action between local scientific societies and representative organisations of labour. There should be close association and a common fellowship, and no suggestion of superior philosophers descending from the clouds to dispense gifts to plebeian assemblies. Above all, it should be remembered that a cause must have a soul as well as a body. The function of a mission-hall is different from that of a cinema-house or other place of entertainment, and manifestations of the spirit of science are more uplifting than the most instructive descriptive lectures.

Science needs champions and advocates, in addition to actual makers of new knowledge and exponents of it. There are now more workers in scientific fields than at any other time, yet relatively less is done to create enthusiasm for their labour and regard for its results than was accomplished fifty years ago. Every social or religious movement passes through like stages, from that of fervent belief to formal ritual. In science specialisation is essential for progress, but the price which has to be paid for it is loss of contact with the general body of knowledge. Concentration upon any particular subject tends to make people indifferent to the aims and work of others; for, while high magnifying powers enable minute details to be discerned, the field of vision is correspondingly narrowed, and the relation of the structure as a whole to pulsating life around it is unperceived.

As successful research is now necessarily limited for the most part to complex ideas and intricate details requiring special knowledge to comprehend them, very special aptitude is required to present it in such a way as will awaken the interest of people familiar only with the vocabulary of everyday life. In the scientific world the way to distinction is discovery, and not exposition, and rarely are the two faculties combined. Most investigators are so closely absorbed in their researches that they are indifferent as to whether people in general know anything of the results or not. In the strict sense of the word, science can never be popular, and its pure pursuit can never pay, but where one person will exercise his intelligence to understand the description of a new natural fact or principle a thousand are ready to admire the high purpose of a scientific quest and reverence the disinterested service rendered by it to humanity. The record of discovery or description of progress is, therefore, only one function of a local scientific society; beyond this is the duty of using the light of science to reveal the dangers of ignorance in high as well as in low places. Though in most societies there is only a small nucleus of working members, the others are capable of being interested in results achieved, and a few may be so stimulated by them as to become just and worthy knights of science, ready to remove any dragons which stand in the way of human progress, and continually upholding the virtues of their mistress.

Every local scientific society should be a training ground for these Sir Galahads, and an outpost of the empire of knowledge. The community should look to it for protection from dangers within and without the settlement, and for assistance in pressing further forward into the surrounding woods of obscurity. At present it is unusual for this civic responsibility to be accepted by a scientific society, with the result that local movements are undertaken without the guidance necessary to make them successful. A local scientific society should be the natural body for the civic authority to consult before any action is taken in which scientific knowledge will be of service. It should be to the city or county in which it is situated what the Royal Society is to the State, and not a thing apart from public life and affairs. As an example of what a local society may usefully do, the action taken by the Manchester Field Naturalists' and Archæologists' Society several years ago may be mentioned. The Society appointed a Committee for the purpose of promoting the planting of trees and shrubs in Manchester and its immediate suburbs. The idea commended itself to the Corporation, and the Committee obtained advice as to the best trees for open spaces in the district, shrubs for tubs and boxes, and tree culture in towns generally. This is the kind of guidance which a scientific society should be particularly competent to give, and which the community has a right to expect from it. Many similar questions continually arise in which ascertained knowledge can be used for the promotion of healthy individual and social life, and if scientific societies are indifferent to them they neglect their best opportunities of playing a strong part in the scheme of human progress.

When wisdom is justified of her children, and local scientific societies are no longer esoteric circles, but effective groups of enlightened citizens of all classes, they will provide the touchstone by which fact is distinguished from assertion and promise from performance. As the sun draws into our system all substantial bodies which come within its sphere of influence, while the pressure of sunlight drives away the fine dust which would tend to obscure one body from another, so a local scientific society possesses the power of attracting within itself all people of weight in the region around it and of dispersing the mist and fog which commonly prevail in the social atmosphere. Thus may the forces of modern civilisation, moral and material, be brought together, and an allied plan of campaign instituted against the armies of ignorance and sloth. The service is that of truth, the discipline that of scientific investigation, and the unifying aim human well-being. Kingsley long ago expressed the democratic basis upon which this fellowship is founded. 'If,' he said, 'you want a ground of brotherhood with men, not merely in these islands, but in America, on the Continent—in a word, all over the world—such as rank, wealth, fashion, or other artificial arrangements of the world cannot give and cannot take away; if you want to feel yourself as good as any man in theory, because you are as good as any man in practice, except those who are better than you in the same line, which is open to any and every man, if you wish to have the inspiring and ennobling feeling of being a brother in a great freemasonry which owns no difference of rank, of creed, or of nationality—the only freemasonry, the only International League which is likely to make mankind (as we all hope they will be some day) one—then become men of science. Join the freemasonry in which Hugh Miller, the poor Cromarty stonemason, in which Michael Faraday, the poor bookbinder's boy, became the companions and friends of the noblest and most learned on earth, looked up to by them not as equals merely, but as teachers and guides, because philosophers and discoverers.'

When Kingsley delivered this message artisans were crowding in thousands to lectures in Manchester and other populous places by leaders in the scientific world of that time. Labour then welcomed science as its ally in the struggle for civil rights and spiritual liberty. That battle has been fought and won, and subjects in bitter dispute fifty years ago now repose in the limbo of forgotten things. There is no longer a conflict between religion and science, and labour can assert its claims in the market-place or council house without fear of repression. Science is likewise free to pursue its own researches and apply its own principles and methods within the realm of observable phenomena, and it does not desire to usurp the functions of faith in sacred dogmas to be perpetually retained and infallibly declared. The Royal Society of London was founded for the extension of *natural* knowledge in contra-distinction to the *supernatural*, and it is content to leave priests and philosophers to describe the world beyond the domain of observation and experiment. When, however, phenomena belonging to the natural world are made subjects of supernatural revelation or uncritical inquiry, science has the right to present an attitude of suspicion towards them. Its only interest in mysteries is to discover the natural meaning of them. It does not need messages from the spirit world to acquire a few elementary facts relating to the stellar universe, and it must ask for resistless evidence before observations contrary to all natural law are accepted as scientific truth. If there are circumstances in which matter may be divested of the property of mass, fairies may be photographed, lucky charms may determine physical events, magnetic people disturb compass needles, and so on, by all means let them be investigated, but the burden of proof is upon those who believe in them and every witness will be challenged at the bar of scientific opinion.

We do not want to go back to the days when absolute credulity was inculcated as a virtue and doubt punished as a crime. It is easy to find in works of uncritical observers of mediæval times most circumstantial accounts of all kinds of astonishing manifestations, but we are not compelled to accept the records as scientifically accurate and to provide natural explanations of them. We need not doubt the sincerity of the observer even when we decline to accept his testimony as scientific truth. The maxim that "Seeing is believing" may be sound enough doctrine for the majority of people, but it is insufficient as a principle of scientific inquiry. For thousands of years it led men to believe that the earth was the centre of the universe, with the sun and other celestial bodies circling round it, and controlling the destiny of man, yet what seemed obvious was shown by Copernicus to be untrue. This was the beginning of the liberation of human life and intellect from the maze of puerile description and philosophic conception. Careful observation and crucial experiment later took the place of personal assertion and showed that events in Nature are determined by permanent law and are not subject to haphazard changes by supernatural agencies. When this position was gained by science, belief in astrology, necromancy, and sorcery of every kind began to decline, and men learned that they were masters of their own destinies. The late War is responsible for a recrudescence of these mediæval superstitions, but if natural science is true to the principles by which it has advanced it will continue to bring to bear upon them the piercing light by which civilised man was freed from their baleful consequences.

There is abundant need for the use of the intellectual enlightenment which science can supply to counteract the ever-present tendency of humanity to revert to primitive ideas. Fifty years of compulsory education are but a moment in the history of man's development, and their influence is as nothing

in comparison with instincts derived from our early ancestors and traditions of more recent times grafted upon them. So little is known of science that to most people old women's tales or the single testimony of a casual onlooker are as credible as the statements and conclusions of the most careful observers. Where exact knowledge exists, however, to place opinion by the side of fact is to blow a bubble into a flame. Within its own domain science is concerned not with belief—except as a subject of inquiry—but with evidence. It claims the right to test all things in order to be able to hold fast to that which is good. It declines to accept popular beliefs as to thunderbolts; living frogs and toads embedded in blocks of coal or other hard rock without an opening, though the rock was formed millions of years ago and all fossils found in it are crushed as flat as paper; the inheritance of microbic diseases; the production of rain by explosions when the air is far removed from its saturation point; the influence of the moon on the weather or of underground water upon a twig held by a dowser, and dozens of like fallacies, solely because when weighed in the balance they have been found wanting in scientific truth. Its only interest in mysteries is that of inquiring into them and finding a natural reason for them. Mystery is thus not destroyed by knowledge but removed to a higher plane.

Never let it be acknowledged that science destroys imagination, for the reverse is the truth. 'The Gods are dead,' said W. E. Henley.

> 'The world, a world of prose,
> Full-crammed with facts, in science swathed and sheeted,
> Nods in a stertorous after-dinner doze!
> Plangent and sad, in every wind that blows
> Who will may hear the sorry words repeated :—
> "The Gods are dead."'

It is true that the old idols of wood and stone are gone, but far nobler conceptions have taken their place. The universe no longer consists of a few thousand lamps lit nightly by angel torches, but of millions of suns moving in the infinite azure, into which the mind of man is continually penetrating further. Astronomy shows that realms of celestial light exist where darkness was supposed to prevail, while scientific imagination enables obscure stars to be found which can never be brought within the sense of human vision, the invisible lattice work of crystals to be discerned, and the movements of constituent particles of atoms to be determined as accurately as those of planets around the sun. The greatest advances of science are made by the disciplined use of imagination; but in this field the picture conceived is always presented to Nature for approval or rejection, and her decision upon it is final. In contemporary art, literature, and drama imagination may be dead, but not in science, which can provide hundreds of arresting ideas awaiting beautiful expression by pen and pencil. It has been said that the purpose of poetry is not truth, but pleasure; yet, even if this definition be accepted, we submit that insight into the mysteries of Nature should exalt, rather than repress, the poetic spirit, and be used to enrich verse, as it was by some of the world's greatest poets—Lucretius, Dante, Milton, Goethe, Tennyson, and Browning. With one or two brilliant exceptions, popular writers of the present day are completely oblivious to the knowledge gained by scientific study, and unmoved by the message which science is alone able to give. Unbounded riches have been placed before them, yet they continue to rake the muck-heap of animal passions for themes of composition. Not by their works shall we become 'children of light,' but by the indomitable spirit of man ever straining upwards to reach the stars.

Where there is ignorance of natural laws all physical phenomena are referred to supernatural causes. Disease is accepted as Divine punishment to be met by prayer and fasting, or the act of a secret enemy in communion with evil spirits. Because of these beliefs thousands of innocent people were formerly burnt and tortured as witches and sorcerers, while many thousands more paid in devastating pestilences the penalty which Nature inevitably exacts for crimes against her. In one sense it may be said that the human race gets the diseases it deserves; but the sins are those of ignorance and neglect of physical laws rather than against spiritual ordinances. Plague is not now explained by supposed iniquities of the Jews or conjunctions of particular planets, but by the presence of an organism conveyed by fleas from rats; malaria and yellow fever are conquered by destroying the breeding places of mosquitoes; typhus fever by getting rid of lice; typhoid by cleanliness; tuberculosis by improved housing; and most like diseases by following the teachings of science concerning them. Though the mind does undoubtedly influence the resistance of the body to invasion by microbes, it cannot create the specific organism of any disease, and the responsibility of showing how to keep such germs under control, and prevent, therefore, the poverty and distress due to them, is a scientific rather than a spiritual duty.

The methods of science are pursued whenever observations are made critically, recorded faithfully, and tested rigidly, with the object of using conclusions based upon them as stepping-stones to further progress. They demand an impartial attitude towards evidence and fearless judgment upon it. These are the principles by which the foundations of science have been laid, and a noble structure of natural knowledge erected upon them. A scientific inquiry is understood to be one undertaken solely with the view of arriving at the truth, and this disinterested motive will always command public confidence. It is poles apart from the spirit in which social and political subjects are discussed : it is the rock against which waves of emotion and storms of rhetoric lash themselves in vain. If political science were guided by the same methods it would present an open mind to all sides of a question, weighing objections to proposals as justly as reasons in support of them, whereas usually it sees only the views of a particular class or party, and cannot be trusted, therefore, to strike a judicial balance. The methods of science should be the methods applied to social problems if sound principles of progress are to be determined. When they are so used a statesman will be judged, as a scientific man is judged, by correct observation, just inference, and verified prediction; in their absence politics will remain stranded on the shifting sands of barter, concession, and expediency.

Democracy may be politically an irrational force, but that is all the more reason why those who direct it should have full knowledge of the possibilities offered by science for construction as well as for destruction. In a chemical research an experiment is not the haphazard mixture of substances made in the hope that something good will come from it, but the deliberate test of consequences which ought to follow if certain ideas are true. So with all scientific experiment : reason is the source of action, and principles are tested by results. Social problems are perhaps more complicated than those of the laboratory, yet the only way to discover solutions of them is to apply scientific standards to the methods used and results obtained. Laws of Nature are merely expressions of our knowledge at a particular epoch, and they are more precise than those of political economy because they are investigated purely from the point of view of progress. If the general laws which constitute the science of sociology are to be discovered and accepted, their study must be

as impartial as that of any other science. 'The discovery of exact laws,' said W. K. Clifford, 'has only one purpose—the guidance of conduct by means of them. The laws of political economy are as rigid as those of gravitation; wealth distributes itself as surely as water finds its level. But the use we have to make of the laws of gravitation is not to sit down and cry "Kismet" to the flowing stream, but to construct irrigation works.'

Organised Labour has on more than one occasion pronounced a benison upon scientific research, and urged that full facilities should be afforded to those who undertake it. Not long ago the American Federation of Labour in Convention assembled resolved 'that a broad programme of scientific and technical research is of major importance to the national welfare,' and in a noteworthy document insisted upon its essential value in the development of industries, increased production, and the general welfare of the workers. The British Labour Party has also stated that it places the 'advancement of science in the forefront of its political programme,' but its manifesto refers particularly to the 'undeveloped science of society' rather than to the science of material things; and whatever Labour may declare officially, it is scarcely too much to say that artisans in general show less active interest in scientific knowledge now than they did fifty years ago. Not by the study of science does a manual worker become a leader among his fellows, but by the discovery of wrongs to be remedied or rights to be established, and by fertility of resource in disputations concerning them. This is natural enough, yet when we remember that many of the greatest pioneers in the fields of pure and applied science were of humble origin it is surprising that Labour makes no effort to keep men of this type within its lodges.

If Trades Unions were true to their title, and not merely wage unions, their members would give as much attention to papers on scientific principles of their industry, craftsmanship, and possible new developments as they do to the consideration of the uttermost they can claim and secure for their members. Not a single labour organisation concerns itself with actual means of industrial progress, but only with the sharing of the profits from processes or machinery devised by others. Labour may express approval of scientific and technical research, but if it wishes to be a creative force it should take part in this work instead of limiting itself to getting the greatest possible advantage from the results. Under present conditions an artisan with original ideas or inventive genius has to go outside the circle of his union to describe his work, and he thus becomes separated from his fellows through no fault of his own. His contributions are judged by a scientific or technical society purely on their merit and without any consideration as to his social position. Labour can never be great until it affords like opportunities to its own original men by accepting and issuing papers upon discoveries of value to science and industry. When it does this, and its publications occupy an honoured place among those of scientific and technical societies, it will be able to command a position in national polity which can never be justly conceded to any organisation concerned solely with the rights and privileges of a single class in the community.

We know, of course, that few workmen can be expected to possess sufficient knowledge and originality to make developments important enough to be recorded in papers for the benefit of science or industry generally, but every such contribution published by a Trade Union or other Labour organisation, federated or otherwise, would do far more to command respect than sheaves of pamphlets upon economic aspects of industry from the point of view of workpeople. If no fundamental or suggestive papers of this kind are forthcoming, or if organised Labour persists in its policy of letting its men of

practical genius find elsewhere the people who know how to appreciate them, it is tacitly acknowledged that others are expected to provide the seeds of industrial developments while Labour concerns itself solely with the distribution of the fruits derived from them.

It is true that some of the leaders of the Labour movement realise that close association with progressive science is essential to the expansion of industry and the consequent provision of wages in the future. What is here urged is that Labour should itself take part in the scientific and industrial research which it acknowledges is necessary for existence, and should show by its own contributions that it possesses the power to produce useful knowledge as well as the dexterity to apply it. The machinery of trade unionism is capable of much more extensive use than that to which it has hitherto been put, and when it is concerned not only with securing 'for the producers by hand or by brain the full fruits of their industry,' but also with the creation of new plantations by its own efforts, no one will be able to doubt its fitness to exercise a controlling influence upon modern industry.

The Workers' Educational Association has proved that very many artisans are ready to take advantage of opportunities of becoming familiar with the noblest works of literature, science, and art, with the single motive of enriching their outlook upon life. Many more attend classes in economics, and nearly all are in favour of extended facilities for further education, though there is a difference of intention between the Marxian element in Labour and the more impartial supporters of the W.E.A. or of the Co-operative Education Union. 'There is practically no limit,' says Mr. G. D. H. Cole in 'An Introduction to Trade Unionism,' 'to what could be done if there only existed among the national and local leaders of Labour a clear idea of the part which education must play if the working-class is ever to achieve emancipation from the wage system.' To education should be added original research if labour is to signify something more than a class of hewers of wood and drawers of water. The Guild movement represents a step in this direction, but if it signifies merely a return to the mediæval system it can scarcely be so important a factor of general development as its advocates imagine, and it may mean the institution of caste in labour. Such a system no doubt leads to perfection of craftsmanship, and it is to be welcomed as an antidote to the deadening influence of specialised industry; but a caste nation at last becomes stationary, for in each caste a habit of action and a type of mind are established which can only be changed with difficulty. What is wanted to make the race strong is cross-fertilisation, and not in-breeding.

Local scientific societies should provide a common forum where workers with hand or brain can meet to consider new ideas and discuss judicially the significance of scientific discovery or applied device in relation to human progress. At present such societies are mostly out of touch with these practical aspects of knowledge, and are more interested in prehistoric pottery than in the living world around them. Most of those connected to the British Association are concerned with natural history, but all scientific societies in a district should form a federation to proclaim the message of knowledge from the house-tops. Men are ready to listen to the gospel of science and to believe in its power and its guidance, but its disciples disregard the appeal and are content to let others minister to the throbbing human heart. Civilisation awaits the lead which science can give in the name of wisdom and truth and unprejudiced inquiry into all things visible and invisible, but the missionary spirit which would make men eager to declare this noble message to the world has yet to be created.

This is as true of the British Association itself as it is of local scientific societies. It seems to be forgotten that one of the functions of the Association is to inspire belief and confidence in science as the chief formative factor of modern life, and not only to display discoveries or enable specialists to discuss technical advances in segregated sections. Though members of the Association may be able to live on scientific bread alone, most of the community in any place of meeting need something more spiritual to awaken in them the admiration and belief which beget confidence and hope. They ask for a trumpet-call which will unite the forces of natural and social science, and are unmoved by the parade of trophies of scientific conquests displayed to them. It was the primary purpose of Canon W. V. Harcourt, the chief founder of this Association, and General Secretary from 1831 to 1837, to sound this note for 'the stimulation of interest in science at the various places of meeting, and through it the provision of funds for carrying on research,' and not for 'the discussion of scientific subjects in the sections.' In the course of time these sectional discussions have taken a prominent place in the Association's programme, and rightly so, for they have promoted the advancement of science in many directions; but, while we recognise their value to scientific workers, we plead for something more for the great mass of people outside the section-rooms, for a statement of ideals and of service, of the strength of knowledge and of responsibility for its use. These are the subjects which will quicken the pulse of the community and convert those who hate and fear science and associate it solely with debasing aspects of modern civilisation into fervent disciples of a new social faith upon which a lever made in the workshops of natural knowledge may be placed to move the world.

REPORT OF THE CONFERENCE.

At the first meeting of the Conference of Delegates, at Edinburgh, on Thursday, September 8, the President, Sir Richard Gregory, delivered the preceding address on 'The Message of Science.' A discussion followed on 'Science and Citizenship,' opened by Sir Leslie Mackenzie, who emphasised the fact that the Scientific Societies created volume upon volume of Transactions, some of them incapable of explanation except to the expert, some of them, however, full of fascination to the whole world and crying aloud to be told in the voice of the skilled speaker and in the phrase of the skilled writer. Masses of scientific knowledge were lost to the world, and sometimes even to the scientific world, because its writers could not write.

Mr. Andrew Eunson said that Trade Unions need not be expected to make contributions to Science outside their own particular occupation. If their members had scientific leanings or literary tastes they should join the appropriate society for their cultivation. Trade Unionists, as such, were not in the position to put into operation their ideas, but employers from information given them by their employees could do so.

At the request of the Glasgow Natural History Society and the Rochdale Literary and Scientific Society the following resolution was passed for presentation to the Committee of Recommendations and for circulation among the Sections :—

> 'That the Council be asked to represent to the Postmaster-General the very heavy burden which the postage of their publications and notices entails upon the Scientific Societies, and to request him to alleviate it at the earliest possible moment.'

A further resolution was also passed, namely :—

> 'That the Council be asked to consider what action should be taken to reduce the cost of publications of Scientific Societies.'

The Committee of Recommendations did not advise any action upon this resolution.

A discussion on 'Regional Surveys' was arranged for the second meeting of the Conference on Tuesday, September 13, 1921.

Professor GEDDES, who spoke first, described regional survey. It was, he said, an inquiry into everything around them, an outlook near and far, a prying into every detail of nature and of human activities as well. It reached out through education into action. It was geography and all its sub-sciences of geology, meteorology, and other physical studies. It was biology, too, the study of the plant and animals, and biology in relation to man. It was anthropology and archæology, and thus psychology also. It was economics, the key of economics, for it observed all forms of human effort from the earliest occupations to the most complex of modern ones. They were out for the study of life itself. Their chief aim was civic development; their object was to make the world something better than it was before.

Mr. HAROLD PEAKE said that the first thing to be done was to define the district to be surveyed, and he advocated the maximum of maps and illustrations with the minimum of text. He also gave important examples of how one survey might throw considerable light upon another.

Mrs. FRASER DAVIES exhibited and described specimens of regional surveys. One of the problems she said was to secure co-operation between the teachers in the schools and the scientific men outside.

Professor H. J. FLEURE spoke of the need of the British Association, if it were to maintain its place for the advancement of science, to provide for the synthetic point of view, and he emphasised the importance of the Conference of Delegates for bringing together specialisms lest they diverged unduly and fell apart into things that were almost ridiculous.

Mr. ALEXANDER FARQUHARSON pointed out the connection of the regional survey with the general movement for an increase in the knowledge and feeling in citizenship.

Sir FRANCIS OGILVIE said that the essential point in regional survey was that there should be room for diversity of attitude, subject, study and investigation and mode of record. It would be of the utmost value if a selection could be made of the methods of representing the various interests that cropped up in regional surveys, and these were given as much publicity as possible.

Other points of view, including the medical aspect, were also touched upon, and the following resolution was passed :—

> 'That this meeting of Delegates of the Corresponding Societies of the British Association approves the movement of regional survey, of which examples have been exhibited and explained; and towards the promotion and initiation of such surveys they invite a further exhibition at next year's meeting (at Hull) with discussion towards methods of presentment, &c.
>
> 'It also desires to aid co-operation among Scientific Societies, educational institutions, public libraries, museums, &c., with civic societies and municipalities, or otherwise, towards the preparation of surveys, and their due preservation and exhibition, for educational, general, and municipal purposes.
>
> 'Note as to particulars : The following have undertaken to give information : Mrs. Fraser Davies, Le Play House, 65 Belgrave Road, S.W. 1; Professor H. J. Fleure, University College of Wales, Aberystwyth; Professor P. Geddes, Department of Sociology and Civics, University of Bombay; and Miss Ritchie, Outlook Tower, Edinburgh.'

The Vice-President then took the chair, and a brief discussion followed as to the advisability of affiliated and associated societies paying a subscription to the British Association, the matter being referred to the Corresponding Societies Committee.

Professor Myres (General Secretary of the Association) then explained the various alternatives that had been suggested with regard to the holding of the Conference of Delegates in the year 1924, in the event of the Association meeting, as proposed, in Canada.

LIST OF PAPERS

BEARING UPON THE ZOOLOGY, BOTANY, AND PREHISTORIC ARCHÆOLOGY OF THE BRITISH ISLES, ISSUED DURING 1920.

By T. Sheppard, *M.Sc., F.G.S., The Museum, Hull.*

As a result of the request which was made last year for suggestions for the improvement of the style of this Bibliography, two important changes, and a number of unimportant ones, occur in the list now published.

The first is that the papers are divided into three sections, viz. : Zoology, Botany, and Prehistoric Archæology, each in alphabetical order, instead of all being in one alphabetical list with letters of identification at the front of each title. In the present form I believe the list will be more useful to students. The second change is that the surnames of the authors appear first, followed by initials or Christian names, in this way being more handy for reference.

Next year it is proposed to publish a complete list of the various publications which have been consulted in connection with this Bibliography, and if in the meantime any omissions from the list now published are noticed I should be glad to know.

It is gratifying to find that many of the suggestions made in the introductory remarks last year (British Association Report for 1919, pp. 406-408) have been adopted, a number of the Journals having fallen in with the suggestion as regards references, and also as regards giving suitable titles to the papers printed; in this way the following list has been considerably simplified.

With reference to the various "anonymous" articles, with these have been included notes signed by pen-names or initials where the name of the actual author is not obvious. Where the authors are clearly known, however, the articles have been put in their places under the names of the authors.

In these various anonymous articles, as in the signed articles, my object has been to keep the contents of each publication together, and in datal order.

Zoology.

Anon. An Unusual Visitor [seal] at Newport. *Animal World.* Jan., p. 8.

—— Notes. [Ornithological] *Bird Notes and News.* Vol. IX., No. 3, pp. 21-22.

—— Economic Ornithology, *tom. cit.*, p. 23, Jan., p. 29. *Winter*, p. 29.

—— Fisheries in the Great War, being the Report on Sea Fisheries for the years 1915, 1916, 1917, and 1918 of the Board of Agriculture and Fisheries, parts i. and ii., pp. 1-194.

—— Report of General Summer Excursions, 1918. *Proc. Bournemouth Nat. Sci. Soc.* Vol. X., p. 49.

—— Account of the Annual and General Meetings. *Ann. Rep. and Proc. Bristol Nat. Soc.* Vol. V., pt. ii., pp. 67-70.

—— Notes. *Brit. Birds.* Jan., p. 221; Feb., pp. 246-247; Mar., p. 276; Apr., pp. 298-300; May, p. 316; June, pp. 20-22; July, pp. 45-47; Aug., pp. 66-67; Sept., p. 96; Oct., p. 120; Nov., p. 143; Dec., pp. 165-167.

—— Frederick Webb Headly [Obituary], *tom. cit.* Feb., pp. 235-246.

—— Recovery of Marked Birds, *tom. cit.* Nov., pp. 129-131.

—— Summary Guide to the Exhibition Galleries of the British Museum (Natural History), 16 pp.

—— Wild Geese. *Country Life.* Jan. 3, pp. 33-34.

—— Weasel as Mole Hunter, *tom. cit.* Jan. 10, p. 60.

—— Wild Life under Water, *tom. cit.* Jan. 10, pp. 44-45.

—— Domestic Fox, *tom. cit.* Jan. 17, p. 92.

—— Curious Catch ['lump-fish' at Clacton-on-Sea], *tom. cit.* Jan. 24. p. 122.

—— Grey Squirrel, *tom. cit.* Feb. 7, p. lviii.

—— Plea for Crayfish Farms in England, *tom. cit.* February 28, p. 286.

ANON. Piping Hoverer-Flies, *tom. cit.* Mar. 6, p. 321.
—— Gull Ponds in the British Isles, *tom. cit.* Mar. 13, p. 331.
—— Wheatears on Chesil Beach, *tom. cit.* Mar. 20, p. 392.
—— Birds' Food during Snow, *loc. cit.*
—— How Sea Snails Burrow, *tom. cit.* Mar. 27, p. xciv.
—— Bird Protection and the Police, *tom. cit.* Mar. 27, p. 419.
—— Pheasants in Peace Time, *tom. cit.* Apr. 3, p. 463.
—— Hunting Tactics of Magpies, *tom. cit.* Apr. 3, p. 464.
—— Earthworms in Flooded Fields, *loc. cit.*
—— A New Riddle of the Sands. Seals in Norfolk, *tom. cit.* Apr. 10 pp. 499-500.
—— Wagtails Flocking, *tom. cit.* Apr. 24, p. 563.
—— Albinism in Blackbirds, *tom. cit.* Apr. 24, p. 572.
—— Birds in the English Channel, *loc. cit.*
—— Starling in the Woodpeckers' Nest, *tom. cit.* May 1, pp. 582-584.
—— Nesting Habits of the Missel-Thrush, *tom. cit.* May 1, p. 607.
—— Albinism in Blackbirds, *tom. cit.* May 8, p. 635.
—— Bees Swarming Early, *tom. cit.* May 15, p. 671.
—— Wagtails Flocking, *tom. cit.* May 22, p. 706.
—— How a Pheasant Hides, *tom. cit.* May 22, p. 711.
—— Distribution of Birds, *tom. cit.* May 22, pp. 711-712.
—— Bees Swarming Early, *tom. cit.* May 29, p. 739.
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